

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

Anonymous Referee #2

Received and published: 22 December 2008

General comments

The relevance of the paper is two-fold: first, the authors apply a Lagrangian transport scheme (in a CCM not a common practice) to the ECHAM-L39 version extending this scheme to all chemical tracers. They find significant improvements for tracer distributions in general and for ozone relevant substances specifically, reducing substantial deficits observed for this model configuration in previous model-model and model-observation comparisons. The paper underlines the importance of the treatment of transport in CCMs which even influences chemo-dynamical coupling effects via wave propagation, and it provides a possible solution for the ECHAM/L39 model environment, valid for its own. Second, the relevance of the paper goes beyond the specific model configuration, as it also touches the important question if low top level mod-

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



Interactive
Comment

els (as the their model is) can be used for chemistry climate coupling studies in and via the stratosphere. This would be preferable just by practical reasons. The authors are able to attribute some deficits of the model to the standard transport scheme, but by analysing the influence of the upper boundary condition, the authors identify some influence of the mid and upper stratosphere for chemistry climate studies in the stratosphere too.

Despite the paper's high relevance deserves publication, the paper needs major revisions before being published: The authors strongly refer to the results of the paper of Stenke et al. (2008), especially when discussing stratospheric dynamics, thereby repeating the findings and arguments of Stenke et al. (2008) too broadly (despite the authors apply a transient run in the more recent experiment). In this respect I would ask the authors to focus on new results specific to the extension of the ATTILA scheme to chemical tracers. On the other hand, they completely miss a thorough discussion of the dynamical effects of the changed ozone field, which was not included in the former study. Perhaps I misunderstood the paper, and ozone wasn't coupled to the dynamics. In that case the study isn't complete. Or ozone is coupled to the dynamics (as I assume), and then the effect of the changes of the ozone profile on dynamics, especially evident in the lowest stratosphere, has to be discussed. In the light of the better performance of other models not applying Lagrangian transport schemes some discussion of what really causes the improvements (is it the Lagrangian scheme or just effectively higher resolution, direct or by means of sophisticated transport schemes) would be helpful.

Specific comments

p-18728 in the abstract and also spread over the paper the authors use abbreviations for their different model configurations. Within the abstract I would prefer to avoid too much use of them. The explanation of E39C as ECHAM4.L39(DLR)/CHEM or E39C-A as E39C-ATTILA isn't really helpful. Perhaps a table describing the different versions could help.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

p-18728/I-13: this has been found already by Stenke et al. (2008) and is not a new result.

p-18730/I-2: Can you give a reference for this statement?

p-18733/I-31 and p-18741/I-9: Whereas the meteorology may tolerate loosing some horizontal resolution in the forcing (eg. many models use zonal mean heating rates and don't include tides) transport and chemistry requires a statistically sound representation of the tracer fields. As the number of air parcels scales with the pressure, I guess there are only very few air parcels left representing for example the polar vortex near the upper boundary. This could introduce inconsistencies in the model which should be analysed and discussed. Does this problem limit the applicability of the ATTILA transport scheme, also with respect to computer resources?

p-18735/I-9, whole paragraph: what are the effects of this change to dynamics?

p-18736/I-20-24: Total Cly will depend not just on the boundary conditions of CFCs but also on the mean transport paths and times in the stratosphere. Does the 2D model show a 'realistic' change of the Brewer-Dobson circulation and can you specify the size of the both effects? Does the use of prescribed CFC-fields mean that coupling effects may be underestimated?

p-18739/I-22: What is the reason for the different behavior in NH and SH (off in SH, but better agreement for E39C-A, good agreement for both versions and no change)? H2O and T is changing in both hemispheres?

p-18744/I-2, whole paragraph: would a upper boundary condition similar as introduced for CH4 further improve the results?

p-18745/I-14: The comparison in the winter/spring hemisphere may be misleading as the influence of the transport barrier at the vortex edge may outweigh vertical mixing effects.

Minor Comments

p 18728, l 5-9: You may put these sentences into the introduction.

p-18735, l-9: Change 'Eruption-related' to Additional radiative heating by volcanic aerosols

p-18740/l22: atmospheric tape recorder is introduced on page 18730; why do you use here 'so-called'?

Technical corrections

Fig-7: a legend could be helpful

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 18727, 2008.

ACPD

8, S10033–S10036, 2008

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

S10036

