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

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.
by Anonymous Referee #2

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 addresses the important question of vertical transport across the tropical tropopause layer (TTL) in tropical deep convection. This is done at the local scale and the authors use a single tropical storm observed on the 30th of November over the Tiwi Islands as a case study for their high resolution model integrations. Overall the model reproduces many of the storm features observed.
Aircraft measurements of O3 and CO are compared to similarly initialised passive tracers in the model simulations and additional idealised tracers are also used to infer upward/downward transport within the convective cloud. The authors have also investigated how changes in water vapour are affected by different processes in the convective system. Overall, the study includes some novel aspects (vertical transport in convective updraft and downdraft) and is clearly structured and well written.

Major point:
The authors should use 'appropriate' scales for the inert tracers plots (Fig 6, Fig7, panels b and d in Fig 8 and 9) and the IWC plots (Fig 5). I think the scale used for plotting a quantity should reflect a significance range interval for the quantity being plotted.
For IWC, convective clouds have a typical IWC of ~1g/m3 at the core while values of 1e-4 g/m3 are generally associated to thin and sub-visible cirrus clouds. The current scale extends to 1e-5 g/m3. Similarly, when looking at inert tracers initialised in a specific layer, the main question the plots are trying to answer is "to which height is a 'significant' fraction of this tracer being transported due to convection?".
Having a scale that extends to very small values is misleading. The current plots show which height an amount of tracers which is respectively 5 (Fig 6, 8, 9) or 15 (Fig 7) orders of magnitude smaller than the initial tracer concentration can be moved by convection. In my opinion, a 'significant' amount of tracer would be 5% to 1% relative to the initial tracer concentration; given the strong vertical gradients of some chemical species around the tropopause an amount as small as 0.1% of the initial concentration might still make a small difference. However, I find it hard to justify plotting anything smaller than 0.1% of the initial tracer concentration (this corresponds to scales down to 1e-3). Plots with smaller scales can be misleading as they show transport of quantities that are so small they are not significant therefore they don't help in trying to explain observed changes in e.g. O3 and CO. It would also help if all scales used for inert tracers were the same (currently plots of the T and A tracers use different scales).
Additionally, sentences in the texts which are currently vague or misleading as a result of the scales used for plotting should be corrected. For example: sentence starting on page 1049, line 27; sentence starting on page 1051, line 26; page 1051, line 6 (note about different scales); page 1053, line 8.

Reply to major points:
IWC:
You are right, that IWCs in the deep convective cloud cores reach those high values. However, as observations have shown, IWCs in the overshoots are much lower. De Reus et al., 2009, Table 1, show that the average IWC (and that means that smaller values are found as well) range between 7.7e-5g/m^3 – 1.3e-3g/m^3. Since these overshoots are major

components of our study, we need to also plot IWCs of down to 1e-5g/m^3 in magnitude. Additionally, the layer of low IWCs below the cold point tropopause is important to understand/explain the simulated dehydration layer in Fig. 13/14.

Tracers:

The first aim of Figure 6 is to show where air masses are transported, to make clear the effect of the convection. Thus, it does not show significant transport, but transport in general.

Figure 7 shows domain averages, where a small amount of tracer might still be meaningful, since the averaging includes large areas where no tracer transport happened. However, these areas affect the value of the average significantly. That is also why the scale changes for the in-cloud tracers, which do not include convectively unaffected areas and accordingly amounts are much higher.

To better guide the reader in context of significance, we decided to add dash-dotted lines in Figure 8 and 9 to show the 1% and 0.1% thresholds.

Minor points:

a)

A previous study using cloud resolving model simulations to investigate vertical convective transport of chemical species (including ozone and CO) has been published in the literature and should be mentioned in the introduction (see Barth et al., Cloud-scale model intercomparison of chemical constituent transport in deep convection, Atmos. Chem. Phys., 7, 4709–4731, 2007)

Reply: We added this publication to the introduction.

b)

Although overshooting convection has been observed (Corti et al. 2008, De Reus et al. 2009) the relative impact of these very localised storms at the global scale has not been fully quantified and could still be negligible if the horizontal extent and overall number of such penetrating storm is small. This should be pointed out and a caveat added in the Introduction (for example following sentence on page 1044, line 13-15).

