Review of "Pollution trace gas distributions and their transport in the Asian monsoon upper troposphere and lowermost stratosphere during the StratoClim campaign 2017" by Johansson et al.

This manuscript presents observations obtained by the GLORIA airborne imaging infrared limb sounder in the UTLS during the StratoClim field campaign, which investigated the 2017 Asian summer monsoon. Measurements of HNO_3, O_3, PAN, C_2H_2, and HCOOH are analyzed in detail. Two sets of back trajectory calculations using different models, each employing a novel scheme for detection of convective events, are used to identify source regions for the sampled air masses. GLORIA data are also used to evaluate the CAMS reanalysis and simulations from the EMAC chemistry climate model.

Overall, this is an interesting and valuable paper reporting on measurements from an important campaign, and I think it will be of interest to the broad community. Unfortunately, however, the manuscript is marred by many instances of unclear and awkward wording. This is not just a matter of style – the writing is confusing enough in places that the meaning the authors are trying to convey is obscured. Thus, in my opinion, the manuscript requires a substantial amount of "cleaning up" before it can be published, and I have compiled a rather large number of comments. In most cases my concerns can be allayed by simply correcting and clarifying the discussion, with few requiring additional analysis or other significant changes. In some places I have tried to offer suggestions to improve the clarity and readability of the text. But, although each point is perhaps minor when considered in isolation, in aggregate they add up to major revisions. In addition, although some minor wording and grammar corrections have been suggested below, the manuscript should be copy-edited to improve the English. In particular, many errors in the use of commas are present throughout the manuscript.

Specific comments and questions: both 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

Abstract

- P1, L3-4: with base in --> based in
- P1, L7: This sentence does not make sense: why is the word "instead" used? Longer than what? No timescales have yet been mentioned.
- P1, L8: This line is misleading, since NH_3 is not presented in this paper. I suggest adding "previously reported" in front of "maximum".
- P1, L9: transport to the measured pollution trace gas occurrences --> transport on the measured pollution trace gases
- P1, L13: OMI should be spelled out here.
- P1, L16-17: It is not clear to me what the assertion that the models reproduce the large-scale structures of the pollutant distributions "if the convective influence on the measured air masses is captured by the meteorological fields used by these simulations" is based on, since this study does nothing to demonstrate that the models capture convective influence well,

and in fact numerous prior studies have shown that they do not. Perhaps the large-scale trace gas distributions are controlled mainly by the large-scale circulation, which global models do simulate reasonably well.

- P1, L17: Both models do not have --> Neither model has
- P1, L20: to reproduce --> in reproducing

Introduction

- P2, L4: The manuscript by Basha et al. (2019) has been rejected and should not be cited.
- P2, L6: The Santee et al. (2017) citation in the Reference list is an abstract for a symposium presentation (on a topic unrelated to this work) and is clearly not the intended reference, which should be a 2017 paper on the ASM published in JGR-Atmospheres.
- P2, L6-7: I don't think it is quite fair to characterize the vertical and horizontal resolution and sampling of satellite limb sounders as "low". Their sampling is vastly better than that of ground-based or airborne sensors, and their vertical resolution is much better than that of nadir sounders. Also, "low" is not the most suitable qualifier for "sampling". I suggest "relatively coarse" instead.
- P2, L7-8: Some references for the sentence about airborne in situ measurements inside the AMA would be appropriate.
- P2, L10-23: This paragraph as a whole is rather disjoint, with multiple independent thoughts assembled together with no thread connecting them. The last two sentences in particular seem out of place and do not follow from previous lines, and it's not clear why the last one begins with "However". I suggest rewriting to improve the cohesion and flow.
- P2, L14-15: Numerous papers have touched on this topic between Singh (1987) and Höpfner et al. (2019), so "e.g." is needed here.
- P2, L24-25: This sentence is overly general – it should be made more clear that it specifically refers to the region of the ASM, not the entire upper troposphere.
- P2, L28: "these data" could be interpreted as referring to the entire StratoClim dataset from all instruments, so: these data --> the data reported here
- P2, L32: are --> have been

