Response

Accurate representation of transport in climate models is difficult. Unwanted numerical diffusion introduces artifical fluxes in the stratosphere which prevent representation of tracer gradients with fidelity. This issue is more prominent in the stratosphere where three key transport barriers - one in the subtropics, the other at the vortex edge and the third near the tropopause - exist. Artifical diffusive fluxes tend to enhance transport across these barriers inducing unwanted mixing of tracers.

Even though such fluxes should ideally vanish at high grid resolutions, integrating comprehensive climate models at such resolutions is not practically possible. Therefore, the subject of focus of the manuscript is an important one. The manuscript introduces (or rather tests) a new class of Lagrangian transport scheme CLaMS and compares its performance with the state of the art transport scheme popularly used by modern climate models. Using the same underlying dynamical fields, age tracers are advected by the two schemes and prominent differences are found in the performance of the two schemes. Care has been taken to isolate the influence of any other factors, as much as possible, in dictating the differences. Resultantly, the results show inter-representation differences in critical areas in the UTLS subtropics and higher latitudes using different transport metrics.

Therefore, these results from testing CLaMS in a practical setting with a realistic background stratospheric flow provides promise and strengthen confidence in the use of Lagrangian schemes in handling tracer advection. I do not doubt the scientific significance of this work and I believe researchers in the climate modeling community will definitely benefit from these results. I find the methodology scientific and the presentation fair. In this regard, I suggest the editor to **reconsider the manuscript pending major revisions.**

To facilitate the discussion process, I provide a couple of comments that require immediate attention and will definitely improve the quality of the manuscript. I follow up with a comprehensive review later.

Comments :

1. Since the authors themselves stress on the tracer being *passively* advected by the background flow, it would be great if the authors can include a figure showing the zonal mean Boreal summer and Austral summer climatology obtained from the model. This would greatly help the readers interpret the results that follow, in particular the meridional extent of the polar vortex.

2. Since the focus of the study is the UTLS region, I am surprised that none of the figures show the seasonally averaged tropopause profile. I urge the authors to add a thermal tropopause to Figures 1, 2, 4, 5, 6 and 7 in the manuscript.

3. In the same spirit as the comment above, since one of the schemes is formulated in purely isentropic cordinates in the stratosphere, where the vertical velocity rep-

resents the diabatic ascent rate - an important quantity for stratospheric mass upwelling - I strongly suggest the authors to demarcate the regions of tropical upwelling and extratropical downwelling by adding a line associated with $\dot{\theta} = 0$ in Figures 1, 2, 4, 5, 6 and 7.

4. The figure captions and the actual figures conflict with each other at a couple of places. In Figure 3 for instance, the relative and the absolute differences are flipped. Moreover, irrespective of the order, the (currrently (a)) relative differences suggest the relative difference shoud go negative at some point between 0.1 and 1 years, but that does not seem to be the case. The same holds for Figure 10 as well, where the Southern and northern hemisphere seem to flip. Due to this, I recommend the authors to revise Section 4.3 again.

5. To the extent of my understanding, Lagrangian schemes have been computationally more expensive than the traditional Eulerian schemes. It would be great if the authors can provide a rough estimate of the computation time used by the two schemes.