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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A PV-based determination of the transport barrier in the Asian summer monsoon anticyclone" *by* F. Ploeger et al.

G. Manney (Referee)

manney@nwra.com

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The authors develop a method using PV for examining the extent of and confinement of air in the Asian summer monsoon (ASM) anticyclone. Overall, the paper is well-written and original, and should be of interest to many readers of ACP. There are some issues that could, I believe, be fairly readily addressed that would substantially improve the paper, as well as a few clarifications and minor revisions as detailed below:

OVERALL COMMENTS:

1. Much of the analysis is focused on 6 July 2011. Why was this particular date chosen? How representative are this date and this year of the Asian monsoon anticyclone conditions in general?

2. After showing the MLS ozone in comparison with the CLaMS data in Figure 2, the ensuing analysis is done entirely with the model data. For the method to be most valuable, it would be nice to demonstrate more directly that it is useful for analysis of "real" data such as those from MLS as well as for the model dataset. Part of this would be demonstrating more thoroughly the degree of agreement between MLS and CLaMS. Specifically:

a. Why not show MLS CO as well as MLS ozone in Figure 2? This would be especially valuable since the ozone chemistry in the ASM anticyclone can be complicated [e.g., Lawrence and Lelieveld, 2010], and thus it may not always be a good tracer of transport.

b. In conjunction with (1), how representative is the agreement between MLS and CLaMS around 6 July 2011 of that at other times?

c. What is the vertical resolution of the model? The MLS v3 ozone vertical resolution in the UTLS is about 3km – is the model really that much better? (Values for vertical resolution for both should be given in the data description.)

d. Because the MLS data are time-averaged, one would expect some smoothing out of extrema, which might also contribute to the MLS ozone showing higher minima in the ASM anticyclone (which is where that apparent bias between MLS and CLaMS is most apparent). For the purpose of the comparison, why not time-average the CLaMS data as well and/or interpolate it to the MLS locations and average it in the same way as for MLS?

SPECIFIC COMMENTS (In order of appearance in text):

-p10594, is the monsoon circulation really "strictly in the TTL"? It can extend to around 40N, which seems at least subtropical?

-p10596, L7: This section contains a lot of (useful) tutorial material not typically found in "data and model" sections. A more appropriate section title might include "methods"

or "analysis" or some similar word. Also, the MLS data used in the paper should be described in this section.

-p10595, and subsequently in the paper: Numerous studies in addition to Nash et al (1996) have used PV gradients to define the edge of the polar vortex and assess the strength of its transport barrier (e.g., Manney et al, 1994, GRL – there are many others, this is just one that comes immediately to mind, not necessarily the best or earliest). The method that Nash et al introduced was to use the PV gradients constrined by being near a windspeed maximum. Since that windspeed constraint is not being followed here, the method does not "follow Nash" (as is said later in the text), and it would be appropriate to indicate that the PV gradient has been used extensively in this manner both before and after Nash et al.

-p10596, L16: The ASM region is more subtropical than tropical; therefore 100hPa is closer to 390K in the ASM region.

-p10599, L4-5: Doesn't the agreement depend to so extent on the selection of contours? How were the PV and Montgomery stream function contours that are shown chosen? Certainly, the higher Montgomery streamfunction contour shown is obviously irrelevant to defining the anticyclone region. But mightn't a Montgomery stream function contour in between the two lower ones shown do a better job of "outlining" the main anticyclone features?

-p10599, L6-9: Do the MLS data resolve such small-scale eddies? If not, how is the reliability and accuracy of such fine-scale structure in the model assessed? That is, are we confident that these are "real" features?

-p10600, L11: Isn't 10N a little close to the equator to be sure of eliminating all effects of low equatorial PV? Some of the figures seem to show well-separated low PV values at the lower edge of the plots.

-p10600, L17: See comment above re Nash et al.

-p10601, L13: What is the reasoning behind the choice of 30% as the threshold by which the maximum must exceed the minimum?

-p10601, L29: Shouldn't this be "Equivalent latitudes *higher* than the minimum circulation?

-p10602, L7: Using "the maximum" here is rather sloppy language, since the largest maximum (and hence "the" maximum if you allow only one) is always that associated with the subtropical jet.

-p10602, L11: Shouldn't this be "at PV values *smaller* than 5 PVU"?

-p10602, L17: "enhanced dynamic variability" seems a bit vague – many sorts of dynamic variability exist that do not weaken transport barriers.

-p10603, L1-8: While the agreement between CO and the selected PV contour does appear to be good overall, I think the current text does overstate it somewhat – for example, on 2011-07-09, 2011-07-18 and 2011-07-21, some of the highest CO values extend outside the PV contour, and the "split" on the last day is not obvious in CO. It would be more accurate to soften the statements here, and I do not believe this detracts from the message of the paper.

-p10603, L20-21: Does the 20 June to 20 August period cover the entire period for which human inspection of the fields (i.e., looking at maps) shows an obvious signature of the ASM anticyclone in CLaMS and MLS trace gas fields? If not, how long are the periods before/after when there is a signature in the trace gases but (presumably) the transport barrier is not strong enough to detect using this method? The CO field in Figure 11 doesn't show an obvious disappearance of that signature at the beginning or end of the plotted period.