Section 2.1

- P3, L20: The Santee et al. (1998) paper, which focuses on PSCs, is not really the best reference for MLS observations of HNO3 in the UTLS. A more relevant paper to cite for this point would be Santee et al. (JGR-Atmospheres, 2011).
- P3, L24-25: as stratospheric --> as a stratospheric; "within" is not the right word in this context; PAN is not defined until P4
- P3, L30: This is a very abrupt transition to tropospheric ozone; it would be better to say something about background values of tropospheric ozone, and possibly its sources as well, before talking about the magnitude of enhancements.
- P4, L4-5: Numerous papers (some of which are referenced elsewhere in this manuscript) have discussed the low abundances of ozone inside the AMA, so it is not appropriate to cite only a single paper for this point; at the very least an "e.g." is needed here.

- P4, L10-11: This sentence is somewhat inaccurate. Ozone is typically low inside the AMA; the Park et al. papers cited here use low ozone abundances (along with enhanced CO) as a marker of tropospheric air trapped inside the AMA. Park et al. (and others) have used larger abundances of ozone as an indicator of the presence stratospheric air, but not "polluted air" as stated here. If the authors are referring to the findings of Gottschaldt et al. (2017), then that paper should be cited here. In addition: measurements of O_3 … is --> O_3 is.
- P4, section 2.1.3: Typical background abundances of PAN should be stated here, as they are in the respective subsections for HNO_3 and O_3. This information is given in Section 3, but for completeness it should appear here as well.
- P4, L17-18: It is stated that photolysis plays a minor role, but according to Fadnavis et al. (ACP, 2015), photolysis is the dominant loss process for PAN in the UTLS. In addition, 250 K is not a higher altitude than 298 K.
- P4, L21: The CRISTA acronym needs to be defined on first use.
- P4, section 2.1.4: It is even more critical to help readers by providing some idea of typical background values for acetylene since that information is not given in Section 3.
- P4, L27: This sentence is awkward. I suggest reordering as: "Acetylene or ethyne (C_2H_2), a product of biofuel and fossil fuel combustion and biomass burning, has maximum tropospheric mixing ratios of a few pptv."
- P4, L30: The ATMOS acronym needs to be defined on first use.
- P5, L5: estimated to --> estimated to be
- P5, L8: or --> and
- P5, L9: measurements --> measurements of

Section 2.2

- P5, L11: performed with basis in --> conducted from a base in
- P5, L13-14: Why is only a single research flight singled out for analysis in this study? Unless some explanation is given about why the data available from the other three flights are not considered, readers may draw their own inferences about their quality or consistency.
- P6, Table 1 caption: Used spectral regions --> Spectral regions used
- P6, L8: I don't think that this sentence is completely clear. I suggest instead: "The retrieval strategy used here differs from that of Johansson et al. mainly in the applied …"
- P6, L12: substituted by retrieving --> replaced by retrieval of

Section 2.3

- P7, L4: atmosphere --> atmospheric; interaction --> interactions
- P7, L9: The grid specification should be written in a manner consistent with that in L29.
- P7, L10-15: These sentences are poorly written and unclear. Was the extension of the MECCA model performed by the authors as part of this work, or by the EMAC team? Are the values quoted for the number of reactions, etc., for the "standard" MECCA submodel or the "extended" one? It would be clearer to say "two sensitivity simulations with emissions of NMVOC increased by 50% and 100%". Other minor wording suggestions: in contrast to --> beyond that of; with regard to a better --> to improve the; photolyses --> photolysis reactions. ECMWF should be defined here, not in the following paragraph.

