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Atmospheric Chemistry and Physics Discussions

Interactive comment on “Implications of Lagrangian transport for coupled chemistry-climate simulations” *by* A. Stenke et al.

Anonymous Referee #3

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1 Overview

To my knowledge, this paper, for first time, describes a Lagrangian advection scheme used in a CCM simulation. The focus here is on the lower stratosphere and simulations of ozone and in particular polar ozone. The paper clearly presents the deficiencies in previous model simulations and also addresses remaining problems with the improved model E39/C-A.

It is important that the CCMs which are being used for the prediction of the future of the ozone layer are thoroughly tested and that the results of these tests (showing both strength and weaknesses of the models) are discussed in the scientific literature. This paper is an important contribution to this effort.

I have several suggestions (listed below) for further improvement of the manuscript, not all of them might be technically feasible. Possible an electronic appendix is a good compromise to report more model details without overly increasing the length of the paper. After a revision taking the comments into account, I suggest to accept the paper for publication.

2 General Comments

This paper presents the effect of the improved transport scheme Attila on the stratospheric simulation with the E39/C model. I agree with the conclusion of the paper that the use of Attila leads to a better model performance in several ways, but I believe that this message could be communicated better than in the present form of the paper

An issue only addressed in passing in the present manuscript is the impact of the findings on other, comparable models (CCMs). Is it likely that many of these models use transport schemes that are similarly diffusive as the semi-Lagrangian scheme used here? If yes, then the strategy for a better model performance should be focused on the transport scheme rather than on (for example) improving the spatial resolution (which has of course also some effect on the transport scheme). I note that E39/C-A is used here with a rather limited spatial resolution (T30).

I agree that the semi-Lagrangian scheme is likely highly diffusive. But it would be good to give a citation for this statement. And of course it would be important to know how bad the scheme actually is. Is it as diffusive as a simple up-stream scheme? Further, it would be good know, to what extent the results presented here for the impact of the semi-Lagrangian scheme on E39/C carry over to other models. In particular those that use the same or a similarly diffusive Eulerian transport scheme.

It is stated that the Lagrangian transport scheme Attila is “strictly mass conserving”. As far as I understand the scheme this is correct insofar as the mass that the particle

ACPD

8, S9745–S9755, 2008

Interactive Comment

is representing does not change by definition (only its size). But doesn't that mean that the 'Lagrangian mass distribution' decouples from the mass distribution of the underlying Eulerian grid? To make my point clear through exaggeration: if an Eulerian grid box (as it occasionally happens) contained no Lagrangian/Attila points, would that mean that the Eulerian grid box has mass zero? Of course not. But a re-mapping on the Eulerian grid is necessary to couple the chemical fields with the radiation and cloud modules (etc.) of the underlying Eulerian CCM. This issue should be discussed in more detail.

I do not understand why CFCs are not transported by means of the Attila scheme. Why are the CFCs deprived of their right of being transported by the best scheme available in the model (i.e., Attila)? The CFCs could be seen as even more important for the simulations presented here than CH_4 and N_2O (which *are* transported by Attila) as they are the source of Cly in the stratosphere. Has the distribution of CFCs in the model an impact on the chemistry (i.e., does it contribute to Cly production in the model)? If the CFC fields are externally prescribed, then the CFC and the Cly fields in the model are not completely consistent; is this a problem for the model chemistry? And how well is the Cly distribution described in the mid-latitudes and tropics in the stratosphere in EC39/C? I think there should be a more detailed discussion of the CFC/Cly chemistry and transport in the model (see also details below).

3 Detailed comments

Abstract: In the abstract it is stated twice (l. 21, l. 25/26) that Cly trends in the new model version are realistic. They are much improved compared to previous simulations indeed, but I think more work is needed before they are really realistic (which is, of course, a formidable task). L. 24: This sentence describes the positive aspects of the ozone simulation, I would suggest to mention also the aspects of the ozone simulations

ACPD
8, S9745–S9755, 2008

Interactive Comment

where the model shows discrepancies to observations.

p. 18729, l. 14: “more or less agreement. . . ”: I do not agree with this statement. First, I think the question whether models agree among each other (possibly all showing the same problem, as for example the ‘cold bias’) needs to be separated from the question of how well models compare with observations. Second, there are not too many studies, which are devoted to a detailed comparison of CCM results with observations (the present paper is an example for a paper where such comparisons are being made). But if comparisons are being made of CCM results with observations, the agreement is often found to be poor. Model estimates of the average daily ozone mass deficit in the Antarctic in the late 1990s (WMO, 2007) rage between substantial overestimation to grossly underestimated values (about one fifth of the observed value). (Sankey and Shepherd, 2003) report that in the CMAM model there is virtually no transport barrier at the edge of the Arctic vortex, Lemmen et al. (2006) report a severe underestimate of the chemical ozone loss in E39/C in the Antarctic, and Tilmes et al. (2007) found that ozone loss in WACCM is underestimated substantially at the edge of the Antarctic vortex whereas the ozone loss is reasonably reproduced in the core of the vortex; the chemical ozone loss in the Arctic vortex, however, is rather poorly reproduced in WACCM. Finally, is was recently pointed out that using certain simple diagnostics of polar ozone loss like minimum ozone column can be rather misleading (Müller et al., 2008).

