Review of "Upward transport into and within the Asian monsoon anticyclone as inferred from StratoClim trace gas observations" by von Hobe et al.

Airborne in situ measurements of CO, O_3, and N_2O collected in the Asian summer monsoon (ASM) anticyclone and surrounding regions during the 2016 and 2017 StratoClim field campaigns are analyzed to elucidate troposphere-stratosphere transport pathways and mechanisms. The manuscript is well written, the figures are generally well done, and the supplementary information is helpful and appropriate. I do, however, have a number of comments on both the analysis and the description thereof that I feel should be addressed before the manuscript is accepted for publication.

Specific comments and questions (major substantive issues and minor points of clarification, wording suggestions, and grammar / typo corrections are listed together for each Section in sequential order through the manuscript):

General (throughout the manuscript)

- In many (perhaps most) cases, acronyms are not spelled out the first time they are used.
- In several places (e.g., L103, L116, L171), "dynamic" should be "dynamical".
- In several places (e.g., L288-289, L354, L374), "incidences" should be "occurrences".

Abstract

- L22-25: Are all of the values (both mixing ratios and potential temperatures) quoted in these lines fully consistent with those given in the main text? The tropospheric abundance of N_2O in particular departs from that stated in Section 3.2.
- L31-33: I find the wording of this sentence awkward. One suggestion would be to rewrite along these lines: "For the key tracers (CO, O_3, and N_2O) in our study, none of which are subject to microphysical processes, neither the lapse rate tropopause (LRT) around 380 K nor the cold point tropopause (CPT) around 390 K marks a strong discontinuity in their profiles."
- L33: It seems a bit odd to focus on the CPT here, when most of the results in the paper are described relative to the LRT (and H_2O is not one of the measurements discussed).

Introduction

- L73: "uprising" (which means "revolt" or "rebellion") is not the right word here; I suggest "lofting".

Section 2

- Figure 1: I find the description of this figure and its relation to Fig. S4 confusing and the related discussion in the text (one sentence, L113-114) inadequate.
    - Although the color bar label indicates that the contour plots show PV at 380 K, that information should be stated in the figure caption itself. That 380 K is the only isentropic surface on which the method of Ploeger et al. [2015] can be applied is acknowledged later in the manuscript (L206-207), but many readers may not appreciate that limitation when Fig. 1 is introduced.

- The 380 K level is difficult to distinguish in the theta color bar used for the flight tracks. The color palette should be constructed to facilitate identification of the portions of the flights at or near 380 K (for which the defined anticyclone boundary is pertinent). Much of the flight time in both deployments took place at levels well above or below 380 K.
- The overlaid yellow lines in Fig. 1 do not closely resemble the cyan contours in Fig. S4 (especially for the 2016 period), nor do the average PV values quoted in their respective captions match. Is that because Fig. 1 shows the averages over the specific campaign phases, whereas Fig. S4 shows seasonal averages? This point should be clarified.

- L138: Add a comma after “flight”.
- L139: The semicolons in this line should be commas.
- L141: of ozone --> of an ozone
- L146: instrument operated by the University of Wuppertal comprises --> instrument, operated by the University of Wuppertal, comprises
- L151: Delete the comma after “Kalamata”.
- L154: Dlugokendky --> Dlugokencky
- L157: a N_2O --> an N_2O
- L167: Add “and” before the last item in the lists of both pressure and altitude levels.
- L178: the reanalysis grid points around --> the surrounding reanalysis grid points
- L181: material that --> material, which
- L192-193: Quite a number of authors (beyond the short list given here) have used GPH to define the ASM anticyclone boundary; a similar comment can be made about the use of PV, and more than one paper has used MSF as well. Thus, it would be best to add “e.g.” in all of these cases.
- L210-211: The discussion of the limitations of the approach used in this study to segregate measurements made inside and outside the anticyclone should explicitly note that the anticyclone varies in size at different levels, and also that it tilts northward with altitude. So while it may be the best that the authors can do, I feel that they are a little too cavalier in dismissing the impact that their approach might have on the interpretation of their results. They state that the focus of this analysis is on the tropopause level near 380 K, but that is not an entirely true statement – in particular, Fig. 4 (based on inside-anticyclone points) spans the domain 310–480 K, and quite a bit of discussion is devoted to the stratosphere above the 400 K level. I do not share their optimism that the inherent ambiguity introduced by their PV-based approach to identifying the anticyclone boundary necessarily has little impact.
- L225-229: I feel that more should be said about the ability of CESM2/WACCM6-SD to faithfully reproduce the observed confinement of trace gases within the ASM anticyclone. Two recent papers [Orbe et al., GRL 2017; ACP, 2020] show the sensitivity of both convection and large-scale circulation to the details of how nudging is implemented. My understanding is that CESM2 includes substantial changes to the nudging scheme and convective parameterizations from CESM1. Has any previously published study demonstrated the fidelity of the model’s depiction of the ASM anticyclone for the specific configuration used here? If so, it should be cited. If not, then it would be appropriate to elaborate further on that issue in this paper.