- P7, section 2.3.1: How are emissions prescribed in the EMAC runs done for this study? This information seems just as critical to me as the details of the chemical submodel. In particular, if emissions were prescribed using RCP scenarios, which do not include specific events, such as major fires in any given year, then even specified-dynamics EMAC simulations cannot be expected to replicate the observations closely.
- P7, section 2.3.2: Similarly, information about the emissions in CAMS also needs to be given.
- P7, L23-24: This sentence mentions a study evaluating the CAMS chemical reanalysis using aircraft measurements but provides no information about the results of those comparisons. Did Wang et al. (2020) find that CAMS fields match the measured species well or not? What are the implications for this work? In addition, the paper by Wang et al. has now been published, so the reference needs to be updated.
- P8, L9-10: ECMWF has previously been defined. Is this 3-h ERA5 product different from the one mentioned on P7, L28 with 1-h temporal resolution?
- P8, L16: The paper by Wohltmann et al. (2019) has now been published, so the reference should be updated.
- P8, L22-26: The investigation described in these sentences is interesting, but the results reported here are vague and their implications for this study are unclear (and the last sentence in this paragraph could also be better composed). What exactly is meant by "major differences" and "minor influences"? This discussion should be more quantitative. Do the findings from these ATLAS and TRACZILLA tests have any implications for the results from EMAC, since those runs were driven with ERA-I?
- P8, section 2.3.5: It is not appropriate to include the discussion of OMI tropospheric column NO_2 as part of Section 2.3, which is entitled "Atmospheric model simulation". Perhaps it should be in its own subsection. Alternatively, perhaps it could go in Section 2.1, "Measured trace gases". That section contains a general description of the species measured by GLORIA and analyzed in this study, but it could be slightly restructured to include the OMI NO_2 data.
- P8, section 2.3.5: It is necessary to provide information on the quality and resolution of the OMI tropospheric column NO_2 data, as well as a suitable reference for this specific product (beyond the general OMI instrument paper and the Krotkov (2013) citation, which is just for the L3 files and which is also incomplete).
- P8, L28: delete "instrument"
- P8, L30: troposheric --> tropospheric

Section 3

- P9, L4: Actually, HNO_3 strongly increases a few km above the tropopause, starting at about 19 km.
- P9, L6-7: This wording is unclear. By "local enhancements up to 0.5 ppbv", do the authors mean that the measured mixing ratios approach 0.5 ppbv, or that they are 0.5 ppbv larger than the regional background values (it looks like the latter to me). Some of these enhancements appear to be located at altitudes higher than 16 km. In fact, the particular structure noted at 4:00 UTC is at more like 16.5 km.
- P9, L8: Why is the magenta box drawn so as to exclude the peak in this enhancement at 4:00 UTC, and also the higher values right at 16 km just before 4:15 UTC? If this enhanced

structure is of interest for further analysis, I would think that it would be desirable to encompass the region of its strongest signature.

- P9, L14-15: It would be helpful if the colored boxes on Figure 2 were also overlaid on Supplementary Figures 2, 4, 6, 8, and 10.
- P9, L18: A local --> A PAN local
- P9, L32: with VMRs --> with C_2H_2 VMRs
- P11, L1: of the --> on the
- P11, L3-4: as for all other gases than HNO_3, considerably large HCOOH of more than 200 pptv is --> as for all gases other than HNO_3 and O_3, considerably larger abundances of HCOOH of more than 200 pptv are
- P11, L5-9: I'm wondering why the authors have chosen not to highlight the region with the minimum in HCOOH where PAN and C_2H_2 are present in its own colored box. Considerable discussion is devoted to this part of the flight, possibly more than for some of the regions that are enclosed within boxes.
- P11, L10-11: This presence of PAN and C_2H_2 and the absence of HCOOH --> The presence of PAN and C_2H_2 together with the absence of HCOOH
- P11, L14-15: The period after "liquid" should be moved to after "2016)".
- P11, L19-20: The authors need to clarify that they are not talking about "all discussed gases" in these lines, as stated, but only the tropospheric tracers.
- P11, L27: which is --> as
- P11, L28: of the --> on the; but not in HCOOH suggests --> but not in HCOOH, suggest

Figure 2:

- It would be extremely helpful to the reader to: (1) enlarge the major tick marks on both x- and y-axes, (2) add minor tick marks, and (3) include tick marks on the right-hand y-axis and the top x-axis. Without them, it is very difficult to judge the values quoted in the text.
- The colored boxes on both the maps and the curtain plots are a little hard to see, as is the green line marking the tropopause. Perhaps it would help to make these lines a bit thicker.
- Caption: Specify that the green line is on the cross section plots. Delete ", which are".

