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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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

Anonymous Referee #1

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General Comments

This paper is well-written and has clearly stated goals of documenting improvements in the E39 model’s dynamics and trace gas distributions resulting from a new advection algorithm. It sets out to show that many of the E39C CCM’s problems are due to its diffusive advection scheme. The paper shows the differences between E39C, with Rasch and Williamson advection, and E39C-A using the ‘ATTILA’ Lagrangian advection scheme which is far less diffusive and is also mass conserving. Significant improvements in tracer transport (e.g., CH4 and O3 profiles, UT/LS H2O) and model dynamics (e.g. tropopause temperature) are demonstrated. This definitely merits publication in ACP. The changes I suggest are all minor in nature.

Another stated goal of the paper is “to identify remaining model deficiencies that cannot be cured by a superior transport algorithm”. All models, no matter how sophisticated, will have deficiencies, so perhaps a useful question to answer would be, with the known deficiencies and proficiencies of this model, which issues is the model well-suited for studying? Because the E39C-A is being contrasted with the E39C, a model that participated in CCM exercises predicting future stratospheric climate and ozone recovery, I suspect that this model will be used for similar work. The authors have demonstrated how an improved advection algorithm improves representation of stratospheric dynamics and trace gas distributions, and what some of the limits to improvement are. It would be appropriate for the authors to comment on what type of problem this model is best suited for. Much of what may happen to stratospheric ozone in the future depends on changes in upper stratospheric temperatures, changes in constituent composition (e.g., CH4 and NOy, both of which have important sources or sinks in the upper stratosphere), and changes in planetary wave propagation and dissipation (much of which occurs above 10 hPa). All of these important factors strongly affect ozone recovery and climate change and thus require realistic, physical representation of processes occurring above 10 hPa. I invite the authors to discuss what the E39C-A with its known proficiencies and deficiencies is best suited for.

A valuable addition to this paper would be a discussion of how the new advection scheme actually improves the model dynamics. This is touched on briefly in several spots in the paper, but it would be nice to see a short summary (perhaps in the conclusions?) describing the ways in which improved transport affects model dynamics.

Minor Comments and Questions

p. 18733, l. 18. The mean age with ATTILA may be improved over E39C but it is still younger than most of the current CCM models (e.g., Eyring et al. 2006). Its mean age is realistic in the tropics but is too younger by 1 year or more poleward of 20o in both hemispheres. It is a stretch to call this realistic outside of the tropics.

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p. 18736, l. 6-7: Does this mean the 3D CO_2 field is prescribed? If so, why isn’t it a transported species?

p. 18738, l.15-17: How can you know whether the transfer of wave energy is correctly simulated in a model? What is ‘truth’? What observations or analyses show this?

p. 18738, l. 27-29: Model dynamics are affected by 1) changes in the transport of radiatively active species (which leads to temperature changes), and 2) the propagation/dissipation of planetary waves. Or, does 1) cause 2)? This is related to my general comment of wanting to see more discussion about the mechanism for improvement of the model dynamics.

p. 18739, l. 20-22: Can you explain or speculate why the E39C-A has a smaller cold bias than E39C? Is it mostly due to changes in water vapor transport, or are there other factors (e.g., resolution, other radiatively active species) that affect temperature?

p. 18739, last paragraph: “It should be mentioned that both the advanced numerics of the advection algorithm and the improved model dynamics contribute to …”. Do the improved model dynamics derived directly from the change in the advection algorithm? If not, then where do the improved model dynamics come from? And if so, then please discuss how the improved advection leads to the observed improvements in model dynamics.

p. 18740, last lines: It might be noted that the ‘apparent phase lag’ and the change in amplitude with height are also impacted by model vertical diffusion. Diffusion acts to shift the phase upward leading to an even faster apparent transport and a more rapidly decaying amplitude. Since the E39C advection is described in this paper as being very diffusive I suspect that vertical diffusion plays a role here.

Could you add overlays of the HALOE water vapor contours on the E39C and E39C-A figures (Figure 4)? This will make it much easier to gauge the degree of improvement in the E39C-A model.

p. 18742, l. 1-21: Most CH4 losses occur above 4 hPa. While weak polar descent plays some role in the inadequate representation of CH4 below 10 hPa, the primary problem is not having an upper atmosphere for chemical loss to take place in. This is mentioned at the end of these 2 paragraphs, but as it is main reason for high CH4 in the model, I think your discussion would benefit from stating this right away. This means you don’t need to say that you would expect ATTILA to do better (in fact, you might expect it to do about the same). Also, this shows that Eyring’s attribution of this problem to transport isn’t right either. On the next page (p. 18743, l. 12), I would describe this as an ‘unrepresented’ rather than ‘underestimated’ methane sink.

