Review of "Future trends in stratosphere-to-troposphere transport in CCMI models"

Recommendation: minor revision

This paper by Abalos et al. brings insights on STT as well as the overall atmospheric tracer transport in the UTLS region, and is well written. It is important to confirm the role of tropopause rise on transport in the extratropical UTLS region from multiple models with multiple idealized tracers. Also, the results highlighting the role of advective transport for subtropical STT are informative.

However, the paper does have a few places that are somewhat confusing. Below I'll list my major comments followed by other minor and technic comments. I'm happy to see the revision after the authors appropriately respond or address these concerns.

Major comments:

(a) The use of TEM budget analysis.

Besides that the advective transport shows significant contribution to the subtropical tongue of st80, I feel like the TEM budget analysis does not help that much on interpreting the spatial distribution of STT trend inferred by two stratospheric tracers (O3S and st80). Firstly, I am wondering how consistent, in terms of the spatial distribution, between the trend of tracers (unit: ppbv/decade) and the combined tracer tendency from advective-diffusive processes estimated in the TEM framework (unit: ppbv/day)? My first impression is not so much. For example, none of the advective transport or eddy mixing or combined can explain the negative trend of O3S in the NH extratropics especially the part below the tropopause. Moreover, the prominent positive trend of O3S in the SH extratropics in contrasting to the negative trend in the NH extratropics is suggested by neither advective transport nor eddy mixing of O3S. Therefore, multiple things need to be checked, which include (i) how much the trend is captured by tracer concentration differences between the present and the future, (ii) how much the difference is captured by the resolved transport approximated by the TEM framework. These checks have mostly been done in Abalos et al. (2017) so should not be problems to additionally apply to the stratospheric tracers. Also, Abalos et al. (2017) used tracer concentration difference (see their equation 2) to interpret the future trend of e90, but I am not so sure how valid it is for O3S if there is also a change in O3S lifetime τ (lifetime is fixed for e90 and st80). In sum, I think interpretation of TEM budget analysis should use more cautions if the leading spatial features of tracer trend (especially those near the tropopause) cannot be captured by this framework.

(b) Interpretation of stratospheric tracer trends at the extratropical tropopause.

The paper has shown positive trend of e90 and negative trends of stratospheric tracers over the extratropical tropopause region, except the O3S in the SH extratropics where the authors argued recovery of ozone hole matters. I highly agree with the authors this feature is associated with the upward shift of tropopause. However, I am not so sure for their additional claim on enhanced isentropic mixing on the tropopause. In my opinion, without any change in the strength of mixing at the tropopause, an upward shift of tropopause alone can already cause the increase (decrease) of tropospheric (stratospheric) tracers in the tropopause region. Specifically, as the tropopause shifting upward, tropospheric tracers (e.g., e90) can move further upward before encountering the transport barrier by tropopause and thus more tropospheric tracers near the tropopause region which the positive trend tends to maximize in between the old and new tropopauses (see Fig. 4). By contrast, as the tropopause shifting upward, downward transport of stratospheric tracers encounters earlier with the tropopause barrier, and thus less stratospheric tracers near the tropopause region with the negative trend also maximizing in between the old and new tropopauses, as shown in Figs. 5 and 6. The enhancement of isentropic mixing on the tropopause could indeed amplify this effect, but given the fact that models are not showing consistent results about the eddy mixing component (briefly noted in the manuscript) plus the results of eddy mixing component are much more noisy, I don't think a strong conclusion on enhanced isentropic mixing on the tropopause can be made. Finally, as noted earlier in (a), neither advective transport nor eddy mixing seem to reflect the prominent negative trend of stratospheric tracers in the NH extratropics, particularly the part below the tropopause. Therefore, I suspect that the TEM budget analysis may not show up the effect of tropopause rise on extratratropic tracer transport.

