Atmospheric
Chemistry
and Physics
Discussions

Open Access

*Interactive comment on* "Space-Time Variability of UTLS Chemical Distribution in the Asian Summer Monsoon Viewed by Limb and Nadir Satellite Sensors" *by* Jiali Luo et al.

**Anonymous Referee #1**

Received and published: 26 July 2017

This manuscript investigates whether coarse vertical but high horizontal resolution measurements from two nadir-viewing sensors – IASI and OMI – can be used to characterize changes in UTLS composition in response to the Asian summer monsoon (ASM). Comparisons with Aura MLS limb sounding data are made to explore how well the nadir measurements represent enhancements in CO and depressions in O3 within the ASM anticyclone as well as the sub-seasonal variations therein. The manuscript is generally well organized and well written (although further copy-editing, beyond the minor points listed below, is needed before final publication) and could make a valuable contribution to the extensive ASM literature. However, in my opinion the manuscript has a significant shortcoming that needs to be addressed before I can recommend it

for publication: The authors have not done enough to demonstrate that IASI CO retrievals actually do resolve CO enhancements in the UTLS associated with the ASM. Certainly the data have some sensitivity in the upper troposphere. But the analysis presented in this manuscript has not gone far enough to paint a truly convincing picture that IASI has sufficient sensitivity in the upper troposphere to discriminate variations there from the equally large (or possibly even larger) variations in the middle and lower troposphere to which it has much greater sensitivity. I also have a number of specific comments and points of clarification (detailed below) that I would like to see addressed.

General comments:

* Figure 1b shows that nearly as much information is contributed to the IASI "12-16 km" product from 10 and 18 km as from 12-16 km. Moreover, there is a not insignificant degree of overlap with the averaging kernel for the so-called "0-12 km" partial column; in fact, the averaging kernel for the "0-12 km" partial column shows considerably more sensitivity in the upper troposphere than does the averaging kernel for the "12-16 km" partial column. In addition, non-negligible contributions to the "12-16 km" layer derive from 4-10 km. Thus it seems somewhat misleading to refer to the volume of air being sensed by IASI, which is at least 8-10 km thick, as the "12-16 km" CO value throughout the paper.

* The study by Barret et al. [2016] is cited to support the statement that the IASI CO retrieval captures UTLS variability at middle and tropical latitudes (P4, L2-4). But that study focused on examining large-scale monthly mean features, not sub-seasonal localized variations such as eddy shedding. Furthermore, Barret et al. note that the smoothing of GEOS-Chem model fields imposed by the IASI averaging kernels mixes high CO concentrations from near the surface into the middle and upper troposphere. Thus I feel that the authors need to do more to conclusively demonstrate the power of IASI CO measurements to distinguish upper tropospheric variations from those at lower levels. One approach might be to perform sensitivity tests with model output (e.g., from GEOS-Chem as in Barret et al., or from WACCM4 as in Pan et al. [2016], etc.) to

investigate the influence of the CO distributions in the lower and middle troposphere on the partial columns measured by IASI. Results from the "raw" model fields multiplied by the IASI averaging kernels could be compared to those derived from the same model output but with mid-tropospheric CO abundances reduced or enhanced by some fraction (say, 20%). Differences between the inferred "12-16 km" layer averages from such tests would help to gauge how much independent information IASI provides in the upper troposphere.

* Why was a comparison of the vertical resolution similar to that in Figure 1b not shown for MLS and OMI O3?

Specific substantive comments:

* P2, L16: I don't believe that Garny & Randel [2015] is the correct reference for this point. First, the citation for that paper is incorrect: it should be ACP 16, 2703-2718, 2016. Second, that transport pathways paper is not appropriate here: I'm guessing that the authors meant to cite Garny & Randel [2013] instead.

* P3, L2-3: Three main questions are articulated that this study is aimed at. The first two are clearly addressed in the manuscript. However, I am not sure that the third question – What can we learn from the complementary information from limb and nadir viewing instruments? – has actually been explicitly touched on. It would be good if the authors could add a sentence or two, probably in the Conclusions section, that circle back to this question to offer guidance on how the limb and nadir data sets could be applied synergistically to investigate specific science questions.

* P3, L22-23: According to the most recent MLS Data Quality Document [Livesey et al., 2017] (not [2015] as cited in the manuscript), the accuracy of MLS v4 O3 at 100 hPa is +0.005 + 7% ppmv, or ~20 ppbv for 200 ppbv of O3, not 50 ppbv. Although the along-track resolution is ~300 km for the O3 product at 100 hPa as stated, it is ~550-600 km for CO at 147 hPa.