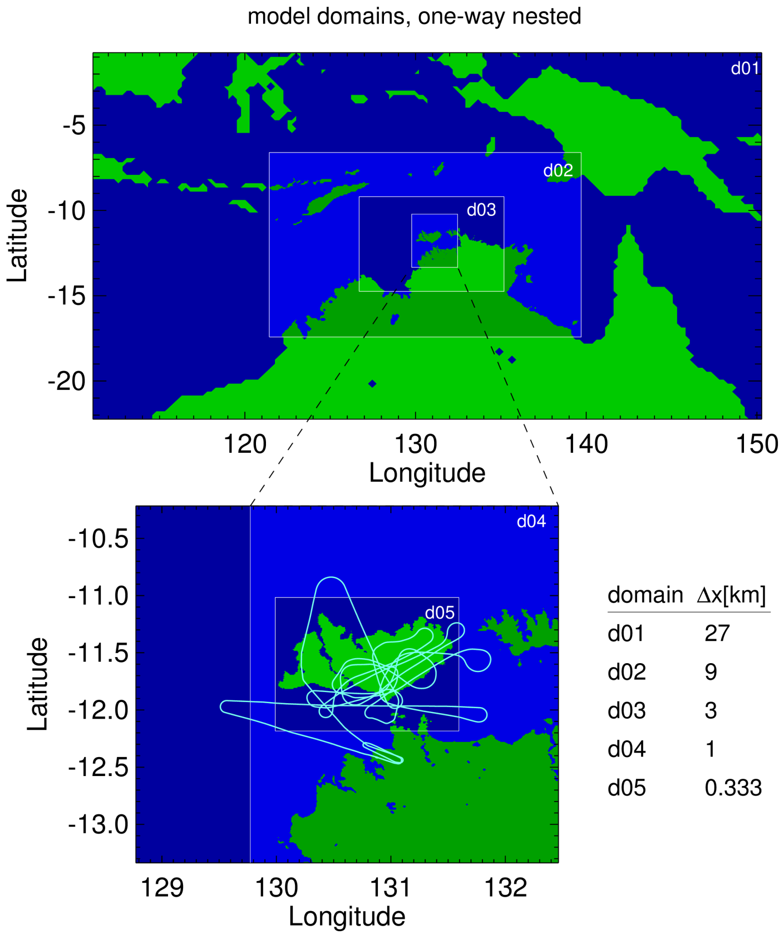
Reply: We added the following text:

“At present it is unclear what the relative impact of these localised storms are on the global scale, though observational campaigns during the early years of this millennium demonstrated a high frequency of overshooting events (Pommereau, 2010), which contrast the generally assumed scarcity of these events. A high resolution climatology of extend and number of overshooting convection events would be needed to fully quantify their impact.”

c)

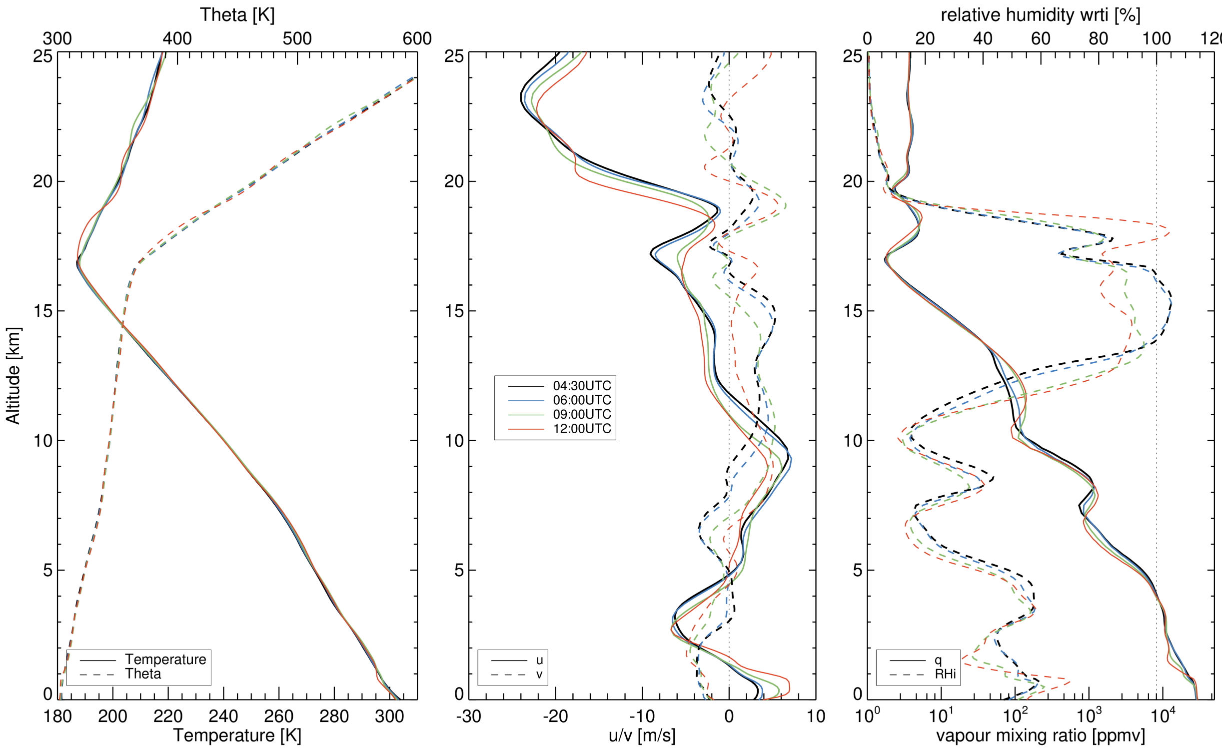
Figure 2: the light blue Geophisica flight track is hardly visible on the green and dark blue background. I suggest using a different colour and thicker line or adding it in a figure inset showing a zoomed-in version of d04. At the moment this confuses the picture without adding much extra information.