p. 18743, l. 17-19. I agree with your conclusion that the low upper boundary may limit descent inside the vortex. However, it would be interesting to apply some of the tests in the CCMVal evaluation table to see how isolated the Antarctic vortex is in spring – this would make it easier to identify the cause of the too high CH4 (too much horizontal mixing or insufficient descent). Not all tests depend on the CH4 mixing ratio.

p. 18744: Some questions about the Cly upper boundaries that are used: Are they the same in the E39C and E39C-A simulations? Is the maximum Cly of that boundary condition always consistent with (i.e., less than or equal to) the assumed total Cl of the CFCs emissions at the surface? Midlatitude Cly is also important to ozone recovery. It would be interesting to show a midlatitude Cly comparison between the two models similar to what is shown in Waugh & Eyring ACP 2008, Figure 1.

p. 18745, l. 25-27: Do you think the improved representation of the ozone gradient at the tropopause results from improved model dynamics (i.e., the change in the location of the tropopause) or from improved transport (i.e., improved gradients due to the change in the diffusiveness of the transport), or both?

p. 18746, up to l. 10: Cly is more realistic in the Antarctic vortex in E39C-A and

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the cold bias is reduced, however, Figure 2 suggests the Antarctic temperatures are still lower than observed. If this is true, then how can higher Cly and more-than-cold-enough temperatures result in a greater overestimation of total O3 in E39C-A? Could the higher total O3 be caused, at least in part, by a lower tropopause in E39C-A?

p. 18748, l. 19-24. I understand what you are saying but I would suggest a different approach. A point that should be emphasized is that the E39 models do NOT adequately capture all relevant source and sink processes, and because of this, some of the standard comparisons with observations (long-lived trace distributions) would lead one to misinterpret the cause of the disagreement. I agree that you have to be careful (and you have been) in diagnosing a model with a low lid. Based on what this paper shows about the diffusiveness of E39C I think that Eyring is in part correct about E39C transport, even though the lack of upper stratospheric CH4 loss also contributes. But you are also making the additional, valid point that disagreement with long-lived trace gas distributions alone does not prove transport problems. This is demonstrated by the UB sensitivity experiment.

Figures 7 and 8 do not have a legend showing the identity of the colored lines like the other figures do.

Figures 8 and 9: Would it be possible to show these anomalies and differences derived from observations too? It would be very helpful to see how much closer the E39C-A is to agreeing with observations. This is relevant to the goal of identifying which aspects of the simulation cannot be fixed by the improved advection algorithm.

Language/Technical corrections:

p. 18729, l. 3: ‘…knowledge of the capabilities’ p. 18731, l. 6: delete ‘Concerning chemistry modelling’ – the sentence doesn’t make sense as written. p. 18731, lines 12-14. The wording is odd. Suggested rewrite: “Here we present E39C-A, an upgraded model version of the couple chemistry-climate model E39C, employing the Lagrangian advec-

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tion scheme ATTILA for transport of water vapour, cloud water, and chemical trace species.” p. 18731, l. 21: delete ‘performed’ p. 18731, l. 23: change ’shortly’ to ’briefly’ p. 18733, l. 6: Suggest using units of Tg(N)/yr instead of /a. p. 18733, l. 10: change ‘has been used’ to ‘was used’;. p. 18737, l. 8: I think what you meant was “This aspect is of particular interest in view of the need for reliable estimates of future changes.”

p. 18737, l. 27: delete the comma after ‘both’ p. 18738, l. 21: suggest this rewrite…”…represented by the E39C, allowing planetary waves to propagate…”;. p. 18739, l. 16: did you mean “…wave activity and the temperature response of…”? p. 18739, l. 27: delete two commas and add two ‘the’s: “…that both the advanced numerics of the advection algorithm and the improved model dynamics [no comma] contribute to…” p. 18740, l. 6: change ‘are obtained’ to ‘is obtained’;. p. 18740, l. 19-20: subvect/verb agreement. “The annual variations in…which provide information…” p. 18742, l.18: “In both model versions,…” p. 18744, l. 3-4: change to “..to what extent air masses…descend inside the polar vortex…” p. 18745, l. 7-8: “in the case of” and add a comma after the parentheses. p. 18746, l. 26: The change from -8% to -12% is actually a decrease. One could say “the increase in the negative trend…” or “the increase in the magnitude of the trend…”;. p. 18747, l. 21: change to “…heating rates, modelled temperatures also benefit…” p. 18757, change to “The slope of both models differ significantly…” p. 18760: May I suggest a rewording for the middle sentence of the caption? “Light blue (E39C) and orange (E39C-A) show the sensitivity of the simulation to using an upper boundary condition with a meridionally varying CH4 gradient.”

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The Reithmeier et al. (2008) Clim. Dyn. reference is actually 2007.

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