Reply to Anonymous Referee #1:

We thank the Anonymous Referee #1 for the careful reading of the manuscript and the valuable comments. We have carefully considered each of the comments in our revision. Our responses are provided below inline in italics.

Suggestions:

1. Title may want to specifically state "Tropical" UTLS.

Reply:
Besides tropical region, we also showed some results in polar region. So it might be more appropriate to change the title to "... in the lower stratosphere (LS)". Noted that we could have results covering the entire UTLS but due to MERRA negative heating rates at lower levels that prevent parcels ascending to the stratosphere, we cannot initiate parcels at lower region.

2. Line 22, page 5994; insert "the" in front of 370K

Reply: *Done.*

3. Line 8, page 5995: states "re-entered the troposphere"....if you're concerned with the UTLS distributions, don't you need to only ignore parcels that can't impact the UT anymore? So, possible, a lower pressure level would be relevant?

Reply:
Those parcels travel back to 250 hPa and below have little impact on the lower stratosphere, so we just ignored them. An even lower boundary level would have no impact on our results because we mostly concern about parcels at and above 100 hPa.

4. Line 9, page 5995; need a "the" in front of ~2200K

Reply: *Done.*

5. Line 20, page 5995; change "waves" to "wave"

Reply: *Done.*

6. Question on heating rates: Does the reanalysis diabatic rates include some portion related to reanalysis increments? That is, if you did a radiative heating rate calculation with the gas and temperature profiles in the reanalysis, would you get the same diabatic heating rate that is stored in the reanalysis output? What causes the differences in Q

shown in Figure 1? Do the four reanalyses have dramatically different temperature and ozone/ch4/h2o/n2o inputs in their heating calculations?

Reply:
The details of differences in diabatic heating rates among reanalysis data sets are a complex issue, and have recently been discussed by Wright and Fueglistaler (2013). The differences in Q are mostly caused by the differences in the long-wave radiative heating, which is affected by temperature and ozone. For example, ERAi uses zonal-mean monthly mean climatology of ozone, MERRA and CFSR use prognostic ozone simulated by the underlying forecast model, and JRA25 uses daily offline calculations. Ploeger et al. (2012) shows that the ERAi heating rates in the tropical lower stratosphere are significantly larger than radiative heating calculation estimates, but this is just one aspect of the overall uncertainties. Part of our motivation for including trajectory model calculations based on both ERAi and MERRA was to test the sensitivity to heating rates for lower stratospheric transport. We have highlighted this in our revised Discussions.

7. Question on chemistry: When you pick out production and loss rates from WACCM and apply them to the reanalysis based trajectories, are you considering any variation in season, or with temperature? If the WACCM temperatures aren't the same as the reanalysis temperatures, does that introduce error? Just a bit more explanation of how the production and loss rates are applied to the trajectory model would be helpful (Section 2.2).

Reply:
We did consider the seasonal variations of production and loss rates from WACCM. In Page 5996 lines 14-17 we mentioned that we calculated production rates and loss frequencies as a function of latitude, altitude, and climatological months to cover the annual (constant) cycle of chemical behavior. However, these calculations will not accurately handle the situation where chemical losses are linked to meteorological behavior, such as intense ozone losses during unusually cold polar winter stratosphere. We have included a brief discussion of this in the revised Discussion section. We have also included more details as to how we applied the production and loss rates to the trajectory model in the revised manuscript.

8. Line 2, page 5998; what kind of MLS climatology is used? (annual, monthly averaged, daily averaged, averaged over what time period?)

Reply:
Thanks for reminding us. We used the climatology of O_3 and CO averaged over August 2004 to December 2012 to set the initial chemical abundances when parcels are initialized. This means that parcels are initialized with a constant annual cycle of O_3 and CO. We have addressed this carefully in the revised manuscript.

9. Line 24, page 5998; change to "differences from MLS retrievals"

Reply: Done.

10. Section 3.1, page 5999: Define "reasonable agreement". I would conclude a spread from looking at Figure 4.

Reply:
In Fig. 4a we see reasonably good agreement between TRAJ_MER and MLS. Noted that the x-axis is in log scale to highlight the differences. Given very low concentrations of O_3 at this altitude (100-50 hPa), the discrepancy between TRAJ_MER and MLS is rather small so we think they are in reasonable agreement.

11. Can you compare WACCM heating rates to those in the reanalyses? There seems to be a significant difference between the WACCM and trajectory runs at 68 hPa. Is that a consequence of a difference between upwelling computed in WACCM versus that in the reanalyses?

Reply:
We could compare the WACCM heating rates to reanalysis, but this would not be especially insightful. Three-dimensional transport within WACCM is based on a finite-volume semi-Lagrangian scheme, and is not directly related to radiative heating rates in the model.

12. Line 7-9, pg 6005; why do you suspect that the ERAi upwelling may be too strong? Please add a sentence summarizing Ploeger's conclusions.

Reply:
Ploeger et al. [2012] performed a radiative calculation based on CHAMP temperature and HALOE ozone and water vapor data, which shows that ERAi heating rates is about 40% too high. This conclusion is consistent with Schoeberl et al. [2012], which shows that trajectory simulations of water vapor tape recorder signal based on ERAi heating rates is at least ~30% too fast compared with MLS observations. Thanks for the reviewer's reminding. Now we have clarified this in the revised manuscript.

13. Line 26, pg 6005; change discussions to discussion

Reply: *Done.*

14. Summary and Conclusions: Could you add a few sentences as to how the trajectory approach presented here provides more information than analyzing an SD WACCM run?

Reply:
In the last version we have mentioned some potential strengths of using trajectory modeled chemical species in the last paragraph. Now we modified this part to make it more obvious to the reader. Thanks for pointing it out.

[**References**]

Ploeger, F., Konopka, P., Müller, R., Fueglistaler, S., Schmidt, T., Manners, J. C., Grooß, J.-U., Günther, G., Forster, P. M., and Riese, M.: Horizontal transport affecting trace gas seasonality in the Tropical Tropopause Layer (TTL), J. Geophys. Res., 117, D09303, doi:10.1029/2011JD017267, 2012.

Wright, J. S. and Fueglistaler, S.: Large differences in reanalyses of diabatic heating in the tropical upper troposphere and lower stratosphere, Atmos. Chem. Phys., 13, 9565–9576, doi:10.5194/acp-13-9565-2013, 2013.