Section 3

- L239-240: Since the Stroh et al. StratoClim overview manuscript is still in preparation, it might be good to add a reference to Bucci et al. [2020] for the point that convection strengthened in the latter part of the Katmandu deployment. The paper by Bucci et al., which has been accepted for publication in ACP, is already being cited elsewhere.
- Fig. 2 and associated discussion in L247-248:
  - Fig. 2 nicely conveys a lot of information in a compact form that facilitates comparison of the two data sets. However, the labels on the CO color bar are too small to easily read without substantial enlargement, and the color palette makes it difficult to distinguish between different abundances.
  - It is stated that "small CO mixing ratios in the 10 to 20 ppb range" were measured during the Kalamata flights. Although it is hard to judge, I do not really see any indication of CO values below about 35–40 ppb, and certainly none as low as 10–20 ppb.
- L248: These air masses --> Such air masses
- L252: in the latitude --> in latitude
- L253: Just to be really clear, I suggest adding "(higher-latitude)" in front of "Kalamata".
- L255: as new --> as the new
- L258: Section 2.4.2 --> Section 2.3.2
- L264-266: This sentence states that at and just above the LRT, O_3 decreases and N_2O increases with increasing MeqLat (i.e., "toward the centre of the ASMA"). But to my eye, both appear to change little; if anything, O_3 actually increases slightly with MeqLat, whereas N_2O shows only a very slight increase that is barely perceptible in the color scheme used.
- Fig. 3 and its caption:
  - The x-axis labels should be "MeqLat" to match the text.
  - The dotted line marking 65° MeqLat is not defined in the caption. Moreover, this line is barely visible; a solid (or even a dashed) line would show up better.
  - L711: To ensure clarity, I suggest adding "from both deployments" or something similar in front of "averaged".
  - L716: Section 2.4.2 --> Section 2.3.2
- Fig. 4 and its caption:
  - Obviously, monitors vary, but on my screen the color of green used for the expected range information is too bright to show up well, making these lines hard to read.
  - The x-axis ranges for the panels showing the # of data points lie too close to the mixing ratio panels for both the same species to their left and those of their neighbors to the right, so the individual scales are hard to differentiate in some cases.
  - I assume that the mean, max, and min values of the LRT and CPT vary slightly between the columns because, as explained in the Fig. 3 caption, the bins with valid measurements are different for each instrument, but it would be good to clarify that explicitly in this caption.
  - Is there an explanation for the cluster of samples below 350 K (below –30 K in $\Delta\theta$) with much lower CO values (~40–70 ppb)? Some of those bins contain a fair number of points.
  - L721: Since the number of observations is partly obscured by the LRT/CPT ranges, it might be good to add something along the lines of "as the grey histogram" after "plotted".
  - L724: There is no Ploeger et al. [2010] reference – perhaps 2017 is meant?

