**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Sensitivity of tracer transport to model resolution, forcing data and tracer lifetime in the general circulation model ECHAM5" *by* A. Aghedo et al.

A. Aghedo et al.

We thank the anonymous referee #3 for the valuable comments. Our responses are provided immediately after each of the referee comments (in bold).

Suggestions, questions, and specific comments

1. The meaning of the term "forcing data" in the title is not clear by reading the title alone, so I suggest to replace it with "meteorological fields" or something similar.

The term "forcing data" is replaced with "prescribed meteorology".

2. A study on the dependence of model results on horizontal and vertical resolu-

tion should include a discussion on the convergence properties of the numerical solutions. In this context, the authors should also highlight the practical implications of their numerical results (recommendations on which model resolution to use, etc.). They should also briefly discuss how the quality of the transport processes could be evaluated by comparing with observations in a follow-up study (i.e. whether there is better agreement with observed quantities by increasing the resolution or by using nudging techniques).

The systematic errors and convergence properties of the simulated climate in the ECHAM5 model has been discussed in Roeckner et al, J. Climate, 19, 3771–3791, 2006. We agree that the quality of the transport processes can only be evaluated with observed tracer of known source (i.e. emissions), lifetime and atmospheric concentration e.g. SF6 (as used for the MAECHAM4 model in Manzini and Feichter, J. Geophys. Res., 104, 31097–31108, 1999). Unfortunately, the setup and the use of artificial tracers in the simulations make it impossible to make any recommendation on which model resolution is "best", further work based on tracer(s) with known emissions and concentrations would be needed in this regard. Having said all this, our results shows that in contrast to what was found by Roeckner et al, 2006 that T42L31 is not superior to T42L19 for climate simulation purposes, we argue that there is an added gain in using T42L31 when it concerns transport of tracers such as needed in the GCM with chemical tracers. We have included this explanation in the manuscript.

3. p.139, lines 17-21: The underlying assumption seems to be that differences in model resolution might play an important role in explaining differences in the distribution or seasonality of atmospheric trace gases from different models. This assumption is not necessarily true. An example where this assumption does not seem to be supported is a recent study from the HTAP initiative (the passive tracer experiment) which indicates that the major cause of the spread in model results are differences in the emissions. The authors should briefly comment on whether other factors (e.g. emissions, operator splitting (this is

related to point 5)) could dominate the influence of the model resolution on the tracer transport.

We understand that there are other drivers of model differences, for example emissions, as rightly pointed out. However, our current focus is on only one model, which is ECHAM5, and the difference in the transport of tracers within ECHAM5 that is solely due to model resolution (both vertical and horizontal) and prescribed meteorology. Therefore, our introduction was tailored to reflect the focus of the manuscript. We do not make any assumption that all model differences are due to model resolution, actually the lines being quoted includes the words "may" and "some", which the referee is overlooking. We stated "Also by comparing the results from different model resolutions, idealized tracers may explain some of the discrepancies observed in the distribution or seasonality of atmospheric trace species in different models..." Furthermore, the HTAP passive transport study does indeed show significant inter-model differences caused by transport, because in these runs all models used the same emissions. A few models also submitted results in different resolutions and these generally show different results – the TM5 model is an exception because it was tuned to give similar results for different resolutions. We do not feel obliged to discuss differences of models under a multi-model comparison, since we did not perform a multi-model study.

4. p.140, line 1: In order for this study to be an assessment (or extension of an assessment), i.e. to assess the quality of the ECHAM model in terms of tracer transport, it is necessary to provide more observational data. Otherwise, assessment should be replaced by "analysis of the sensitivity of tracer transport in the ECHAM5 model to model resolution etc."

Assessment is now replaced with analysis.

5. p.141, line 25: Explain the meaning of strang splitting. Although the focus of this paper is on the influence of model resolution, it would be interesting to analyze the influence the particular choice of the operator splitting method has

on the tracer transport (see e.g. Dubal et al., Mon. Weather Rev., 132, 2004). It is not necessary that the authors provide a detailed discussion, but they should comment briefly on the potential influence this numerical technique has on the accuracy of the tracer transport, and whether this has been analyzed elsewhere with respect to the ECHAM model.

We removed the reference to the term "strang splitting"; since we do not discuss this particular method of operator splitting in our paper. We did not alter any of the parameterizations (including operator splitting) of the ECHAM5 model.

6. p. 141, lines 27-28: The authors keep the mass mixing ratio of the tracers at a constant value of 1 in each source region. On the other hand it is possible that the transport processes will cause the tracers to be redistributed within a source region and also transport tracers back into a source region. How are these potential changes to the mass mixing ration handled in the model? Are they simply discarded for grid points within a source region (i.e. are the grid points in a source region reset to 1 after each process was computed)?

