

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.

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We thank the anonymous referee #2 for the useful comments. Our response are provided after each of the referee comments (in bold).

Specific comments:

1) Why was the T63L31 resolution chosen to test the tracer lifetimes, and the effect of meteorology? How would the results of these tests have differed if a different resolution had been chosen?

T63L31 is the resolution used by the Max Planck Research climate group in the IPCC

AR4 simulations and therefore represent a “preferred” resolution. We do not believe the results would differ if for example T42L31 is used instead of the T63L31, however this would remain to be tested.

2) At several points, it is mentioned that the inter-hemispheric transport times are lower than those in the literature. The authors suggest that the results of Levin and Hesshaimer (1996) may overestimate the transport time, however they point out that the results of Czeplak and Junge (1974) are also larger than those presented. A discussion of the reasons for the lower estimate with ECHAM5 should be included.

We have recalculated the inter-hemispheric transport time according to the suggestion of K. Bowman, by using only transport from one extra-tropics to another (i.e. surfN to S regions and surfS to N regions), and this resulted in a 4-year mean IHT ranging from about 17 months in the T21L19 resolution, to approximately 12 months in the T106L31 resolution. The assumption that the ITCZ was located at the equator caused too low inter-hemispheric transport time.

3) I’m not totally clear on how the tropopause tracers were set up. Is the altitude of the tropopause fixed in the model, and are the tracers released at single model level in each region (N,S,T), through the middle of which runs the tropopause (as it appears in Figure 1)? Or can the level of the tropopause change? At different vertical resolutions, is the fraction of the tracer being released into the troposphere and the stratosphere identical? Similarly, when the horizontal resolution changes, does this affect the total volume of source model boxes for the tropopause tracer which is in the stratosphere and the troposphere?

The altitude of the tropopause is not fixed in the model. All our tracers are idealized, and we put the “trop” tracers to the model levels which corresponds to 200hPa for “N” and “S” tracers, and 100hPa for the “T” tracers (see the experiment description in Section 2.2). Therefore, the levels where the “trop” tracers are defined will change

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during the course of the simulation (due to influence of the surface pressure on the sigma-hybrid levels). We however agree that the actual thermal tropopause (defined according to WMO definition) of the model differs from our definition of "tropopause" in the injection of the tracers. In this regard, the location of the tropopause tracers (tropN, tropT and tropS) may be different from the actual thermal tropopause of the model. Our calculation of the global mass of each of the tracers shows that the source of each tracer is not identical across various resolutions; this is why we have based our analysis on normalized quantities that accounted for these differences. To put this in another way, if we fix the total amount of tracers that are released, we do not need to update the tracers' mixing ratio after depletion or transport out of the source region. We are confident that our experiment design captures the objective of this study.

Having said all this, we would remove the tropopause tracers since their definition (and thus characteristics) may not really reflect the behaviour of the actual model tropopause.

4) pg 142 - The 50 month lifetime tracer required a longer spinup time, and therefore this experiment was run for 13 years, where as the others were run for 5 years. In order to evaluate the effect of tracer lifetime, all tracers should be run in the same model experiment, which is run for 13 years. The last 4 years can be analysed for all tracers then. That way, differences in circulation will not affect the results.

All our experiments are driven by constant yearly circulation pattern, because monthly-mean climatology of the sea-surface temperature and sea-ice fields of the year 1990s are used to drive the simulations (except the T63L31-era40 simulation, see section 2.2, page 142, lines 18– 20). Therefore, the total number of years (as well as the exact years) is rather arbitrary. We made sure all simulations reach steady state (i.e. the yearly change in the tracers' global masses is constant) before we perform the analysis. What we note in the manuscript is that tracers with long lifetime (e.g. 50 months) will normally require a longer simulation period to reach this steady state, due

to their build-up in the atmosphere until they start to decay.

5) Page 144, line 4-5: The author writes "most of the simulations reached a quasi steady state over the last 4-year period". How can the results be compared for those simulations which did not reach steady state?

Figure 2 shows that all simulations reach quasi steady state. The experiments involving 50-month lifetime tracers require longer simulation time to reach this quasi steady state. We will delete the word "most" in the revised manuscript. We have included our definition of quasi steady state, as used in this manuscript, according to comment 9 of anonymous Referee #3.