- L725: The applied maximum lifetime of UTLS CO of 90 days seems somewhat on the long side to me. Based not only on Xiao et al. [2007], but also Duncan et al. [JGR, 2007] and Holloway et al. [JGR, 2000], I think that the total atmospheric lifetime of CO over Asian continental regions in summer is more like 1–2 months, not 2–3 months. So it is possible that the expected range for CO in this figure should be adjusted.
- L277: Since the fact that no CO mixing ratios exceeding 100 ppb are observed above the LRT is an important point, it might be helpful to add a vertical line on the CO panels in Fig. 4 marking that value.
- L284: In addition to the point that not all O_3 mixing ratios fall in the expected concentration range, it is also worth noting that much of the expected range is unpopulated in the data.
- L286: It is suggested that more rapid ascent may account for the apparently tropospheric character of some of the air masses observed to fall outside of the expected range for O_3, but wouldn't stronger ascent also affect the CO in those parcels, leading their measured CO concentrations to extend beyond the expected range (which is not seen)?
- L289: Delete "anymore".
- L298: For the reasons mentioned above, I think that this line should be amended to note that 3 months represents the upper end of the range for CO lifetime in the summertime UTLS.
- L301: For clarity, I think it would be good to start this sentence "Considering the upper end of the N_2O range and allowing for a 1 ppb increase between 2016 and 2017, …".
- Fig. 5 and its caption:
  - Although I have no objection if the authors prefer to leave Fig. S8 in the supplementary material, I note that there is plenty of room in the observational panel of Fig. 5 for a sizeable inset showing the O_3-CO relationship color-coded by N_2O. The theta color bar could be moved to the model panel or to the space above the two panels (which would also underscore that it pertains to both). In any case, it would help orient the reader to add the magenta box on the correlation plot in Fig. S8.
  - The grey dashed line in Fig. 5 representing the tropospheric regime is too pale to be easily seen. I think it would be better to make this a black dashed line as well and then add "vertical" and "horizontal" in the caption to distinguish the two black dashed lines.
- L315: campaigns --> flights (or campaigns --> campaign)
- L325-326: It would be appropriate to add tildes in front of all of these ranges, as is done in each panel of Fig. 6.
- Fig. 7: It seems odd to me that the colors used to represent the LRT and CPT have suddenly changed in this figure. It would provide more continuity for the reader to draw the LRT histogram in red and outline the CPT one in blue, in accordance with Figs. 2 and 4. (Of course, the zero line would also need to be colored differently in that case.)
- L328-330: As the authors note, some points lie as much as 45 K above the LRT, thus it is necessary to add "mainly" or some similar qualifier to "are found"; a similar comment can be made regarding the statement about the CPT.
- L331-333: The sentence "The points circled … NO_x." feels out of place, tacked on at the end of the paragraph. It might flow better at the end of the previous paragraph. Alternatively, although it is plausible that these points are related to lightning NO_x, the authors could do

more to back up that statement (e.g., through references to previous studies), in which case this discussion could make up its own short paragraph, probably at the end of the section.

- L341-345: It is a bit of an understatement to say that the modeled correlation is “somewhat” more compact than that observed – it is considerably more compact, not only in the transition region but also throughout the stratospheric regime. Couldn’t the measurement uncertainties (e.g., precision of 20 ppb for CO, 8% for O_3) account for some of this scatter?
- L344: of model’s --> of the model’s

Section 4

- L351-353: I find this discussion confusing. The authors state that their results provide clear evidence of vertical transport “to the tropopause”, but then they go on to note that the data show transport “up to at least 360 and often 370 K”, whereas they have shown that the mean LRT is at 380 K. Moreover, on L374 in the next section, it is stated that convective signatures are occasionally observed up to 380 K. This should be reconciled / clarified.
- L354: The wording in this line is awkward. If I have understood it correctly, then rather than “immediate convective outflow” it would be better to say something along the lines of: “convective outflow above the tropopause immediately prior to the measurement time”.
- L355: Here too I think the wording is a bit awkward and unclear. I suggest instead: “mixing with the local background following transport to this level, signatures of deep convection”.
- L358-359: It would be better to add commas after “correlations” and “(Section 3.3)”.
- L361-365: I would like to see more discussion here placing these conclusions into the context of other recent studies touching on this issue, including Ploeger et al. [2017], Vogel et al. [2019], Yan et al. [2019], and Legras & Bucci [2020] (and possibly others).
- L371: The year for the Bucci et al. reference should be [2020].
- L373: It would be more appropriate to say “the top of the *main* convective outflow layer”.
- Fig. 8 and its caption:
  - Again, it would be nice if the colors of the LRT and CPT overlays matched those in previous figures, even though that would mean choosing a new color palette for the contour plots.
  - L758: 30°E --> 30°N
  - L760: CRT --> CPT
  - The caption should explain the shading. One possibility would be to begin the sentence about how the zonal and meridional averages are calculated with “As demarcated by the vertical grey lines and bolder colors on the respective panels, …”.
- L382: Delete the comma after “(2020)”.
- L387-388: It would be appropriate to include a reference for the speed of the BDC.
- L388-391: It took me a couple of minutes to figure out that the statement that the observed CO decline and O_3 production point to upwelling that is somewhat slower than the ERA-I vertical velocities is based on the fact that the green lines in Fig. 4 were derived assuming an ascent rate of 0.3 K day^{-1} < $d\theta/dt$ < 0.8 K day^{-1}. Since this information appears only in the caption and on the figure itself (not in the main text), it would be good to remind readers of these values here. Moreover, it seems to me that this would also be an appropriate place to remind readers of the uncertainties associated with using PV at 380 K to identify inside-