Reply: We changed the figure according to your comment, please find the new version below:



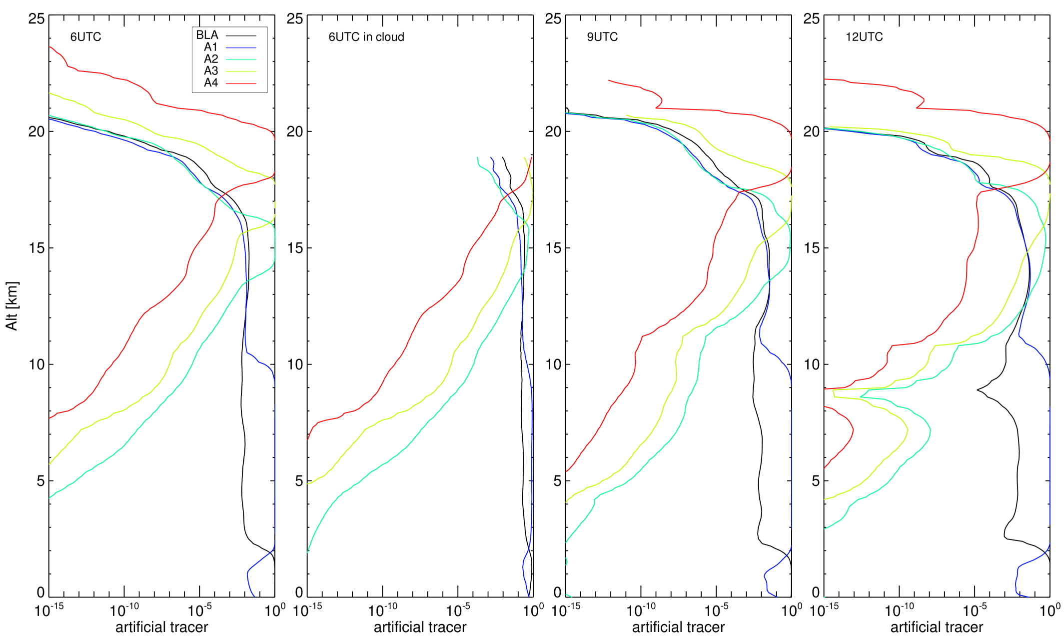
d)
Figure 3: it would be useful to add extra panels, or extra lines in the existing panels, or an extra figure to compare these quantities (shown prior to Hector) with the same during and after Hector (say at 6 and 12UTC). In particular, the height of the tropopause (panel a) is critical to address the extent of modelled cross-tropopause transport and at the moment it is not clear how this changes in response to convection in the model.

