Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-586-RC3, 2019

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The efficiency of transport into the stratosphere via the Asian and North American summer monsoon circulations" *by* Xiaolu Yan et al.

Anonymous Referee #2

Received and published: 9 September 2019

This manuscript uses CLaMS simulations driven by both ERA-Interim and MERRA-2 to quantify the relative contributions and transport efficiencies from different atmospheric layers in the Asian and North American summer monsoon regions to the stratosphere. Artificial tracers elucidate the main transport pathways from the two monsoon source regions and the Tropics to three destination regions: the tropical pipe and the extratropical lower stratosphere in both hemispheres. The manuscript is very well written and well prepared, and the analysis is sound. The results will be of interest to the broad ACP readership. However, there are a few issues (mostly minor points of clarification) that I would like to see addressed before the manuscript is published.

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General comments:

(1) Somewhere in this manuscript (probably in multiple places, including the introduction, main discussion of results, and conclusions), the authors need to relate their findings to those of Vogel et al. [2019], who also used CLaMS artificial tracers to study transport of pollutants and the pathways by which air masses enter the tropical pipe via the ASM circulation. While that paper is cited as part of a list of references in the first paragraph of the Introduction, it is never mentioned again. Although the details of the two analysis approaches differ, I feel that some discussion of how the results from the current manuscript fit in with the concept of the "upward spiraling range" introduced by Vogel et al. [2019], as well as comparisons with the transit times and fraction of ASM air masses in the TrP that they calculated, is warranted here.

(2) The authors tend to cite one or two papers for well-established points, without adding "e.g." at the front of their short list. Obviously not all relevant papers can or even should be cited, but I feel that overlooking the literature to this extent does a disservice to both the authors (because it erroneously reflects poorly on the depth of their knowledge of the field) and the previous studies, so I encourage them to make a greater effort in referencing prior work. Some places where the lack of references particularly bothered me are called out in the specific comments below.

Specific substantive comments and questions:

L30: Liang et al. [2004], while not an inappropriate reference for the point that ASM air can be transported to distant locations, should certainly not be the ONLY paper cited for this point – in fact, there are probably at least a dozen papers that could be added here.

L47-64: Although it is true that the NASM has received much less scientific attention than the ASM, it has not been neglected quite to the extent implied by these two paragraphs. I think that the authors should do a more thorough job of summarizing previous work on the influence of the NASM on UTLS composition. Given that water vapor is of

particular interest in this manuscript, Anderson et al. [Science, 2012], Schwartz et al. [GRL, 2013], and Randel et al. [JGRA, 2015] should be mentioned. More generally, other references to consider including are: Q. Li et al. [JGR, 2005], Cooper et al. [JGR, 2006, 2007], Barth et al. [ACP, 2012], etc.

Figure 1b: The 340 K surface seems too low to be appropriate for the map of MLS CO, which is not recommended for scientific use at pressures greater than 215 hPa. In fact, if proper data quality screening were applied, I would expect to see much of the tropics blanked out in a map of CO at this level. Thus some of the "hot spots" in this panel may be suspect. Since the specific level shown in this figure is not critical, I suggest 350 K instead.

L110-111: Some of the choices made here, while no doubt perfectly legitimate, should be explained. For instance, why are the simulations run over the 3-year period from 2010 to 2013? That is, why three years (and not two or ten), and why those particular years? Why are the tracers initialized over the interval 1 July through 31 August, when the anticyclone spins up by the beginning of June (if not earlier) and persists through September in most years? I am not suggesting that the analysis based on these choices is flawed, merely that they need to be better justified.

L131: Similarly, why is the April-to-June period used for Figures 3 and 4? I assume that June was chosen as the end of the interval because the tracers are re-initialized at the start of the next ASM season in July. But why include results starting in April (and not March or May)?

L155-161: References should be given for all three of the effects listed in L157-158. I see why these factors would lead to higher column amounts in the SH at 350-360 K than in the SH at 370-380 K. But it is less clear to me why they would lead to higher column amounts in the SH at 350-360 K than in the NH at that level.

L203-204: Other references would be appropriate here as well, including Bannister et al. [QJRMS, 2004], James et al. [GRL, 2008], and Dethof et al. [1999] (already cited

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elsewhere in the manuscript), etc.

L215: Again, a brief explanation of why the 400 K level is selected to be shown in Figure 6 might be good. Also, it might be helpful to add a horizontal line (maybe dashed or in grey) at this level in Figure 5, to orient the reader for the following plot.

Figure 7: Have the results in this figure been aggregated over the 2010-2013 period? How was the particular interval shown (November-May) chosen? It would be good to define what is meant by "young" in the figure caption as well as the main text. As stated in L239-240, the tropical pathway is more common for tracers released at 350-360 K, but it does not appear to be entirely absent for the 370-380 K tracers in Figure 7c. There are hints of a "fork" in the ASM tracer distribution between $\sim$3.7-4.0 ppmv and $\sim$2% (in which case the cyan arrow may be slightly misplaced). There may even be a faint hint of similar structure for the NASM tracer (Figure 7d), but the cyan arrow, useful though it is, obscures it.

L262-263: Is this time difference consistent with known upwelling rates? (A reference would be good.)

Figure 8: I understand that scaling the standard deviations improves the legibility of the plot, but multiplying by 0.2 seems like a fairly drastic step that produces a misleading impression of the degree of variability. How can such a substantial reduction in the scatter in this plot be justified? If the full envelopes were presented, results for the various destination regions would likely overlap significantly. As it is, I fear that the figure instills more confidence in the separability of the regions than is really warranted.