Section 4

- P11, L33: estimate --> identify; high --> strong
- P12, L4: aides --> aids
- P12, L11: Although the overlaid boxes in Figure 3 facilitate comparison with Figure 2, the authors should consider adding an altitude scale on the right-hand y-axis of the panels as well. It would also be helpful to state the approximate pressure level corresponding to 15 km in this line.
- P12, L16: Along these trajectories, regions are bordered orange, where the density of convective events along these trajectories --> Regions are outlined in orange where the density of convective events along these trajectories
- P12, L19: I. e. the smallest bordered regions --> That is, the smallest outlined regions; 1.0% and 10.0% --> 1.0% or 10.0%

- P12, L16-19: My apologies, but I am missing something here. I don't quite understand how the densities of convective events discussed in this paragraph relate to the convection probabilities shown in Figure 3 and discussed in the previous paragraph (which are an order of magnitude larger). Please clarify the relationship between these two quantities.
- P12, L20: I'm confused here too – why would it necessarily be the case that "larger regions contain accordingly a larger fraction"? A large region encompassed by a single colored contour but no inner contours would still have convective densities between 0.1% and 1.0%, no matter its size. Unless an inner contour is present, the fraction does not reach 1.0%. In addition, I have looked closely at Figure 4, and I am not convinced that any of the outlined regions contain the innermost contour representing 10%, except for one orange region in the TRACZILLA panel. Perhaps the rarity of that occurrence should be pointed out.
- P14, L1: as average over 14 days --> as an average over the 14 days
- P14, L6: delete "from the measurements"
- P14, L7: since HNO_3 is not a pollutant, it would be best to delete "otherwise".
- P14, L8-9: The flow in this paragraph needs to be improved. The sentence about the small fraction of trajectories experiencing convection in the 5 days leading up to the measurement is ambiguous; it immediately follows a sentence on the magenta region and thus appears to be about that area, but in fact I think it is referring to the red region. This should be clarified.
- P14, L10-12: The writing in these lines is very unclear. Assuming that I have interpreted them correctly, I suggest instead: "For most regions marked red, only the 0.1% contours are present; thus convective influence along the trajectories was weak. However, most regions marked red in northeastern China lie close to areas with enhanced NO_2, so these regions may possibly have contributed to the measured enhanced pollution trace gases."
- P14, L13-14: Again, I am confused about how the 30% value quoted here for the red regions can be reconciled with the 1% contour outlining those regions in Figure 4. The sentence in these lines is quite unclear. I'm also confused about exactly what is being shown in Supplementary Figure 12. As I understand it, the trajectories are launched from the GLORIA measurement locations, which in many/most cases are not characterized by ongoing convection. However, although the caption to Figure S12 is unclear, particularly the description of panel (c), it seems to suggest that a convective event was occurring at the time the trajectories were launched, and that 30% of those back trajectories had experienced convection leading up to that point. Please clarify.
- P14, L15-16: The magenta box is not shown on Fig. 2i, j, nor was a minimum in HCOOH in this region discussed (P11, L1-17). If anything, HCOOH looks slightly high in that area. I assume that "close to the red maximum" is referring to the pollutant enhancements in the red box?
- P14, L16-17: These two sentences are poorly written, but if I understand their meaning correctly, then I think that it would be much clearer to say: "Both trajectory models show similar convective densities as for the red regions above China, and they also show substantial convective activity above the South China and Philippine Seas. TRACZILLA also indicates regions of strong convection northwest of the flight path."
- P14, L17-18: regions only the 0,1% --> regions, only the 0.1%; influence of --> influence on; also, eliminate one of the instances of "again" in this sentence