Reply: Find the figure with new times (6, 9, 12UTC) included below. As you can see, there are no substantial changes in the temperature profiles. Therefore, we think it could be sufficient to mention this in the text but not to plot in the Figure, to avoid cluttering and confusing the figure too much. Furthermore, the cold point tropopause is shown in Figure 5 and 6 as dotted line. We added the following at the beginning of Section 4: “The height of the simulated cold point tropopause changes to 16.8km/16.8km/17.1km at 06:00UTC/09:00UTC/12:00UTC.”



e)
Figure 6: it would be beneficial to add an extra column for 9UTC. This would be more consistent with Figure 10 and also illustrate the point made in the text about BLA, A1 and A2 reaching highest at 9UTC (page 1051, line 21-22).

<u>Reply:</u> We added a column with cross sections for 9UTC into Figure 6. The point about tracers reaching higher into the stratosphere was based on the following profiles, where small amounts reach up to 21km. As mentioned in the reply to your major comment, in a domain average a small amount might still be meaningful, since the averaging includes large areas where no tracer transport happened, but these areas affect the value of the average significantly. That is also why the scale changes for the in-cloud tracers, where amounts are much higher.



To address your comment about scales we changed the text accordingly: " Small amounts of BLA, A1, and A2 even reach up to about 21 km at 09:00UTC and fall back to 20km at 12:00UTC (at scales smaller than plotted here)."

f)

Fig 15: at the moment it appears that two different figures are labelled as Fig 15 (one with no caption). This should be corrected.

Reply: These two figures are indeed one figure, we just decided to split them for better visibility in the ACPD format. The captions were typeset according to the ACPD rules (discussed with the typesetter). However, in a final revised version this figure will be put together as one again.

References:

de Reus, M., Borrmann, S., Bansemer, A., Heymsfield, A. J., Weigel, R., Schiller, C., Mitev, V., Frey, W., Kunkel, D., Kürten, A., Curtius, J., Sitnikov, N. M., Ulanovsky, A., and Ravegnani, F.: Evidence for ice particles in the tropical stratosphere from in-situ measurements, *Atmos. Chem. Phys.*, 9, 6775–6792, doi:10.5194/acp-9-6775-2009, 2009.

Pommereau, J. P.: Troposphere-to-stratosphere transport in the tropics, *CR Geosci.*, 342, 331–338, doi:10.1016/j.crte.2009.10.015, 2010.

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 #3

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.

<u>General comments:</u>

...

<u>Specific Comments:</u>

page 1050, line 29: "... the Thompson scheme produced the smallest Hector." This sensitivity discussion is great, as information on sensitivities is helpful to the community. So, I don't find anything wrong with your discussion, but I recommend some added clarification. First, what do you mean by "smallest"? I infer from the following sentence that "smaller" refers to the depth and magnitude of the convective turrets. Are there additional "smaller" traits, or is that the extent of what you meant to convey? You also mention that NSSL performed "slightly better" than the Morrison scheme. Can you be more specific about what fields in particular were "better"?

<u>Reply:</u> For the comparison we used the ice content of the simulated clouds to infer the development and vertical extension of the clouds, as well as the extent of the anvil. Furthermore, we were mainly comparing maximum altitude reached by the cloud turrets and timing of the convective cells/overshoots, which we compared to aircraft and CPOL radar observations. All comparisons were based on simulated storms in domain 4; domain 5 was added only after conclusion of this comparison, and only run for the NSSL microphysics scheme due to the computational cost.

While the maximum altitude reached by the simulated Thompson cloud was about 17km, Morrison reached about 18.5km, and NSSL about 19.5km (cf. aircraft measurements between 18-18.7km - not necessarily reaching to the top of convection - and CPOL radar 19km). Also in the horizontal, the Thompson scheme achieves the smallest spatial extent, followed by Morrison, and the largest diameter is reached by the NSSL scheme.

Timing: While the Thompson and Morrison storms started developing at about the same time, the Morrison clouds reach higher at an earlier time, and on the other end dissipate less quickly than the Thompson clouds. The NSSL scheme achieved an even better timing, starting convection 30min earlier than the Morrison scheme. This scheme achieves the best agreement respective timing compared to the timeframe given by the CPOL observations.