The tracer concentration is restored to 1 at each time step after the tracer is depleted, either due to the lifetime decay or transport out of the source region. The tracers decay everywhere according to their respective lifetimes. We have provided additional explanation based on this comment. Kindly see our response to point 1 of Referee K. Bowman.

7. p.142, line 12: Explain why the pressure at the second level is always 30 hPa. My understanding is that the pressure at each level changes throughout a simulation due to changes in the surface pressure and because ECHAM uses a hybrid sigma-pressure vertical coordinate. Also explain whether the level at the tropopause region changes during the runs (depending on where 100hPa or 200hPa is located) or whether this level is fixed for the duration of the simulation.

We define all tracers based on actual pressure levels. ECHAM5 uses a hybrid coor-

dinate system that calculates the pressure, P at a given location and time, t from the relation: $P(x, y, z, t) = A(z) + B(z).aps(x, y, t)$; where, x is longitude, y is latitude, z is the level, and aps is the atmospheric surface pressure. See our response to referee #1 comment 1.1. Near the surface, the pressure are based on the sigma levels (A tends towards 0), and the topmost $3 - 4$ (depending on the number of vertical levels in the model) levels are pure pressure coordinates, because $B = 0$. We have removed the tropopause tracers (see our response to referee #2 comment 3).

8. p. 142, line 17: The term normally is confusing. The authors should state explicitly that they used 5 months for all experiments except for some special cases where the influence of the tracer lifetime was analyzed.

Done

9. p.142, line 28: Define the term quasi steady state the first time it is used. Is it an approximate steady state, defined in some statistical sense? Is it simply the 4-year average?

What we mean by quasi steady state, is that there are no significant increase in the global-mass of the tracers (that is, the rate of change of the global-mass of the tracer remains approximately constant) over the last 4 years of the simulation.

10. p.144, line4: The authors mention that most of the simulations reached a quasi steady state. They should be more specific and note which simulations did not reach quasi steady state and provide some explanation, if possible.

We have removed the word "most". In the manuscript version prior to the submission to ACPD, we run all models for 5 years irrespective of tracer lifetime, but we have extended this run to 13 years before the ACPD submission.

11. Figure 2: It appears that there is still a trend in the surfN plot (i.e. the simulation is not in a quasi steady state yet). Is this correct and is there an explanation why this is the case for this specific tracer? It also appears that in the tropT

plot the average deviation of the T63L31 curve from 1.0 is larger than that of the T106L31 curve from 1.0, which shouldn't be the case. Please check whether this is indeed the case or not. The T63L31-era40 run shows a tendency not to reach quasi-steady-state over the 4 year period (stratS, stratT, tropT). Do the authors have any explanation for this behavior?

We do not consider the trend in surfN to be significant. Even if we run the model for more years, it will amount to a mere computational time wastage, since the values of R in each of the resolution shown on Fig. 2 is not going to change, and the conclusions would essentially remain the same. See response to referee #1 comment 3. We calculate R for each resolution in such a way that the seasonality of the tracers is preserved in all resolutions, including T63L31. See response to question 10 above. What we see in the T63L31-era40 is not a steady-state issue but the influence of the prescribed meteorology, which varies from year to year, and reflects strong dynamical tendencies such as the ones already discussed in the manuscript.

12. p.144, line 14ff: There does not seem to be a significant difference in the R values of the stratospheric tracers (stratS and stratN for T42L31 and T63L31, and stratT for T42L31) between the L19 and L31 runs. This should be noted. Is this due to an insufficient resolution of the stratosphere in the ECHAM5 model? The statement that there is little influence of the horizontal resolution is too general. The authors already mention two exceptions to this rule, and also see point 14.

There are only 4 (in 19-level models) and 5 (in 31-level models) pressure levels above the 100hPa in the ECHAM5 model we used for the simulations (see response to referee #1 comment 1.1). This limited number of levels may be responsible for why the stratospheric tracers (stratN, stratS, and stratT) show no distinction between the L19 and L31 models. We have modified line 14–16 to include this comment.

13. p.145, line8: Since these simulations are not AMIP2 runs, the term AMIP2 should be omitted (or replaced by AMIP2-style).

done

14. p.145, line 15 ff: The statement that there is little difference between the T42L31 and the T63L31 runs is too general. The plots in Figure 2 clearly show a difference in the tropics (stratT, tropT, surfT) which should be mentioned.

Done

15. p.149, lines12-13: These statements are somewhat too general as I indicated previously (point 12 and 14) and they should be modified.

Done

16. p.160: Are the plots in Fig. 4 the 4-year average? If that is the case, it should be mentioned.

Done.

Technical corrections

All suggested technical corrections have been sychronised with those suggested by other referees, and they are all considered in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 137, 2008.

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