- P14, L19-20: This sentence is badly written and hard to read. I suggest instead: "However, in this case, it is likely that convection in the regions above the South China and Philippine Seas brought up clean maritime air." But perhaps I have not understood this sentence. I can see that convective transport of clean maritime air could produce a local minimum in the pollutants, but how could it have led to enhanced HNO_3 in this region?
- P14, L23: coast, even the 1%, and for TRACZILLA also the 10% lines --> coast, the 1%, and for TRACZILLA even the 10%, convective density lines
- P14, L24-25: orange regions --> regions marked orange; Southern Chinese --> South China
- P14, L27: 50% convective --> 50% of convective
- P14, L28-29: This sentence is unclear. More plausible than what? More likely than what?
- P14, L30: Since the "previous one" discussed was a maximum, not a minimum, it would be better to say "as the region outlined in orange".
- P14, L31-32: indicates regions between the flight path and the Bay of Bengal as source region --> indicate convective source regions between the flight path and the Bay of Bengal; Southern Chinese and Philippine Sea --> South China and Philippine Seas
- P14, L32: low NO_2 measurements --> low NO_2
- P15, L1: delete the comma after "flight" and "in the measurements".
- P15, L2: According to --> Based on; similar to --> similar to that of the
- P15, L3-4: Why would bringing up relatively pristine marine boundary layer air lead to a local enhancement in ozone? Also: west India and above the South Chinese and Philippine Sea --> eastern India and above the South China and Philippine Seas.
- P15, L6-7: I do not follow the logic here. The relevant sentence in Section 3 "suggests that these air masses are older than a few days (lifetime of HCOOH), but younger than 2 weeks (lifetime of C_2H_2)". How does that lead to the statement here that "convection 10 days before the measurement only had a minor influence" – that is, where does the value of 10 days come from? Perhaps the authors mean "convection any time in the last two weeks"? Also: influence to --> influence on.
- P15, L8-10: This statement is slightly inaccurate, so it would be better to be more precise with the language here: "… Fig. 3, which does not show strongly enhanced convection probabilities in the cross sections for either trajectory model. ATLAS and TRACZILLA both only show a small convective region over the Philippine Sea for the cyan region of interest, and ATLAS also shows convective activity above central China."
- P15, L11: less than 20% convective events of all trajectories occurred at all --> less than 20% of all trajectories experienced any convective events
- P15, L15: I'm not sure what the take-away message for the reader is. Does the fact that both models seem to identify source regions that are less "plausible" call into question the entire source attribution analysis? Are these regions really less plausible as source regions because they are characterized by low OMI tropospheric column NO_2? As mentioned in connection with Section 2.3.5, some discussion of the reliability and sensitivity of these OMI data is needed. Moreover, can it necessarily be assumed that tropospheric column NO_2 is a robust proxy that reflects *all* possible sources for these NMVOCs? In particular, according to Section 2.1.5, formic acid arises in part from biogenic emissions. Would those be captured in the NO_2 measurements? Some further discussion is warranted here.

Figures 3 and 4 and Supplementary Figure 12

- Figure 3 caption: pressures below --> pressures less than
- Figure 4 caption: origin of regions of interest --> origin of air masses in the regions of interest; for 10 days of which the temporal evolution fo the first 5 days are --> for 10 days, of which the temporal evolution for the first 5 days is
- Figure S12 caption: Fig. 6b --> Fig. 4b

