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Geoscientific Model Development
Discussions

Interactive comment on "Age of Air as a diagnostic for transport time-scales in global models" *by* Maarten Krol et al.

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gmd-2017-262 A new look at patterns of inter-hemispheric mixing and how the troposphere disperses pollutants.

This is a clearly written paper, easy to read, that describes the results from a TransCom type of study in which a group of models ran the same protocol of trace species in order to test large-scale transport and mixing. There a fundamental difficulty with the protocol itself, and it should be revised before the next generation of studies in inter-hemispheric transport are begun. Nevertheless, it is after the model runs have been completed, and I would just recommend that the authors admit the inconsistency and make the most of what has been done. There is still considerable science to be gotten

out of the results, but one should be more careful about drawing conclusions when comparing the fundamentally different protocols: fixed surface abundances vs. fixed surface fluxes.

Protocol Problem: The age-of-air (AoA) tracers are forced with surface abundances while all the others (SF6, Rn, e90) are flux driven. The problem with surface abundances is (1) the flux from surface to atmosphere will depend on each model, particularly seasonal mixing, and (2) there is no atmospheric species that I know of with a fixed surface abundance. Notice that e90, while an artificial tracer, is still flux driven. This protocol makes it extremely difficult to compare the AoA lessons with the SF6 or Rn observations and draw any conclusions about transport. For example, SF6 and Rn have absolutely no "diode/rectifier effect" since the emissions cannot be stopped and must get out of the boundary layer daily. The AoA tracers can indeed have a diode effect. Alternative approached to defining AoA can be easily defined with the flux boundary – see the CO2 example in Prather et al., 2008.

P3-L17-24. I confess to working in this topic of hemispheric exchange and AoA for 3 decades. I find the referencing OK, but missing some of the original work. Possibly the first stratospheric modeling study to examine age of air and define it for CO2 is Hall and Prather (1993). The Prather et al (2008) work is one of the most thorough efforts to test different advection schemes with the identical wind fields. There are also some very interesting results in Hsu 2014 showing that even the cycle version of the ECMWF fields can notably change STE fluxes.

P7 - are the monthly mean mixing ratios 3D? please state if so. Likewise are the hourly station data 1D (profiles) or just layer 1.

P8 L14 – do you mean 'adapted' instead of adopted?

P11 L29 – is 'IH' defined earlier – might be better just spelling it out everywhere, after all you did not define VT for vertical transport?

P12 L13 – please give real numbers - -how many years older?

P13 L1-4 – The transport from N-to-S in the upper troposphere was clearly shown in the first 3D CFC simulations (Prather et al., 1987), what is truly new here is the S-to-N transport patterns, which have not yet been so well documented, e.g., Fig 3 vs Fig 4. Can you explain why the mechanism is different? Looking at diagnostics of cross-equatorial transport in our model, I suspect that the S-to-N transport occurs at lower altitude via the Indian Ocean monsoons.

P13 L8 – The 'oldest' tropospheric air noted here was also part of the Prather e90 paper, at least compare.

P14 L1 – "Easter Island"?

P14 L7 – This conclusion about BL causing the differences seems speculative. Could you test?

P15 L7 – In addition to Diallo et al 2012, check the Prather 2008 PNAS paper that demonstrated the convergence of two schemes with increased vertical resolution.

P16 L7 – Remember that the SF6 is flux-driven and may show different features than the AoA tracers.

P18 L1 – It really does not matter that TOMCAT accumulates SF6 in the BL because the flux out is the same. The most you would "accumulate" is 1 day's emissions. Try another explanation.

P19 – I like figures 9 and 10. But what is with the NIES result" should not these all go to 0 at the equator? Figure 10 is fun, but not sure how to read it. TOMCAT does not look anomalous in Fig 9 but does in Fig 10? Am I missing something here?

P23 – Please spell out IH and BL etc in the conclusions. The language here should stand alone and not require memory from the rest of the paper.

The Conclusions are a bit too speculative. I think they need to be re-thought and toned

down. This section is the one requiring the most work.

e.g., We know that the models all have different convection, but even if running the same meteorology there are so many other differences (advection scheme, mass conservation scheme, ...) that to say that the convective parameterization is the primary reason could only be justified if one model ran all the different convective schemes in a consistent framework.

e.g., I don't know what a 'seasonal rectifier' is. In the long run all the excess flux from the NH must get into the SH hemisphere and vice-versa. None of it disappears (as would be the case with a rectifier). The flux N-S and S-N is the same, right? So 'faster' is not an obvious attribute. The gradients are clearly different as documented here, but faster is not a useful term.

some references:

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Prather, M., M. McElroy, S. Wofsy, G. Russell, and D. Rind, 1987: Chemistry of the global troposphere: Fluorocarbons as tracers of air motion. J. Geophys. Res., 92, 6579-6613, doi:10.1029/JD092iD06p06579.

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