Interactive comment on "The impact of overshooting deep convection on local transport and mixing in the tropical upper troposphere/lower stratosphere (UTLS)" by W. Frey et al.
Anonymous Referee #1

We'd like to thank the reviewer for his/her comments. Please find a point-to-point reply below, the referee's comments are typeset in bold italic, our replies in normal font.

This study primarily examines output from a high-resolution, convection-permitting model simulation of Hector convection near Darwin, Australia. In particular, the case study is motivated by aircraft observations of downward transport of ozone-rich stratospheric air. Passive tracers representing both discrete layers and typical background profiles of trace gases allowed for novel investigation of the simulated transport and highlight many important processes at work in the model. In addition, the important work of diagnosing perturbations to water vapor in the UTLS was completed and related to recent literature. Overall, I find the paper to have sufficient detail and be well written and constructed. However, the argument that stratosphere-to-troposphere troposphere transport was observed in the aircraft observations is not convincing and must be addressed in order for the paper to continue to be motivated as such. Though the work required to address my comments listed below is mostly minor in nature, I consider the importance of several issues to be major and required for the paper to be accepted for publication.

Major Comments:
1. Aircraft Observations: I agree that there is evidence of downward transport, but it is necessary to demarcate the bounds of the TTL in Figure 1 as determined by aircraft (and potentially include a profile of temperature and potential temperature for full disclosure). It is stated multiple times in the manuscript that the tropopause altitude for this case is 17.3 km, but the corresponding potential temperature is not clear. Without proper identification of the tropopause the argument that this air has been transported from stratosphere to troposphere is not defendable. For example, the tropical tropopause (cold point) typically varies between potential temperatures of 370 and 390 K. In the profile shown, a tropopause level of 380 K would largely suggest convective stirring of the lower stratosphere, while a tropopause level of 390 K would suggest stratosphere-to-troposphere transport. I should also note that the model simulations suggest the tropopause is at 370 K, which would imply no stratosphere-to-troposphere transport in the aircraft observations.

Reply: We had tried to accommodate for this by adding the approximate altitude scale on the right hand side of Figure 1. However, we now also include a shading to indicate the boundaries of the TTL (as defined by Fueglistaler et al., 2009, between 355K-425K) and the cold point tropopause as dashed line at the corresponding altitude, i.e. 385K. Thus, the observations show that the transport feature reaches below the tropopause, and consequently we find stratosphere to troposphere transport and also signs of stratospheric stirring.

Furthermore, we like to note that in general the model simulated the cold point tropopause at lower levels than had been observed. That is, at 16.8km/367.3K at 6UTC, 16.8km/368.1K at 9UTC, and 17.1km/372.2K at 12UTC, whereas the observed CPT was located at 17.3km/385K (flight time 3:45UTC – 8:20UTC).

2. Page 1053, line 14. Though mixing in the cloud should be important, it seems more relevant to me what these conflicting O3 and CO characteristics say about the sensitivity to vertical tracer gradients. Since the vertical gradient in O3 from 350-390 K is roughly 3x that of CO, it is likely that the O3 tracer is more sensitive to changes in vertical velocity. In other words, it would take less time to overcome reductions in O3 from overshooting than it would for increases in CO.

Reply: The reviewer is right, a stronger vertical gradient results in a higher sensitivity to mixing. Moreover, a non-linear tracer -tracer relation (as evident in Fig 8a and c) indeed may introduce a dependency on the strengths of gradient.

However, transport and subsequent mixing do not alter CO and ozone independently.

As long as we assume two air parcels which mix (and have distinct CO and ozone mixing ratios before the mixing event), the relative change of mixing ratios will be the same and only depend on the degree of mixing and the amount of air from each 'reservoir' which mix. The relative change of mixing ratio relative to the end members involved should is the same, as long as only two end members are involved.

Depending on the strengths of mixing and the background curvature of the tarcer-tracer relation one can also produce an enhancement of both CO and ozone, if strong diabatic transport as evident in the cloud, does lead to mixing. It depends on the vertical gradient AND on the potential temperature change, which the mixed air parcel will exhibit after it has been mixed.

Since we don't know the latter from inside cloud measurements, we can only state, that the model probably produces a stronger diabatic transport than observed.

Alternatively, horizontal entrainment from outside the cloud could be underestimated. A stronger entrainment would help to compensate the vertical CO gradient inside the cloud relatively stronger than the corresponding ozone, due to the different vertical gradients.