* P4, L28-P5, L4: The formulation of Figure 1b is slightly confusing. Why are different x-axes used for the IASI and MLS averaging kernels? They could be compared more readily if plotted on the same scale. At the least, the zero lines for the two axes should be aligned (and perhaps a vertical line drawn through zero to guide the eye).

* P5, L1-4: First, according to Livesey et al. [2017], the full width at half maximum of the MLS CO averaging kernel (by which the vertical resolution is defined) is 5 km, as stated on P3, L21, not 6 km as given here. Second, it would be better to refer to this as "vertical resolution" rather than "vertical distribution".

* P5, L5-10: It is interesting that the authors chose to use the GFS operational analysis for this study, rather than one of the more commonly used analyses/reanalyses such as GEOS-5, MERRA or ERA-I. A sentence or two motivating that choice would be appropriate. This is especially true in light of the work of Nuetzel et al. [2016] showing that substantial bimodality in the ASM anticyclone, as asserted in previous studies, is apparent only in the NCEP-NCAR reanalysis and not in data from other modern assimilation systems.

* P5, L19: Park et al. [2007] note that confined tropospheric air masses are present in the ASM anticyclone up to 68 hPa.

* P5, L29: I understand the desire to employ different color bars for MLS and IASI CO. However, Figure 2 could give a false impression of the degree of agreement between the two fields to readers not paying close attention to the figure or its caption. Therefore the fact that the color scales are not the same should be mentioned in the main text as well as the figure caption.

* P6, L3-8: I agree that OMI data appear to represent UTLS O3 in the ASM region fairly well. However, the OMI JJA O3 field does not reflect the signature of tongues of extratropical stratospheric air being transported equatorward and westward around the edge of the anticyclone, which results in a ring of higher ozone surrounding the low values in the anticyclone center. This structure, which has been discussed in several

previous papers based on MLS and also MIPAS data, is readily apparent in Fig. 2c but is barely visible in Fig. 2d. I find this surprising given OMI's higher horizontal sampling/resolution and the fact that other smaller-scale features are seen in the map in Fig. 2d. On the other hand, such a signature of monsoon-induced stratosphere-to-troposphere transport is prominent in the daily maps of Fig. 11. Is its absence in Fig. 2d just a color scale issue (since Figs. 2 and 10/11 have different color bar increments)? Or does the disparity in the JJA means in Fig. 2 imply that MLS and OMI observe different seasonal evolution of this transport feature, some of which lies equatorward of 15N and thus may not be captured in Fig. 13?

* P6, L11: In these lines the "monsoon region" is defined as 0-50N, 0-150E, which is a rather broad area to label as being directly influenced by the ASM. I presume such an extensive region was used in order to encompass a range of values of both the tropospheric and the stratospheric tracer. If so, perhaps that should be explicitly noted, especially given that the ASM region is defined differently for different plots: on P4, L31 it is defined as 15-35N, 30-140E, and on P8, L12 as 15-35N, 0-150E.

* P8, L4-10: Again, it is not obvious to me that IASI has sufficient sensitivity in the upper troposphere to distinguish a "plume" of CO up to (and above, according to the figure) the tropopause that is clearly separable from the large abundances of CO in the lower atmosphere. The color bar in Figure 7 is strongly saturated at the high end. It would be better to adjust the color scale to allow some of the structure in enhanced CO abundances to become visible. That might reveal instances of localized enhancements in the upper troposphere that are not connected to the generally higher mixing ratios in the lower troposphere, providing more confidence that they are not simply a manifestation of contamination from below.

* P8, L33 – P9, L2: (1) The sentence "IASI CO data have a higher horizontal resolution and are able to detect the impacts of vertical transport in the troposphere" is somewhat confusing, because as written it seems to imply that the higher horizontal resolution enables the detection of the impacts of vertical transport. I think it would be better to more

clearly separate these two points. Perhaps something along these lines would work: "IASI CO data have higher horizontal resolution than MLS measurements. Despite its coarse vertical resolution, IASI is able to detect the impacts of vertical transport in the troposphere". (2) In my opinion the conclusion in the last sentence of this section that some of the finer scale structure evident in IASI CO data is attributable to eastward eddy shedding over the western Pacific is stated too definitively. That may be true to some extent, but as Figure 1b shows and the discussion on P7 makes clear, the IASI "12-16 km" layer average is substantially influenced by the mid-tropospheric CO distribution. In fact, the authors have basically said as much in the lines just above (P8, L19-21). Thus, unless the sensitivity analysis suggested earlier conclusively shows that such a statement is fully justified, I would like to see the wording in the last sentence of this section softened. The same comment applies to a similar sentence in the Abstract (P1, L23-25). (3) Although this work demonstrates the utility of IASI data for studying the evolution of CO over the ASM region, I think the essential bottom line is summed up on P8, L32-33: "MLS and IASI data have different advantages. MLS data are better for examining features with a shallow vertical extent, such as the ASM anticyclone, provided those features have a large enough horizontal scale." I feel that it would be appropriate to repeat this sentiment in the Conclusions section, and possibly in the Abstract as well.