L314-318: To my eye, the TE into the LS-SH is never dominated by ASM or NASM sources for tracers released at 370-380 K – after February, the curves for all three sources lay nearly on top of one another. Moreover, for the ASM tracers transport from the 350-360 K layer dominates over that from the Tropics starting in December, not January. Finally, the TE from the ASM is nearly 50% larger than that from the NASM, so perhaps "slightly" should be deleted in L318.

L340-341: It would be appropriate to include here some references for the effects of Rossby wave breaking and eddy shedding on mixing monsoon air into the extratropics.

L404-411: I was confused the first couple of times that I read this paragraph, because I expected the results cited here to have been shown in Figure 11 – it is the last figure in the paper and freshest in readers' minds when they arrive at the Conclusions. I hadn't understood what was meant by "ultimate" in L404 (in fact, I don't think that the usage of that word conveys quite what the authors intend), and so it took me several minutes to realize that the numbers being quoted here for the most part refer to the end of the simulation period in Figures 8 or 9 and thus do not match the values in Figure 11. I concede that I obviously was not reading these sentences carefully enough, but I'm guessing that many readers may do the same and also may fail to note that Figure 11 shows the "maximum" contributions/efficiencies. That information is noted in the figure caption, but it is not stated when this figure is introduced in L358, which instead describes it as showing "overall contributions, efficiencies, and transit times". In addition, stating values such as 0.9 for the TE in L410 without specifying that this value refers to the end of the simulation compounds the confusion, as does stating a range for the TE from the Tropics to the TrP. In my mind this entire discussion needs to be clarified, with a bit more hand-holding to help the reader follow the details. However, this brings up a philosophical question about whether showing the maximum contributions/efficiencies is really the best approach for Figure 11. Moreover, while reading this paragraph I also wondered why a similar panel for the Tropics was not included in that figure.

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

L30: influences –> influence

Figure 1 caption: I questioned the need for the seemingly unimportant detail about the map being produced by python in my initial access review, and I still don't see why this information is useful to the reader. A similar comment applies to Figure. 10.

L57: "Meanwhile" seems like an odd choice of word here

L93: add a comma after “anticyclone”

L116: TrP has already been defined (L41)

Figure 3 and caption. Although it is stated in the main text, it would be good to add “in July and August” somewhere in the caption, perhaps after “initialized” or before “in CLaMS-E1”. Also, some odd glitches are apparent in the dashed line in this figure, especially in panel 3b at about (45N, 10m).

L155: The interhemispheric difference is fairly small, especially for the total column, so I suggest adding “slightly” in front of “larger”

L156: since this sentence is about the SH, just to be really clear, add “boreal” in front of “monsoon”

L158-159: portion . . . enters . . . and is (not “enter” and “are”)

L173: add a comma after “simulations”

L177: “not shown” – is this point not shown by comparison of Figures 3 and 4?

Figure 5 caption: I think it would be helpful to add “over the July 2010 to April 2014 period” after “sections”.

L210: . . . tracers is slightly lower –> . . . tracers is slightly weaker

L221: “spread out” might be better than “widespread”

L239: ASM (NASM) –> ASM (NASM) region

L255: show –> shows

L264-266: it would draw the contrast (and flow) better to move “after three months” to right after “However,” at the beginning of the sentence.

Figure 8 caption: I think it might work better to say “. . . simulations of air mass fractions (in %) in three source regions”

L268: that –> those

L271: it might be good to add “throughout the year” at the end of this sentence

L279-280: it might be good to add “As for the ASM,” at the beginning of this sentence

L289: it might be good to add “Much” in front of “more air”

L291: delete “and”

L297: delete “up to and”

L321: To me, “after March” means “starting in April”, but in fact the NASM TE exceeds the tropical TE in the TrP region at the beginning of March for the 350-360 K tracers. Thus “after March” should be “by March”. Similarly, “after April” should be “by April”. In addition, there is a typo at the end of this line: 380 KIn –> 380 K. In

L325-326: that –> those. Also, the CLaMS-M2 figure is omitted so I cannot judge myself, but I assume that a similar issue to the point raised above exists for “after December . . . or January”.

L337-339: these two sentences are somewhat redundant and could be combined for efficiency (and to eliminate the slightly awkward construction “. . . Fig. 10. Figure 10 . . .”). Also, when were the results for 24 August 2012 shown in this figure initialized?

L342-343: replace the second instance of “CLaMS-EI and CLaMS-M2” in this line with “the two simulations”

L366: it would be good to remind readers of these pathways by adding “(monsoon and tropical)” after “pathways”

L371: It is very confusing to start this sentence with “As for the NASM”. This kind of construction is often used to set up a discussion of similarity, but the previous sentence is also talking about the NASM, so that doesn’t make sense. You may have meant “As is the case for the ASM”, in which case there is a typo (“NASM” should be “ASM”).

That's what I assumed the first time I read this sentence, so I suggested making that change in my access review. Since the phrase remains in this version, I am guessing that was not your intention, and thus it is probably best to simply delete this phrase.

L375: maybe add "(not shown)" again at the end of the sentence

L386: I feel that the Conclusions section starts too abruptly – it needs some sort of introductory sentence to set the stage and sum up what was done in the paper. On the other hand, such a sentence is not really needed at the beginning of the Discussion section. Thus I suggest moving the first sentence in that section ("We have investigated . . .", L330-331) here.

L389-390: "vertical differences" is awkward. I suggest instead "differences in the dynamical situation with altitude"

References: the doi's for many of the references are repeated.

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