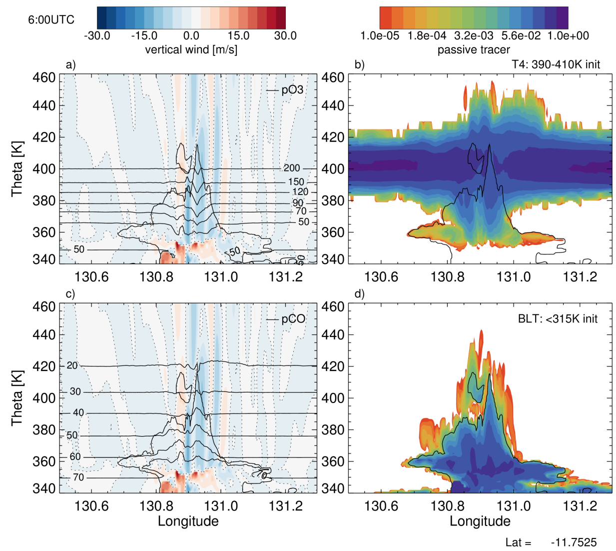
Actually, we also tested the WDM6 scheme, which however did simulate clouds that only reached about 10km altitude, which is why we did not mention this. Following your comment we decided it could be worth mentioning this as well in the revised manuscript version.

Thus, we rephrased the text as follows:

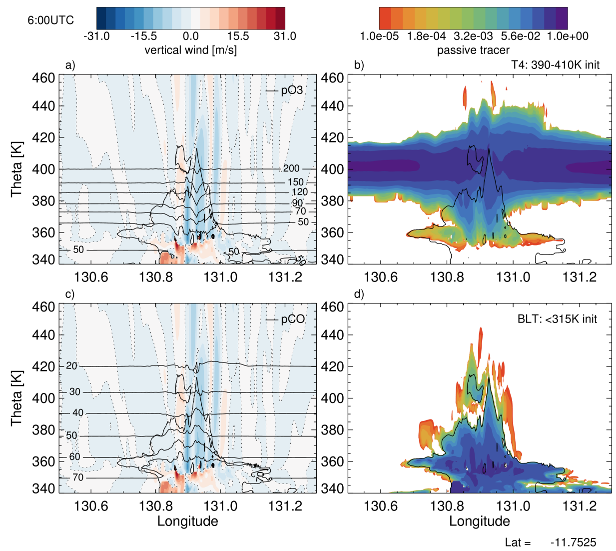
"In addition to the NSSL microphysics, the WDM6, Thompson, and Morrison two-moment schemes (Lim and Hong, 2010; Thompson et al., 2008; Morrison et al., 2009) were tested. While Thompson, Morrison, and NSSL all simulated Hectors with clouds reaching high into the TTL, convection in the WDM6 scheme just reached up to 10km. From the remaining three, the Thompson scheme produced the smallest Hector in horizontal and vertical extension. Morrison and NSSL simulated higher vertical wind speeds (about 8–10ms^{-1} higher) and higher reaching turrets, also producing overshooting into the stratosphere, as observed. The NSSL scheme achieved slightly larger horizontal and vertical extent than the Morrison scheme. While the timing is similar for the Thompson and Morrison schemes, the NSSL scheme showed a better timing by about half an hour. Thus, overall, the NSSL scheme performed best, and was chosen for the rest of the study."

Figures 7-11, 13-14: As stated in "General Comments", I think you've done a great job with the figures in this article, but I need some clarification on the figures that use potential temperature as an axis. Some sort of interpolation to potential temperature surface would need to be performed, as the potential temperature surfaces are not planar when the storm is active (figure 12). You need to explain how this was done, as the method of interpolation would impact your results. Also, gravity waves, particularly near the overshooting tops, often cause near vertical isentropes (figure 12). How did you deal with these vertical isentropes when converting to potential temperature coordinates?

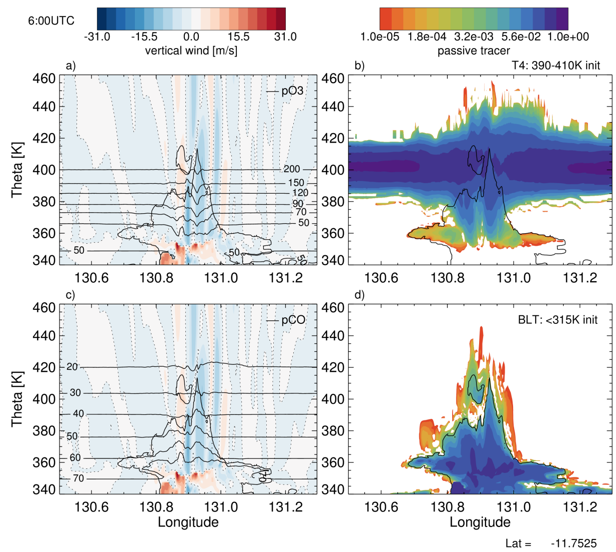
Reply: To regrid data onto the potential temperature, we did vertical interpolation for each lot-lan grid point column onto a regular potential temperature ordinate. The plots in the manuscript use linear interpolation, though using other interpolations does not make a major difference. Below we show a few examples based on Fig. 9 (i.e. at a time where almost vertical isentropes occurred) using different interpolation methods. It shows that the basic conclusions remain unchanged.