Section 5

- P15, L25-26: The EMAC HNO3 mixing ratios at the tropopause look quite a bit smaller than 0.75 ppbv to me. In addition, the writing in this sentence is very awkward; I suggest rewriting as: "… flight; they decrease to values of 0.75 ppbv at the tropopause. Simulated maximum stratospheric values are not always as high as those measured, but they agree to within …."
- P15, L27-28: Delicate … repeated --> Fine-scale … reproduced
- P15, L29: are reproduced --> is reproduced
- P15, L31-P16, L2: The authors posit that the diagonal feature in the HNO_3 field simulated by EMAC may originate from reactions with NO_2, and the tone of the discussion seems to suggest that this may be a model artifact, especially in the latter portion of the flight. But they have made no attempt, here or in the previous section, to account for the similar feature seen in the GLORIA measurements in the first half of the flight. What is the explanation for the observed structure in HNO_3?
- P16, L1: reactions with NO_2, product of the photolysis of PAN. Too high values below the tropopause are also simulated towards --> reactions with NO_2, a product of the photolysis of PAN. Values that are too large are also simulated below the tropopause towards
- P16, L5: It would be appropriate to include a reference for the CAMS assimilation of O_3.
- P17, L1: to simulated --> in simulating
- P17, L5: which is not simulated --> neither of which are simulated
- P17, L6: to reproduce --> in reproducing
- P17, L11: To my eye, PAN values measured in the region outlined in cyan were not higher than 350 pptv, not 450 pptv as stated here. If this is meant to be a general statement (not specifically about the enhancement above the tropopause towards the end of the flight), then this sentence needs to be rewritten.
- P17, L12: which is considerably below --> considerably lower than
- P17, L15: In addition to pointing out that the EMAC C_2H_2 enhancement is in the same geolocation as the measured enhancement, it would be good to note that the simulated enhancement is much weaker and less extensive than the measured enhancement; it would also be helpful to add "(cyan box)" here.
- P17, L12-15: A point that is missing from the C_2H_2 discussion is the fact that EMAC completely fails to simulate the maxima in the red and orange boxes and the minimum in the magenta box, even in a relative sense.
- P17, L22: below --> of less than
- P17, L24: Even for HNO_3, the structure was correctly simulated by only one of the models.

- P17, L25-26: The authors state that their results indicate that the meteorological fields used to prescribe transport in the simulations do not include processes relevant for the observed situation. I presume that they are referring to deep convection, which is not resolved by the reanalyses, but that should be clarified. I am wondering, however, why this would be a factor only for the first part of the flight (which the sentence in question is about). According to Figure 3, as well as much of the discussion over the preceding pages of the manuscript, the second half of the flight was influenced by convection up to 150 hPa to a similar degree.
- P17, L30: to reproduce --> in reproducing
- P17, L32: within --> in

Section 6

- P18, L57: The writing in these sentences is clumsy. Moreover, I'm afraid that I don't follow the logic of the arguments. First, as mentioned in an earlier comment, both portions of the flight are characterized by high convection probabilities up to 150 hPa, so for that reason alone it doesn't make sense to focus only on the second half. Second, the authors appear to be saying that *because* the first part of the flight is strongly influenced by convection, the simulated results would not be affected by increased emissions. But that seems backwards to me – in the absence of convection, the strength of the surface emissions would be of little consequence. This discussion needs to be clarified.
- P18, L8: have comparable horizontal resolutions between --> obtain comparable horizontal resolution for both
- P18, L11: the emission scenario "+100%" --> the scenario with emissions increased by 100% ("+100%")
- P18, L13: E.g., --> For example,
- P19, L6-8: I'm not sure that it is true that GLORIA did not observe the slight enhancement in HCOOH at 6:00 UTC and 16 km. There may be a faint hint of this structure in the data. Perhaps this feature should have been introduced earlier in the discussion, e.g., P17, L16-22.
- P19, L10-14: Of course, although the increased emissions led to larger maximum values of PAN that matched the observed peak abundances better, they did nothing to improve the structure of the simulated field. I do not think that this is an unanticipated result. I would have expected background abundances of these tropospheric tracers to rise along with peak abundances in the increased-emissions scenario. So I am slightly puzzled by the discussion in these lines, which focuses on the impact of vertical resolution on the modeled fields. Its placement in this paragraph seems to imply that the smoothing effect of the coarser resolution of EMAC, which blunts peak abundances and blurs or erases fine-scale features, is somehow responsible for the background values of PAN being too high in this sensitivity test. In fact, I think that the resolution issue is just as relevant for the baseline model run, in which background abundances were also overestimated, and it would be more appropriate to move the discussion about it to Section 5.
- P19, L11-12: Anyhow, the tropospheric background values are modeled too high in both, the "+50%" and "+100%", simulation --> However, the tropospheric background values are substantially overestimated in both the "+50%" and the "+100%" simulations
- P19, L13: resolutions --> resolution