Of course, models for which papers exist analysing deficiencies through a detailed comparison with observations should not be criticised for revealing deficiencies and be considered inferior to models for which such studies do not exist.

p. 18730, l. 8, 9: A bit more discussion would be helpful here. The problems with methane and Cly distributions have different causes I believe. And the problem of polar tracer distributions and mid-latitude distributions should be separated; e.g. is it known how well the Cly distribution in the mid-latitudes in the stratosphere is simulated in EC39/C?

ACPD

8, S9745–S9755, 2008

p. 18730, l. 21: The semi-Lagrangian is likely highly diffusive. But it would be good to give a citation for this statement. Probably it is not as diffusive as a simple up-stream scheme? But is it more or less diffusive as a simple, numerically efficient Eulerian scheme like say the Bott-second order scheme (Bott, 1989)? When 3-dimensional chemical transport models were developed decades ago, many numerical schemes were developed and tested against each other in very simple configurations, for example (Rood, 1987; Müller, 1992a). The Prather (1986) scheme has been very successfully used in chemical transport schemes – could it be an alternative to using Lagrangian transport (McKenna et al. (2002) did such a comparison and concluded that it was not an alternative)?

p. 18730, l. 24: It should be possible to approximately quantify *how* severe the overestimation is.

p. 18732, l. 17-20: I suggest to emphasise the point that the substantial model improvements demonstrated in the paper were achieved by employing a superior transport scheme in spite of the still rather limited horizontal resolution of the model. Does this mean that deficiencies of models of a comparable horizontal resolution (e.g., the problems with representing a transport barrier at the edge of the Arctic vortex reported by Sankey and Shepherd (2003)) are *not* caused by a poor representation of the overall dynamics. The observations that an enhanced horizontal resolution in CCM simulations leads to an improved simulation of the stratosphere (that is sometimes made) might then solely be due to improvements in the transport scheme (because, of course, a simple increase in spatial resolution also increases the quality of the employed transport scheme, e.g. (Müller, 1992b)). I suggest that this question is discussed in more depth in the paper.

p. 18732, l. 27: I am not convinced that the parametrisation of stratospheric bromine chemistry via the photolysis of Cl2O2 is really sufficiently described in Rex et al. (2003). I suggest to better describe the implementation in E39/C. Further, there should be some discussion on how much the chemical ozone loss in E39/C is enhanced by im-

ACPD
8, S9745–S9755, 2008

plementing the new parametrisation. Perhaps it is possible to show an additional line in Fig. 9, showing the chemical ozone loss in E39/C without the parametrisation of bromine induced ozone loss.

p. 18733, l. 22: The parcels are initialised with equal mass and there is no mechanism in the model that any single air-parcel may change its mass, is this correct? That means there is no mass exchange between particles or at the model boundaries, nor does the number of particles change – which means in turn that the scheme must be strictly mass-conserving. Is this interpretation correct?

p. 18735, l. 20: How realistic is this simulated cooling? Are there any observations to support this model result?

p. 18736, l. 16-24: This is an important paragraph of the paper. Fixing the mixing ratios of NOy and Cly (I am assuming that Clx in l. 17 is a typo) is a key feature of the model. It means that reasonable Cly and NOy fields in the model (and in particularly in the polar vortices) may be maintained even if the upper stratospheric chemistry can not be represented because of the location of the top of the model. (However, perhaps water vapour should be fixed as well as the conversion of methane to water vapour through the chemistry in the upper stratosphere is likewise not represented). On the other hand, this means that the temporal development of Cly in the model is essentially driven by the information provided from the two-dimensional model. Unfortunately, there is no very good publication available on the model, perhaps it helps a bit adding Grooß (1996) as a reference for the model. And probably it would be helpful to provide more information on the 2D model results (e.g. temporal development of Cly/NOy at the upper boundary) and examples of Cly and CFC distributions throughout the E39/C-A model domain rather than only in the core of the Antarctic vortex. (Not of all this information might fit in the paper, but an electronic appendix might be an alternative). Further, I suggest adding the development of Cly in the 2D model to Fig 6., right panel. It looks like E39/C-A shows a decrease in Antarctic Cly too early – could this be due to a 2D model deficiency? Furthermore, the transport of CFCs and N2O is treated

ACPD

8, S9745–S9755, 2008

Interactive Comment

differently in the model; I am not sure why. And how consistent can be the Cly and NOy distributions in the model with N2O and CFCs, when for NOy both the upper boundary condition is under control of the 2D model and throughout the model domain even the CFC mixing ratios? There might be very valid practical reasons for proceeding this way, but I think a more thorough discussion of the strategy adopted here is warranted.