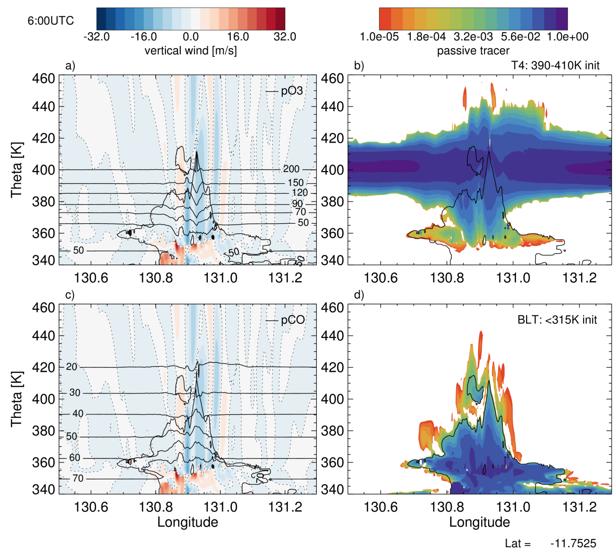
original/linear



quadratic



lsquadratic



spline

At near vertical isentropes the interpolation might lead to smoothing of the gradients, thus, the real changes may actually be larger than displayed in the plots. We added a footnote to the reference to Fig. 7: "Linear interpolation has been applied in the vertical to regrid data onto a regular potential temperature ordinate (Fig. 7-11, 13, 14). Care should be taken interpreting the figures when isentropes are near vertical. However, different interpolation methods lead to almost identical results."

Figure 11: Are you able to show any later times? The storm is still active at 6:00, and this figure shows there are still some changes in the tracer perturbation fields from 6:30 to 7:00. At what time is the transport profile fixed? I.e., at what time are there no longer parcels with positive/negative buoyancy?

Reply: From inspection of the cloud development, we infer that convection becomes inactive at around 7:00UTC. We do include 7:30UTC in Figure 11 now: Some further changes are still obvious, however, these are presumably caused by other processes as horizontal advection.

We add the following text: "Convection becomes inactive at around 07:00UTC, however, some further changes to the in-cloud tracer profiles may occur, presumably due to horizontal advection or other lateral inmixing processes not related to convection."

page 1062, lines 25-26: The sentence that begins with "However, this moistening …" reads very oddly. Having a "however" and a "but" in the same sentence left me confused about what you were trying to say here.

Reply: Replaced "but" with "instead".

Technical Corrections:

page 1045, line 24: "...cloud turrets were performed, which..."

page 1048, line 6: I think there is an extra "the" in this sentence. "Therefore, 3 arc-seconds..."

page 1054, line 18: "...model identifies mixing, Fig. 12 shows..."

page 1066, line 11: "...altitude of the layer..."?

page 1066,line 12: "...can actually lead to both hydration and..." (no comma)

Reply to Technical Corrections:

We applied all corrections as suggested.

References:

Lim, K. S. S. and Hong, S. Y.: Development of an Effective Double-Moment Cloud Microphysics Scheme with Prognostic Cloud Condensation Nuclei (CCN) for Weather and Climate Models, *Mon. Weather Rev.*, 138, 1587-1612, 2010

Morrison, H.; Thompson, G. & Tatarskii, V.: Impact of Cloud Microphysics on the Development of Trailing Stratiform Precipitation in a Simulated Squall Line: Comparison of One- and Two-Moment Schemes, *Mon. Weather Rev.*, 137, 991-1007, 2009

Thompson, G.; Field, P. R.; Rasmussen, R. M. & Hall, W. D.: Explicit Forecasts of Winter Precipitation Using an Improved Bulk Microphysics Scheme. Part II: Implementation of a New Snow Parameterization, *Mon. Weather Rev.*, 136, 5095-5115, 2008