-p10604, L2-3: Figure 11 does show high CO gradients at PV higher than that at the PV gradient maximum for a few days in early and late July, not "only after 15 August".

-p10604, L8-10: It is interesting that both 2012 and 2013 show low minimum PV values

for the transport barriers than 2011 – can you say anything about what this might imply in terms of differences in the ASM circulation?

-p10604, L23: There are numerous studies besides Sparling (2000) that use PDFs to look at transport and transport barriers: McDonald and Smith (2013) and Hegglin and Shepherd (2007) would be good places to start looking for references. At the very least, add an "e.g.," in front of "Sparling".

-p10605, L7-11: This is another place where using MLS trace gas data as well as CLaMS to construct the PDFs might be informative and provide insight as to how well the method applies to real data.

-p10606, L1-5: The dynamical variability in the Arctic polar vortex and in the subtropical jet are also extremely large – I would be astonished if that in the ASM circulation was larger than, for example, that during a strong SSW or a transient excursion of the subtropical jet around a strong ridge/trough pattern – during both of which the transport barriers can nevertheless remain quite strong. It must be the *type* of dynamical variability rather than the magnitude that is critical?

-p10606, L12-15: The ability to define a transport barrier over such a limited vertical range would seem, on the surface, to be a significant limitation of this method, which would be worth discussing a bit more. What do observations show with regard to the coherence of trace gas structures at levels above and below this? Over what vertical range do the dynamical fields – e.g., the winds that define the anticyclonic circulation – show a "closed" circulation? This is also another place where the question of the representativeness of 6 July 2011 is raised – is that vertical structure consistent throughout the monsoon season, and in other years?

-p10606, L21-23: I don't understand this statement – certainly crossing the tropopause is a sufficient condition for there to be a transport barrier – but it is my no means a necessary condition.

-p10606, L24-25: Surely there is no suggestion that a feature as large as the ASM boundary defined by the PV contours derived here could be considered "noise"?

-p10607, L6-7: Giving some indication (perhaps at least from the other two years that have been mentioned here) of the degree of interannual variability expected would be helpful.

-p10607, L12: It would be helpful to state what the longitudes of the Iranian and Tibetan Plateaus are.

-p10607, L15-21: I don't understand the point that is intended here. Is this an argument for a physical basis for bimodality, or an argument that it is an artifact of the geometry?

-p10607, L26-28: It isn't clear to me from this statement how the change in extent/location of the PV contours is related to the "conduit"?

-p10608, L1-8: How would high-resolution (inherently highly localized in space and time) in situ observations help, when full spatial and temporal coverage of the region is needed to assess transport barriers and their variations? What is "sufficiently high resolution" (in the horizontal and vertical)? Here again, it would help to have given the vertical resolution of the model and of MLS, and to argue why these are or aren't sufficient.

-p10608, L19: See comment above re Nash et al.

WORDING AND FIGURE ISSUES, TYPOS:

-Figure 1: The cyan line doesn't show up very well. What is the source of the data plotted in Figure 1?

-Figures 2, 9, 10, and 14 (especially 9 and 10) are too small. I realize this is partly because of the limitations of the ACPD format, but it would be good to insure that they are larger in the final ACP version.

-Figure 2 caption, second to last line, "is" should be "are"

-The Figure 11 color palette and symbols are difficult to read. The black symbols tend to disappear on the dark brown in the CO panel. I would suggest using a brighter color palette and/or a different symbol color – perhaps even two different colors for the symbols for PV and CO gradients.

-p10594, L10: replacde "notwithstanding" with "nevertheless"

-p10596, L13: "focusses" should be "focuses"

-p10596, L15-16: UTLS already defined on p10594

-p10597, L25: in the parenthetical statement either commas or nested parentheses are needed

-p10598, L9: Figures 2a and b show

-p10598, L10; p10600, L13; p10603, L10: The use of "exemplarily" here does not seem appropriate when what you mean is something like "as an example".

-p10599, L1: "to" should be "on"

-p10599, L7: "shedded" should be "shed"

-p10599, L19: add a comma after "structure"

-p10600, L13: Fig. 5 is introduced before Fig. 4 is discussed, thus it would make more sense to switch those figure numbers.

-p10600, L14: "mosoon" should be "monsoon"

-p10601, L20-21: Suggest changing "We apply an additional constraint to exclude the subtropical jet from the calculation, which generally shows much larger PV-gradient values" to "We apply an additional constraint to exclude from the calculation the subtropical jet, which generally shows much larger PV-gradient values"

-p10603, L24: add a comma after "variability"

-p10604, L12: replace "largely" with "highly"; delete "hitherto"

-p10605, L15, "which" should be "that"

-p10607, L8: add a comma after "variability"; also "which" should be "that"

-p10608, L8: replace "strongly" with "highly"

-p10608, L17: add a comma after "jets"

-p10617, Fig 3 caption: "June" should be "July"

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 10593, 2015.