anticyclone points, which is another reason that these upwelling estimates are not "quantitatively conclusive", as noted in L390.

- L398-399: As noted in L330-331, Fig. 7 shows the distance above the CPT to be 30 K.
- L399-403: That the tropopause over the ASM region does not represent a vertical transport barrier has been noted in previous studies, e.g., Vogel et al. [2019].
- L405-409: I find this whole paragraph confusing. For one thing, it's not clear whether the first sentence is meant to be a general statement or an expression of the findings of this study, but in any case it should be made clear that this picture has been described in many previous papers. In addition, I'm not sure why the WACCM simulations reported by Pan et al. [2016] are being discussed here, when simulations with a new (presumably improved) version of the model have been run specifically for this study. Can't these points be made with reference to the simulations shown in Fig. 5 and the animation in the supplement?
- L410: Since the analysis here sheds light only on in-mixing of stratospheric air into the anticyclone, whereas most previous studies quantifying the isolation of the anticyclone have focused on parcels escaping from it (and Legras & Bucci [2020] argue that the latter occurs more easily than the former), it might be better to entitle this subsection "In-mixing of stratospheric air".
- L413-417: It would be relevant to note here that the analysis of Vogel et al. [2019] showed transport of young air masses in the ASM circulation up to as high as ~460 K.
- L417-420: The study of Randel & Park [2006] (already cited elsewhere) should probably also be mentioned here, as they performed a trajectory-based quantification of confinement inside the anticyclone over the range 500–70 hPa.

Conclusions

- L427: LRT --> LRT around 380 K
- L430: times scales consistent with --> times scales largely consistent with
- L434: The term "tropospheric bubble" was not italicized in L403; usage should be consistent.
- L438-440: These lines are somewhat redundant with the two previous bullet points. In addition, the fact that the degree of mixing with surrounding stratospheric air increases at higher levels was noted by Vogel et al. [2019] as well. Finally, the other bullets point to the specific relevant figures, and it would be good if this one did too.
- L443-444: Unlike Ploeger et al. [2017], Legras & Bucci [2020] found that the "blower" mechanism operates at and above ~360 K, not just above the tropopause. (The studies of Vogel et al. [2019] and Yan et al. [2019] also found substantial horizontal flow out of the anticyclone between 340 and 380 K.) Legras & Bucci further found that localized "chimney"-like behavior ends at the cloud tops, above which ascent follows a broad spiral over the entire anticyclone domain, as shown by Vogel et al. I think it would be beneficial for the authors to put their results into the context of other recent studies in a more detailed and nuanced manner, rather than simply stating that they are "largely consistent".
- L445-449: The importance of extreme deep convective clouds in moistening the ASM region is also emphasized by Ueyama et al. [2018], and Legras & Bucci [2020] also found that penetrative convection may have a significant impact.

References

- L490: The paper by Bucci et al. has now been accepted and is in press.
- L570: The paper by Legras & Bucci has now been published.
- L670: hte Comission --> the Commission

Supplementary Material

- L771: CRT --> CPT
- L789: on --> in
- L803: Figure 3 --> Figure 4