- P19, L14-15: reproduce on average and therefore smooths the finer resolved image --> reproduce on average, thereby smoothing the fine-scale structure
- P19, L15-16: with 100% increased NMVOC emissions --> with NMVOC emissions increased by 100%
- P19, L16: The possibility that model/measurement discrepancies may be partly attributable to emission sources not represented in the inventory used in these EMAC runs is mentioned. As I noted in connection with Section 2.3.1, which emission inventories were used in these simulations is a critical piece of information that has been omitted from the manuscript.
- P19, L18: That the meteorological reanalyses do not resolve local deep convection is a well-known issue that is presented here as a finding of this study. In addition, another aspect (besides the reanalyses) that does not appear to have been considered by the authors is the convective parameterization being used for these EMAC simulations. The choice of which convective parameterization is used has been shown to have a substantial impact on modeled trace gas distributions.
- P19, L18: indicates convective events that are not resolved in the meteorological fields that are prescribing dynamics --> indicates the occurrence of convective events that are not resolved in the meteorological fields used to prescribe dynamics

Figure 6

- Caption: EMAC with 50% (middle column), and EMAC with 100% increased NMVOC emissions (right column) distributions --> EMAC distributions with NMVOC emissions increased by 50% (middle column) and 100% (right column)

Conclusions

- P19, L23: In my opinion, the statement that this study discusses "the first measurements of HNO_3, O_3, PAN, C_2H_2, and HCOOH in the center of the AMA UTLS" is too broad. While that may be true for some species of the species listed, it is not true for all of them. This statement should be qualified in some way, e.g.: first airborne measurements, or first measurements by GLORIA.
- P19, L27-28: below 15 km with strongly enhanced pollution trace gases measured are linked to recent convective events as transport mechanism --> below 15 km in which strongly enhanced pollution trace gases were measured are linked to recent convective events as the transport mechanism
- P19, L29-30: This study is not the first to show that PAN is efficiently transported to the UTLS by deep convection, as is implied by the wording in these lines.
- P19, L31: our measurements --> the GLORIA measurements (this is especially important for readers who may focus just on the Conclusions, since the instrument has not yet been identified in this section).
- P20, L2: was --> were
- P20, L3: at the same air masses of --> in the same air masses as
- P20, L4-5: show for both species, HCOOH and NH_3 maxima at --> show maxima for both HCOOH and NH_3 at; this air masses --> these air masses; these species --> the two species

- P20, L6-8: Some of the discussion here is appearing for the first time in this manuscript. I do not think that it is appropriate to introduce new concepts in a section entitled “Conclusions”.
- P20, L16: indicate --> indicates
- P20, L23-25: As noted earlier, the fact that EMAC overestimates tropospheric background mixing ratios is not unique to the increased-emissions scenario – it was also the case for the baseline run, and increased emissions are expected to affect background as well as peak abundances. The same comment regarding vertical resolution applies here as well.
- P20, L29: course --> coarse
- P20, L30: delicate --> fine-scale
- P20, L31-34: These sentences are poorly written. “enhancements” are not transported upward – pollution is transported upward, leading to enhancements in the UTLS. Likewise, a “region” is not transported “around the tropopause” – the measured air masses in that region are transported. And I’m not sure what is meant by “around the tropopause”. Also: the origin of the measured species, which is likely to be caused by uncertainties of the --> the origin of the measured enhancements, likely because of uncertainties in the
- P20, L35: meteorological fields --> the meteorological fields used to drive the model
- P21, L1: estimation of origin for --> identification of source regions of