* P9, L5-7: It might be good to remind readers here that, although UTLS O3 is mainly a tracer of stratospheric air as noted, its distribution can also be affected by photochemical production, as alluded to on P2, L30 of the Introduction. So the interpretation of O3 fields in the ASM region is not necessarily straightforward.

* P9, L19-21: "OMI data show a sharper transition of O3 field across the edge of the anticyclone (as indicted by the 105 hPa tropopause contour) ... differences between MLS and OMI are more pronounced at low latitudes". (1) Why is the tropopause being used to define the edge of the anticyclone here? Elsewhere GPH is used to denote the anticyclone boundary, not the tropopause. (2) How is the tropopause shown in

these figures being defined? I realize that it is taken from the GFS analysis, but is a thermal or dynamical (PV-based) definition being used? That should be clarified. (3) Differences between MLS and OMI may well be more pronounced at low latitudes, but it is difficult to tell from Figs. 10 and 11 since the color scale saturates at high latitudes.

* P10, L16: Although Garny & Randel [2013] did show that spatial variations in CO are well correlated with variations in the region of low PV defining the anticyclone, they did not find evidence of the kind of bimodality in the location of the anticyclone that Yan et al. did. So although it is certainly appropriate to cite Garny and Randel (among many others not listed) for highlighting the significant role of ASM dynamical variability in controlling UTLS tracer distributions, I don't think it is quite fair to include that reference for different "modes" of anticyclone behavior.

* P10, L24-26: In general, the Conclusions section overlooks the potentially significant contamination in the IASI CO "12-16 km" layer average from lower altitudes. Such influence from below is likely to be another factor explaining the apparent lack of consistency with MLS 147 hPa CO and should be acknowledged here.

* P11, L7: I'm intrigued by the notion that the results from this study might be used to refine the IASI or OMI retrievals. Could the authors say more about that, perhaps provide an illustrative example of how these findings could inform IASI or OMI retrieval algorithm development? Also, please clarify which "differences" are being referred to – differences from MLS?

Minor points of clarification, wording / figure suggestions, and grammar / typo corrections:

* P1, L15: The IASI and OMI acronyms should be spelled out here

* P 1, L17: "changes . . . is" –> "changes . . . are"

* P1, L19: "result shows" –> "results show"

* P1, L23: I suggest "captures" rather than "show[s]"

* P1, L31: As the first sentence of the paper notes, the ASM anticyclone has been investigated widely in recent years, but the small subset of references cited for this point seems somewhat arbitrary. Many more equally relevant papers could have been included, so it would be appropriate to add "e.g." at the front of the list.

* P2, L10: "i.e." –> "e.g."

* P2, L12: "in terms of"

* P2, L31: "Short time" –> "Short-term"

* P2, L33: "nadir view" –> "nadir viewing"

* P3, L5: "make" –> "makes"

* P3, L7-8: delete the second instance of "quantitative comparisons"

* P3, L13: "much" –> "more"

* P3, L13: "UTLS chemical tracers variability" –> "variability of UTLS chemical tracers"

* P3, L15: "aim" –> "aims" and "supplement" –> "supplements"

* P3, L16: "inform" is not quite the right word. Perhaps "examine", or something similar

* P3, L23: "and has" –> "with"

* P3, L31: "degrees of freedom signal" –> "degrees of freedom for signal"

* P4, L1-2: The MOZAIC and MOPITT acronyms should be spelled out; also add "and" before "satellite"

* P4, L7: Does Huang et al. [2016] really discuss the OMI O3 profile product? I believe this reference is incorrect.

* P4, L9: delete "and" before "zonal mean". Also, "NCEP" has not yet been defined.

* P4, L11: It seems odd to use a tilde with such precise numbers for the DOFs ("~6.0-

7.0")

* P4, L17: "which" –> "for which" and "is" –> "are"

* P4, L20-21: add "data" after "O3", "began" before "in January", and "has" before "impacted". Again, I don't think that Huang et al. [2016] is the correct reference for OMI O3 profiles.

