

## ***Interactive comment on “TransCom model simulations of CH<sub>4</sub> and related species: linking transport, surface flux and chemical loss with CH<sub>4</sub> variability in the troposphere and lower stratosphere” by P. K. Patra et al.***

**Anonymous Referee #1**

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Prabir Patra and his colleagues from the TransCom modeling community present here a comprehensive description of the major results and new findings from the global TransCom methane model intercomparison exercise. The manuscript is very interesting to read, also for experimentalists and well suited for publication in ACP. I am particularly pleased that the results from these model simulations will be made available, and hopefully easily accessible also for the measurement community that is not so experienced in handling Gigabytes of global model output data that needs special software to be digested. (At this moment, however, I have not been successful to access the

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simulation results via the links reported in section 4 of the manuscript. . .)

Besides a number of minor suggestions for changes or clarifications in the text and figures, I have one major point which needs to be addressed before publication of this manuscript in ACP. This concerns the interpretation of the observed change of SF<sub>6</sub> Inter Hemispheric Gradient (IHG) in terms of a change in the inter-hemispheric exchange time (IHET) of air masses. Patra et al. use their Equation 4 to estimate this exchange time from measurements and also from modeling results of hemispheric mean concentrations. But in both cases, besides the concentrations and their temporal changes, calculating the IHET also requires to know the ratio of emissions in the northern and the southern hemisphere i.e. En/Es. In fact, a trend in “observed” and “modeled” exchange time, as found in the present study, may also show up if a change of the distribution of emissions was not properly taken into account in the emissions ratios used for the period in question. Although it is straight forward, and potentially very accurate, to estimate total global annual SF<sub>6</sub> emissions only from observed mean growth rates of SF<sub>6</sub> in the atmosphere, correctly determining its distribution is a very difficult task. It is not surprising that inter-hemispheric exchange times derived from observations and from model simulations agree well when the model results properly reproduce the observations at the respective background stations used for this estimate. However, this does not automatically imply that the trends in the IHET are correct. As has been discussed e.g. by Levin et al. (2010, cited in the manuscript), a tracer with a completely different distribution of emissions (than SF<sub>6</sub>) could help to confirm (or falsify) the estimated changes of the IHET.

Further comments:

Abstract:

It would be helpful if a few words about the meteorological fields (i.e. that they are different in the different models) were mentioned.

Line 44: The term “CH<sub>4</sub> tracers” is a bit confusing for the non expert. Better use e.g.

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“scenarios” (throughout the text).

Line 56: See major comment above. What is calculated here is not an exchange “rate” but an “exchange time” or “hemispheric residence time”. This term needs to be corrected throughout the manuscript.

Line 63: I guess what is meant is not CH<sub>4</sub> “budget” but CH<sub>4</sub> “distribution”. The term “budget” is confusing here as it is normally used for a total inventory.

Line 67: The term “as noted by the 1sigma” is confusing; I would just leave that out. Is it so surprising that the life times do not vary much when the OH fields and reaction rates are pre-set? Generally, I think that, although there were no real scenarios of the sink processes tested in this study, the influence of the sink processes on the CH<sub>4</sub> and MFC distributions should be more thoroughly at least discussed.

Introduction:

Line 75: Unfortunately, the WDCGG data base still does not give a “complete” list of observational programs.

Line 93: May be it is worth to mention here that these are now bottom-up estimates of individual flux components.

Line 99: Explain CTM (and ACTM, line 185, at least once)

Line 117: I have not seen any discussion on diurnal cycles in this manuscript.

Line 139ff: This sentence needs re-formulation: Why should e.g. inert tracer transport of SF<sub>6</sub> have implications on chemically active species?

Section 2:

Line 163: I assume the reaction rates have units, e.g. 1/s.

Line 184: Add “(MFC)” after CH<sub>3</sub>CCl<sub>3</sub>.

