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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Trace gas transport in the 1999/2000 Arctic winter: comparison of nudged GCM runs with observations" *by* M. K. van Aalst et al.

M. K. van Aalst et al.

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We would like to thank both referees for examining our paper, and for their good comments and suggestions. All their points are addressed below, with reference to the changes we have made in the revised version of our paper.

D. Shindell, referee #1

In the first paragraph of this comment, Dr Shindell argues out that our results are valid mainly because the comparisons are made throughout the course of a winter, but that they would not necessarily be meaningful for an individual day because of a model time lag of up to a few days due to the nudging. While it is true (as pointed out in the paper) that the nudging procedure requires a spinup of a few days, this does not necessarily imply a similar time lag of the model as compared to the actual meteorology (i.e. the ECMWF analysis) throughout the run. Once the spinup is completed, the model's meteorology generally mimics the actual one, and the model only needs minor adjustments from the nudging to retain this coherence. Hence, we have found that comparisons on individual days do seem meaningful.

Specific comments:

Section 3.1, line 21: The model indeed has a hybrid-pressure vertical coordinate; we have added this useful clarification.

Section 4, line 11: Our prescription of tropospheric methane included a rough 2% sine function (not 0.02 ppm, as originally specified). Hence, the interhemispheric gradient was 4% (the amplitude of the sine) times 1.76 ppm, about 70 ppb. While closer to the 100 ppb mentioned by the referee, this is indeed still a low estimate. We have checked whether a higher gradient would affect the results, and it did not in any way. In order not to confuse the reader, we have removed the reference to the gradient.

Section 7, line 15: We have changed the wording to remove the (unintended) suggestion that TM5's first-order slopes advection scheme would be more able than our semi-lagrangian one to reproduce gradients across the vortex edge. The key point (which should now be clearer) is that Van den Broek et al.'s results did not improve with higher horizontal resolution (which would be expected if high horizontal numerical diffusion would be the key problem).

Section 8, line 5: We have included the suggestion that both an underestimate of mixing and an overestimate of subsidence may lead to an overestimate of temperatures within the Arctic vortex in the Discussion (at the end of the first paragraph).

Technical comments:

We have followed all of the referee's suggestions.

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Anonynous Referee #2

The key issue raised by the Referee #2 is that we may be underestimating the influence of the nudging on the final vertical velocities. In particular, the inconsistencies between the physics in ECMWF and ECHAM could be causing vertical velocities that are slightly off.

The referee suggests that a more detailed analysis of the tendencies in the polar vortex would adddress this issue. We have reexamined our data, and, as we noted in our paper, the nudging tendencies are generally much smaller than the model tendencies, inside and outside the polar vortex (without consistent regional patterns). However, this analysis restricts us to the nudged variables: surface pressure, temperature, vorticity and divergence (the nudging takes place in spectral space - for a complete basic model description we refer to Roeckner et al., 1996). As hinted at by the referee, the nudging affects the vertical velocity only indirectly. Hence, it is difficult to deduce immediate effects of the nudging on the vertical velocity, as we cannot separate the effect of the nudging from other model tendencies. Consequently, it is difficult to provide direct insights into the effect of the nudging on the vertical transport.

The referee also suggests (in his final paragraph) that a solution might be to make a comparison between the model's performance in a nudged and non-nudged version. The problem is that the weather in a non-nudged simulation would immediately start to diverge from that in the nudged one (and thus also from the actual meteorology that winter). It then becomes impossible to compare results for one winter, and also to use instantaneous observations to decide which of the two simulations would be performing better. The only way to perform such an assessment would be to do a simulation with a nudged and a non-nudged model for a much longer period and then compare (climatic) averages. Such an experiment was beyond the scope of this paper.

As an additional way of comparing the performance of the nudged and the non-nudged MA-ECHAM4, the referee suggests a comparison with results from the other studies

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with MA-ECHAM4 that are mentioned in the paper. Unfortunately, none of these studies is sufficiently similar to provide clear insights into the quality of the transport in the nudged versus the non-nudged model versions. The SF6 study by Manzini and Feichter (1999) looked at large-scale transport in a 15-year integration, and generally found good results, also for the age of air. They did find descrepancies inside the polar vortex: the age of air at high latitudes in northern winter was about a factor two lower than the age of air derived from observations inside the vortex, which is consistent with the problems encountered in the current paper. However, Manzini and Feichter point out that the character of their analysis (monthly means at a fixed latitude) may not allow for proper representations of the transport inside the polar vortex, and that their calculations lack the mesospheric sink for SF6, which would have produced a higher age of air. Steil et al. (1998, 2003) and Manzini et al. (2003) employed a version of MA-ECHAM4 with coupled chemistry, and with a different advection scheme, so it is also difficult to draw conclusions from comparisons with those results.

All in all, we agree with the referee that we can not exclude the possibility that the nudging itself is affecting our results inside the vortex (on a medium-term timescale the nudging works fine in getting the vortex at the right place, but could potentially be affecting the descent rate over the course of the winter). We have added this note of caution to the Discussion and the Conclusion, and have also added a discussion along the lines of the previous paragraphs, highlighting the lack of insight in the effect of the nudging tendencies on the vertical velocity, and adding the suggestion to compare nudged and non-nudged runs. We have also added a few words of caution about results inside the vortex to the Abstract.

At the same time, we do feel that the basic conclusions about the nudging (the ability to reproduce actual meteorology to intercompare results from a GCM with instantaneous observations, as well as the good correlations outside the vortex) remain valid. We also wish to point out that the nudging is not the only suspect for the problems in the tracer descent rates. As highlighted in the Discussion, Van den Broek et al. (2003) found very similar results using a CTM (not relying on nudging but directly using ECMWF winddata). Hence, other potential culprits include the original ECMWF data (which, as the referee rightly points out, might have their own assimilation problems), the vertical resolution and coordinate system, and the advection scheme.

Finally, we agree with the referee about our remark in line 15 of the Discussion, and have changed the Discussion accordingly.

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