* P4, L29: "analyses" –> "analysis"

* P4, L33: I think it is potentially confusing for readers to refer to the MLS data quality document in this manner here but as Livesey et al. on P3. Please be consistent. Also, the web site information should be provided in the reference list as part of the Livesey et al. citation.

* P5, L1: add "thick" after "8 km"

* P5, L16: "dataset" –> "datasets"

* P6, L5: "tied" –> "tied to"

* P6, L6: delete "in" after "within"

* P6, L17-18: "the UTLS chemical impact by ASM anticyclone" –> "the impact of the ASM anticyclone on UTLS chemical composition". Also, I think it would be more accurate to say "a picture *largely* consistent with that from MLS".

* P6, L32: "the empty" –> "an empty"

* P7, L11: "splits" –> "split"

* P7, L18: "enhancement" –> "enhancements"

* P7, L21: "are much more extended in longitudinal range compared to the MLS, co-located and mimic the east-west extent of the" –> "is much more extended in longitudinal range compared to that from MLS, co-located with and reflecting the east-west extent of the"

C9

* P7, L22: "distributions" –> "distributions on" and "that are not" –> "that is not"

* P7, L25: "on the south" –> "to the south"

* P8, L20-21: "is contributed by the retrieval information in the level lower than that represented by 150 hPa dynamical field" –> "reflect the influence of retrieval information from a level lower than that represented by the 150 hPa dynamical field"

* P8, L30: It is not clear what is meant by "the two sensors are influenced by different over vertical columns". I suggest instead something along the lines of "the two sensors sample quite different volumes of air".

* P9, L7-9: I suggest rearranging /rewording this sentence and replacing "interception", which is not correct here: "The structure of the bulging tropopause in the monsoon region (indicated by the intersection of the tropopause with the 105 hPa pressure level in Figs. 10 & 11) (Bian et al., 2012; Pan et al., 2016) has a significant influence on the O3 distribution."

* P9, L11-12: It is not entirely clear that the Fig. 9 being referred to here is from Park et al., not the current manuscript.

* P9, L18-19: "OMI also shows similar distribution" –> "OMI (Fig. 11) also shows similar morphology". Also, "Quantitatively" –> "Qualitatively" and "indicted" –> "indicated"

* P9, L31: "is" –> "are"

* P10, L1-2: "sectional anomalies" –> "regional anomaly". Also, "vertical and horizontal samplings of two satellites" –> "vertical resolution and horizontal sampling of the two satellite instruments"

* P10, L9: I think that "reduced" would be better than "weakened" here

* P10, L12-13: To be perfectly clear, please change "the weaker UT sensitivity" to "IASI's weaker UT sensitivity" and add "its" in front of "retrieval". Also, "product" could be deleted.

C10

* P10, L14: "dynamic" –> "dynamical"

* P10, L16: "Garney" –> "Garny"

* P10, L29: "convective-driven" –> "convectively driven"

* P11, L6: "dynamic" –> "dynamical"

* P11-15, references: Several references (e.g., Pan, Park 2007, Randel 2006, Vernier, etc) are incomplete (e.g., pages and/or doi missing)

* P12, L15: I am not familiar with the 2015 paper by George et al., but I am quite sure that "Bmc Medical Genetics, 8, 4095-4135" is not the correct citation for it

* P16, Fig 1a caption: please clarify whether the statement about the symbols being enlarged in Figure 1a applies to both data sets or only to MLS.

* P17, Fig 2 caption: "mean of CO" –> "mean CO" and "Note the" –> "Note that the". Also it probably would be a good idea to specify in the caption that the GPH values are also taken from the GFS analysis.

* P17, Fig 4 caption: "geolocation" –> "geolocations" and "selected GPH of" –> "selected GPH values at". Also, a few MLS data points at various spots in the map in Fig 4a are plotted in black – are these points off the color scale (on both ends)? That shouldn't be the case given the way the color bar is constructed.

* P20, Fig 6 caption: note here also that the color scale in this figure differs from that used in Fig 5.

* P21, Fig 7 caption: "white dash" –> "white dashed lines"

* P22, Fig 8 caption: "5 deg bins" –> "5 deg longitude bins" and "period" –> "periods"

* P23, Fig 9 caption: "dash lines" –> "dashed lines"

* P24, Fig 10 caption: "white" –> "white contours" and "interception" –> "intersection"

C11

* P25, Fig 11 caption: "mapped in" –> "mapped onto a" and "grids" –> "grid"

* P26, Fig 12 caption: add "bins" after "longitude" for OMI data

* P27, Fig 13 caption: "dash lines" –> "dashed lines"