(c) Lack of mechanism interpretation on inter-model differences of STT.
The authors give some good examples of comparing trends of tropospheric-column averaged tracer concentration for O3S and st80 in Section 3.1 and 5 to highlight the inter-model differences, which is "one great merit" of looking at inter-model comparison project. However, when coming across the discussion of mechanism in Section 4, none of these inter-model differences are noted again, so is the spatial distribution in Section 3.2. It is good to focus on common features that are supported by most of models, but the inter-model differences could also bring some interesting insights. For example, models like CMAM show larger tropospheric appearance of st80 than models like WACCM and GEOSCCM, which are likely due to stronger subtropical tongue in CMAM than those in WACCM/GEOSCCM and therefore link to stronger lower BDC and upper HC overturning in CMAM than those in WACCM/GEOSCCM (see Fig. 7). This again highlights the importance of advective transport for STT of st80. The inter-model differences in O3S are more complicated but a brief discussion may be helpful.

Minor (and technic) comments:
P1: Institutions 4 and 5 should switch place.

P6L15-P6L19: From later results, it seems that the tropopause rise is more related to variations in STT over the extratropics instead of the global STT shown by tropospheric burden of these stratospheric tracers. The global STT is likely controlled by other processes (e.g., subtropical tongue due to overturning circulation in the UTLS for st80). In short, I am not surprised that the correlation is weaker for tropopause rise than climate response and I doubt how confident the authors can argue tropopause rise act as an important mediator for the global budget.

P7L34-P8L1: The authors noted some differences about spatial distribution between O3S and st80. I think this should be highlighted more often in the manuscript to warn readers that interpretation of O3S should use more cautions as both variations in stratospheric chemistry and source distributions could yield different behaviors from st80. Also, I suggest the authors to insert cross-section maps for climatological O3S and st80 distribution (either a new figure or superimposed in existing figures as contours) so that readers can have a better idea on how the future changes in tracers compare to the climatological distribution.

P8L2-P8L5: I think st80 in upper-troposphere deep tropics can also be interpreted by later results of advective transport and eddy mixing. From Figs. 9 and 10, the advective transport of st80 in deep tropics generally shows negative trend while the eddy mixing shows positive trend. The eddy mixing component seems to have a larger trend than the advective transport so that the net compensation outcome shows the positive trend. For CMAM model in which the negative trend by advective transport is so strong in deep tropics that cannot be fully compensated by eddy mixing shows a net outcome of local negative trend. In sum, I agree with the authors that variations of st80 in deep tropics are related to enhanced diffusion on the tropopause but not quite sure whether the tropopause altitude playing a role here. As mentioned in the major comments, in my opinion, tropopause rise works better for extratropical STT variations which its influence seems not to be captured by the TEM diagnostics.

P8L14: las -> last

P8L15: Polvani et al. 2019 -> Polvani et al. (2019)

P9L2-P9L3: This part reads so similar to earlier part of ozone recovery-related variations in residual circulation, so it confuses initially. I suggest to diffrentiate at the beginning about the double effects of ozone recovery on STT of O3S: (i) weaken the downwelling of residual circulation leading to less polar O3S accumulation by transport, and (ii) increase polar ozone concentrations. Effects of (i) dominates above 20 km so O3S shows negative trend while effects of (ii) surpass below 20 km so that O3S shows positive trend suggesting stronger STT of O3S at polar regions.

P9L12-P9L13: Although both O3S and st80 highlighting the subtropical tongue for transport in the UTLS region, there are some differences about their advective transport: (i) transport in the deep tropics which is likely due to differences in source, and (ii) subtropical tongue seems to intrude more vertically as for st80 than O3S. Do you have an idea on why is so?

P9L32-P10L1: As noted in the major comments, I suspect how strong this conclusion can be given that the TEM diagnostics seems to fail to capture the effects of tropopause rise on STT.

P10L26-P10L29: Would this be clearer if additional lines for the corresponding RCP6.0 cases are added in Fig.11(a-d)?

fGHG vs fODS: I am interested in seeing how much the addition of fGHG+fODS can explain the full trend seen in Figs. 5 and 6. Also, I think for both O3S and st80, the fODS explains more on the full trend response of STT than fGHG, which could be pointed out at the beginning of P11L14.

P11L23: is “the” strongest

P11L24-P11L25: Should these cross-references be Fig. 12?

P12L5: Polvani et al. 2019 -> Polvani et al. (2019)

P13L10: this region -> the extratropical lower stratosphere