Line 192: What about tropospheric Cl as a sink for CH<sub>4</sub>? (possibly of similar size as C7680

the soil sink, see Allan et al., JGR 112, D04306, 2007)

Section 2.2 and Table 1: The explanations in Table 1 are a bit brief. This makes it necessary, when looking at the results, to always go back to Sec. 2.2 for more explanation. Also, it is not really clear to me which anthropogenic emissions were used in scenarios (“CH<sub>4</sub> tracers”) 3-6, i.e. EDGAR 3 or EDGAR 4?

Lines 281ff: Has a seasonal cycle of <sup>222</sup>Radon exhalation rate been used in this study (as is generally measured at sites with fine grained soils)? The potential influence of seasonal <sup>222</sup>Radon emissions will have to be kept in mind when comparing the simulated results with observations.

Section 2.3, lines 306/307: I do not understand this sentence “. . . and do not automatically . . .”

Section 2.4, line 375: Please give a reference for “Pearson’s moment correlation analysis”

Section 3:

Line 410: I guess it should read “Figs. S1-S17”.

Line 427: Include a “more”, i.e. . . . more “leaky”

Line 448: I guess it should be “Figs. S2 – S4”.

Line 470ff: As mentioned earlier, estimating the global SF<sub>6</sub> emissions trends should not be model-dependent. One could even think of estimating this simply from observed trends at one representative site in the northern and one in the southern hemisphere (i.e. without any model).

Line 486ff: “These lifetimes . . . (Prinn et al., 2005)” This sentence is unclear to me.

Line 498: I would say that STE uncertainty should not be tracer-dependent. The EFFECT of STE on the tracer DISTRIBUTION may be different (for CH<sub>4</sub> and CH<sub>3</sub>CCl<sub>3</sub>).

Line 509: I would like to see a bit more discussion on the point made here: This is why also SF6 and 222Radon have been modeled (to check transport differences between the models), right?

Line 545: "... noting that the OH loss is realistically represented, as seen in the CH3CCI3 seasonal cycles." I do not totally agree to this statement, e.g. there is (1) a significant phase shift in the MFC seasonal cycle between models and observations, and (2) noting that CH4 in the southern hemisphere troposphere is mainly sink driven, the seasonal amplitude of this sink seems to be over-estimated by all models by about 20%.

Line 630: I would replace the word "differences" by "range"

Line 632ff: As mentioned above, it is a bit misleading to talk of "observation-based" vs. "model-based" inter-hemispheric exchange times. (But this whole paragraph needs to be revised as explained earlier...)

Line 654: I cannot find such a Figure S17.

Lines 677ff: I think this interpretation (of constant vertical gradients) needs to be explained in a bit more detail.

Line 688: Perhaps include "(Eq. 5)" after "...at 8 sites."

Lines 702ff: Why should the OH estimates from MFC become less uncertain after 2000? May be the method becomes less sensitive because MFC is strongly decreasing? Or is it that the variability of MFC is simply getting smaller? I cannot find the citation Montzka et al., 2011, in the references.

Conclusions:

Line 766: What is meant with "suppressing the seasonality of the underlying emissions"?

Line 769: I guess it should read "A set of six global CH4 flux representations ..."

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Line 801 – end: This sentence needs to be re-formulated.

Figure 1: It is a bit confusing that the surface/soil sink shows up in two boxes, dark green and pink.

Figure 2: I cannot find the black (CTL) line in Figure 2a.

Figure 4: It would be interesting to also see the starting values of SF6 in the different models. Please note in the caption how the "difference" is defined, i.e. observed – simulated or vice versa. The averaging interval for the troposphere (1000-200mbar) is not consistent with the caption in Fig. S19 (850-200 mbar).

Figure 7: Please note in the caption which CH4 scenario is displayed here. It is interesting to note that all models show a steeper IHG for CH4 than observed. May be this is due to some bias in the anthropogenic CH4 emissions distribution (compare remark on inter-hemispheric exchange times).

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 18767, 2011.

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