I think this manuscript presents an important intercomparison of model transport characteristics, with a notable component on stratospheric chemistry and circulation and their contribution to surface trace gas concentrations. The paper includes interesting findings and explanations, such as an apparent increase in the rate of interhemispheric exchange over the past two decades and a possible link to a widening of the tropical belt. Also intriguing is the finding that inter-model differences in stratosphere-troposphere exchange may contribute to differences in CH_4 growth rate.

There are some areas in which I think the manuscript could be improved:

- **The paper would be strengthened with more discussion of the effects of the assumed atmospheric OH abundance on the model distributions of CH_3CCl_3 and CH_4.** Something I found quite striking was the overestimate of the interhemispheric (IH) gradient of CH_4, and to a lesser extent CH_3CCl_3, by a majority of the participating models. (The manuscript lacks discussion of possible causes of these discrepancies and the differences among models.) Given that there isn't an overall bias in the simulated IH gradient of SF_6, it seems to me that the culprit here might be the assumed emissions, and especially OH, rather than transport. Wang et al. (2008) (full reference below) and Wang et al. (2004) (the reference is in your manuscript) provide insight into this, through extensive analysis of the impacts of OH abundance and interhemispheric distribution and trace gas source strengths on the IH gradients of CH_3CCl_3 and CH_4, respectively. Their inversion analyses resulted in a lowering of global OH abundance and emissions in the Northern Hemisphere extratropics in order to reduce the excessive a priori latitudinal gradient across both hemispheres (they demonstrated that their model IH transport wasn't an issue).

  Another result that leads me to suspect that the specified OH abundance in the current study might be too high is the reported median lifetimes for CH_3CCl_3 and CH_4, 4.61±0.13 yr and 9.99±0.08 yr, respectively. You do note that all of the models except for TM5 have shorter CH_3CCl_3 lifetimes than those estimated by a number of previous studies using observed CH_3CCl_3 (4.9-5.0 yr). **You could also cite Wang et al. (2008) here**—they estimated even longer lifetimes, 5.0 yr for CH_3CCl_3 (but note that this also includes a speculative soil sink with lifetime of 45 yr; with respect to tropospheric OH only, the lifetime is 6.9 yr) and 10.1 yr for CH_4 (but this includes the soil sink for CH_4; without it, the lifetime is 10.9 yr).

  The issue of OH abundance could also be discussed in greater depth in the context of the inaccurate simulations of CH_3CCl_3 annual mean time series in Section 3.2.
- It would be helpful if you also provided CH_3CCl_3 lifetime estimates with respect to tropospheric OH only, and CH_4 lifetime estimates with respect to all sinks including the soil sink, for easier comparison with previous estimates including the IPCC assessments.
- Introduction, lines 14-20: Although you cite a good number of relevant previous CH_4 studies, I think you could more precisely and accurately characterize the approaches in the different studies. For example, Wang et al. (2004) not only examined the average "CH_4 emission distributions", but also estimated trends and interannual variations in the emissions and sinks. In addition, Wang et al. (2004) conducted not only "forward modeling", but also an inversion to estimate the CH_4 budget.

Reference:

Wang, J. S., M. B. McElroy, J. A. Logan, P. I. Palmer, W. L. Chameides, Y. Wang, and I. A. Megretskaia, A quantitative assessment of uncertainties affecting estimates of global mean OH derived from methyl chloroform observations, *Journal of Geophysical Research, 113*, D12302, doi:10.1029/2007JD008496, 2008.