We added the following on p. 13 , l. 23ff. (page/line numbers refer to the attached manuscript with tracked changes): “These tracer perturbations indicate that mixing and updrafts within the cloud are very active, leading to the enhancements of the boundary layer tracer. This suggests that either the diabatic upward transport of the tracer is too strong or alternatively horizontal entrainment from outside the tropospheric part of the cloud is underestimated. The latter would also lead to a homogenisation of the tropospheric tracer rather than the stratospheric tracer, which shows a stronger enhancement due to the stronger vertical gradient. “

Minor Comments:

There is a recent paper that documents novel observations of stratosphere-to-troposphere transport in deep convection that should be cited somewhere in the Introduction: Pan, L. L., et al., 2014: Thunderstorms Enhance Tropospheric Ozone by Wrapping and Shedding Stratospheric Air, Geophys. Res. Lett., 41, 7785-7790, doi:10.1002/2014GL061921.

Reply: We included this reference into the introduction.

In addition, there are a handful of modeling studies examining stratosphere-totroposphere transport that could be cited: Gray, S. L., 2003: A case study of stratosphere to troposphere transport: The role of convective transport and the sensitivity to model resolution, J. Geophys. Res., 108, D18, 4590, doi:10.1029/2002JD003317. Chagnon, J. M. and S. L. Gray, 2007: Stratosphere-troposphere transport in a numerical simulation of midlatitude convection, J. Geophys. Res., 112, D06314, doi:10.1029/2006JD007265. Chagnon, J. M. and S. L. Gray, 2010: A comparison of stratosphere-troposphere transport in convection-permitting and convectionparameterizing simulations of three mesoscale convective systems, J. Geophys. Res., 115, D24318, doi:10.1029/2010JD014421.

Reply: The aim of our manuscript is to show the transport processes in the tropics. The suggested papers here all study transport in the midlatitudes, on which generally stratosphere-troposphere transport papers focus. Thus, we decided to add only the most recent paper.

Page 1045, line 16: Suggest replacing “intensive” with “intense”

Reply: We did as suggested.

Page 1046, lines 17-20: Please be specific, what are “typical” mixing ratios and how much larger is the elevated feature?

Reply: “Typical” here refers to the shape of the tracer profiles, which show clear deviations in our case. We rephrased: “Clearly elevated ozone mixing ratios and decreased CO mixing ratios relative to those expected in typical-shaped profiles are seen."

Additionally, we added: "The deviations of the observed median profile from the expected typical-shaped profile are about +50 ppbv for ozone and -10 ppbv for CO."

Page 1049, lines 12-13: Please clarify that this is "column-maximum" radar reflectivity here and in the figure caption. Also, how is reflectivity calculated? Are you using the built-in "do_radar_ref" option in WRF? If so, it should be outlined somewhere that equivalent horizontally polarized radar reflectivity for a 10-cm wavelength radar is computed based on that outlined in Morrison et al, 2009, where only Rayleigh scattering is accounted for. Citation: Morrison, H., et al, 2009: Impact of cloud microphysics on the development of trailing stratiform pre- cipitation in a simulated squall line: Comparison of one- and two-moment schemes, Mon. Weather Rev., 137, 991–1007, doi:10.1175/2008MWR2556.1.

Reply: We did change "maximum" to "column-maximum" reflectivity as suggested. The employed NSSL microphysics scheme has its own inbuild computation of radar reflectivity, which basically follows Ferrier (1994, J. Atmos. Sci) for the equivalent melted drop (Mansell, personal communication, 2015). It assumes pure Rayleigh scattering.

We added a footnote to the text: "The radar reflectivity is calculated by the NSSL microphysics scheme following Ferrier (1994), assuming pure Rayleigh scattering."

Figure 4: The text size in this figure is small and difficult to read. Please increase.

Reply: We did as suggested.

References:

Fueglistaler, S., Dessler, A. E., Dunkerton, T. J., Folkins, I., Fu, Q., and Mote, P. W.: Tropical tropopause layer, *Rev. Geophys.*, 47, RG1004, doi:10.1029/2008RG000267, 2009

Ferrier, B. S.: Double-Moment Multiple-Phase Four-Class Bulk Ice Scheme. Part I: Description, *J. Atmos. Sci.*, 51, 249-280, 1994