p.18738, l. 23: Eddy (or wave) dissipation is not the same thing as wave breaking. For example, in the polar regions there can be significant wave dissipation leading to an acceleration of the diabatic circulation without the necessity of wave breaking (Fusco and Salby, 1994).

p.18742-18743. Discussion of upper boundary: The arguments laid out here make it obvious that the implementation of an upper boundary for methane (and I would argue for N2O, CFCs and similar tracers as well) makes sense. The argument is the same as for Cly and NOy, it is a way of parameterising the missing sink in the upper stratosphere. Therfore I would suggest to make the 'upper boundary' version the default version of E39/C-A. I would further suggest some more discussion of this procedure. For example, does the implemented procedure of setting the methane values at the upper boundary lead to methane values that (at the upper boundary) correspond to HALOE measurements? If not, then of course one does not expect agreement after the descent in the polar vortex has occurred. Further, it is not clear to me why the improvement in E39/C in high southern latitudes is only marginal. And how exactly is the information on the Cly and NOy mixing ratios at the upper boundary communicated to the Lagrangian air parcels (have I overlooked something)? Finally, a very important point is here that Attila maintains steeper gradients at the edge of the polar vortex than E39/C. However, there is no model result shown that clearly makes this point. Fig. 5 gives some indication in this direction but of course taking a zonal average smears out the edge of the polar vortex. One could do the averaging along equivalent latitudes, but a much better alternative, in my view, would be to show a horizontal cut through the polar vortex for both models at, say 50 hPa.

ACPD

8, S9745–S9755, 2008

Interactive Comment

p. 18744, l. 25: First, I suggest providing some more information in the paper on the Cly at the upper boundary because this is such an important parameter for the simulations. Second, while I agree that there is some uncertainty in the MLS HCl measurements there should be other ways to investigate the quality of the assumed Cly trend in the model. For example, in (WMO, 2007) there are estimated of EESC that could be used as a test. But it might also be useful to scrutinise the underlying assumptions about CFC trends in the 2D model that is used to determine the Cly trend.

p. 18746, l. 6: 'this is caused by a compensation of errors. . . ' This might be a plausible explanation, but is it really demonstrated in the paper?

p. 18746, l. 8,9: I am not sure about this line of arguments: First, Cly should not be as important here as suggested, because the ozone concentrations are analysed for June/July/August when the SH polar region is largely in darkness and chemistry should be slow. The main chemical ozone loss occurs in September. Second, the chemistry in polar winter and spring is rather different from extra-polar ozone chemistry, so why should the general overestimation of total ozone in the model have an impact on the polar lower stratosphere? Finally, it should not be forgotten that E39/C, in spite of its cold bias, substantially underestimates polar ozone loss in the Antarctic (Lemmen et al., 2006).

p. 18746, Fig. 8: I agree that the shape of the profile in E39/C is unrealistic (min. at 30 hPa), however I cannot see that this is due to Cly. First, I am not sure if chlorine activation occurs at these altitudes and, second, I cannot see a drastic difference in Cly in Fig. 5 at this altitude; the difference between the two models is rather decreasing with altitude. Moreover, even E39/C-A does not do a good job regarding the representations of the ozone profile when it would be compared to observations – I would expect. And the situation might be worse when one consider the situation for 2000 conditions. Nonetheless, I suggest doing both: including observations from South Pole and including the year 2000. Likewise, in Fig. 9 one should include information from measurements by adding satellite data (e.g. from the NIWA total ozone climatology).

ACPD

8, S9745–S9755, 2008

Interactive Comment

p. 18747, l. 18: The fact that Attila allows to preserve steep meridional tracer gradients is not well demonstrated in the paper, showing the meridional dependence of a tracer distribution without zonal averaging (and comparing Attila vs. semi-Lagr.) should demonstrate the strength of Attila much more clearly.

p. 18748, l. 14: I agree that the Cly trend is much better described in with E39C/-A than before. But I think more work is required to claim that it is 'realistic' in every respect (see also above).

p. 18748, l. 27: It should be stated here which insights are provided by this simulations, this is an important point.

4 Minor points

p. 18730, l. 25: the wet bias is not 'simulated' it is real... The it is a bias of the simulations compared to observations.

p. 18731, l. 9: replace 'CLAMS' by 'CLaMS', more importantly, I suggest to cite here also Konopka et al. (2004), as this paper describes the extention of the CLaMS concept to three-dimensions.

p. 18731, l. 14: all except the CFCs...

p. 18734, l. 25: replace blended by merged.

p. 18737, l. 14: the abbreviation has been used above.

p. 18738, l. 1: variability of what?

p. 18738, l. 15: 'annular ring' – unclear.

p. 18743, l. 18: Why should the low upper boundary affect the descent in the vortex?

p. 18744, l. 4: decent $\rightarrow$ descend

ACPD

8, S9745–S9755, 2008

Interactive Comment

p. 18747, l. 3: upgraded → improved

p. 18747, l. 19: The results of the water vapour simulation are not shown in this paper.

p. 18749, l. 2: reasons → causes

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ACPD

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Interactive Comment

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ACPD

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