6) Page 144, line 14 and 15 - Some discussion of the reasons for the lower R_i values of the L19 runs vs the L31 runs is needed.

This is noted.

7) Page 144, line 17. Horizontal resolution does make a difference for several of the tracers, for example: trop T, T106L31/T63L31 are rather different from T42L31; stratT, T63L31 differs from T42L31, in addition to the differences between T106 and the other L31 experiments, which were pointed out by the authors. The differences between T63L31 and T42L31 in the stratT and tropT panels of Fig 2 are similar to those of the surfT tracer for the T21L19 and T42L19 experiments, which was pointed out by the authors as a "notable exception".

We have now clarified this point, and reformulate the entire paragraph. We have removed the tropopause tracers according to our response to point 3.

8) Page 144, line 21–22 this appears to be related to what I wrote in point 3. If the position of the source regions changes relative to the tropopause, it is rather hard to quantify the effect of the resolution on the results. If this is the case, the tropopause tracers should be removed from this study.

Please see our response to point 3.

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9) Page 145, lines 22–24 The T106L31 results do not fit in with the trend of increasing transport with increasing model resolution. This should be mentioned.

Noted.

10)Page 145, line 24-26 If the coarse resolution model simulations have increased vertical mixing, I would have expected the export fluxes from the source regions to be larger than in the fine resolution simulations. Figure 2 shows that this was not the case.

Figure 2 shows the export fluxes in all directions (both vertical, zonal and meridional). Increase vertical mixing does not necessarily translate into a stronger total export flux. Due to larger boxes and thicker levels, the coarse resolution (L19) tend to re-circulate the tracers more within the latitudinal bounds of the source region (this may be why the total export flux is lower in the coarse models resolutions, as shown in Fig. 2).

11) Page 146, line 20-23: The T21L19 simulation shows a smaller vertical transport in the case of the StratS tracer below 750hPa, than the two L31 simulations.

We now include separate discussion on the surface and stratosphere tracers, providing separate discussions on the role of horizontal and vertical resolutions on the vertical transport of tracers.

12) Page 148, line19-21: It looks like the vertical resolution is more important than the horizontal resolution for inter-hemispheric transport time. A comment to that effect could be included. I take it that "AMIP2 runs" refers to the runs in this paper that were set up in a similar way to the AMIP2 study, and not to runs from the Gates et al. 1999 publication. If this is the case, they should not be described as AMIP2 runs, it is confusing. Likewise for the use of this term on page 150.

These points are noted. The runs only have a similar setup to the ones in Gates et al, 1999. We will replace the word AMIP2 with AMIP2-style or the model resolution.

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13) Page 149, line 11: The statement that transport is more vigorous in the finer resolution models is a bit too general. Table 2 shows the opposite, for vertical transport. It is interesting that in Table 2, the vertical transport mostly increases between T21L19 and T42L19, but in the L31 simulations, it decreases with increasing horizontal resolution.

We have removed this generalization, and provide further clarifications in the revised manuscript.

14) Page 149, line 28-page 150, line 1: “tracer lifetime has a strong influence on the seasonal cycle of the tracers”: As far as I can see, this was not discussed earlier in the paper, nor was any evidence to support this statement presented.

We have now added additional discussion on the influence of tracer lifetime on the seasonality of the transport of tracers in Section 3 and 4. This finding is as expected from the relation between tracer lifetime and their concentration variability (e.g. compare the seasonality of ozone, CO and CO₂, CH₄ concentrations presented in Derwent et al., 1998), short-lived tracers has larger seasonal variability than those of the longer-lived tracers.

Technical corrections:

15) Page 139, line 11 - The word “hinted” is not really appropriate. Genthon and Armengaud (1995) actually suggested rather strongly that model resolution affected the results, although the conclusion was based on results from different models or model versions.

The word “hinted” now changed to “suggested”. The entire sentence is changed to “Genthon and Armengaud (1995) suggested that model spatial resolution could be an important factor in the simulation of the distribution ...”

18)pg 140 line 24- page 141 line 4 - I do not see why the description of the cloud

scheme, and the transport of water vapour, ice etc. is necessary. I suggest removing it.

We would keep the parts describing the transport of tracers, and delete all cloud parameterization description.

Other technical corrections are noted.

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