Atmos. Chem. Phys. Discuss., 15, C5920–C5934, 2015
www.atmos-chem-phys-discuss.net/15/C5920/2015/

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.

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Received and published: 14 August 2015

General comment:

We thank the reviewer Gloria Manney for her careful consideration of the manuscript and her well thought-out comments, which significantly helped to improve the paper. In the following, we address all comments and questions raised (Reviewer's comments in italics). Text changes in the manuscript are highlighted in red (except minor wording changes). Main changes, related to all Reviewers's comments, concern: (i) an extended discussion of the evolution of the PV-gradients and the related transport barrier over the season and potential relations to convective activity (including ozone, mean age, OLR and diabatic heating rates in the revised Fig. 12) in section 5, (ii) a criti-

cal discussion of the leakiness of the diagnosed barrier (discussion), (iii) an extended discussion of MLS observations and the comparison between model and MLS (discussion, including a new Fig. 14), and (iv) shifts of the old section 6 to the appendix, of the discussion of the layer where our criterion is applicable to section 4, and of the discussion of the anticyclone location probability to the new section 6.

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?*

The 6 July has been chosen as an example of a distinct anticyclonic pattern in PV and several trace gas species and a clear PV-gradient maximum. It is indeed one of the better dates for application of the PV-gradient criterion, although not the best. Figure 12 shows that a similarly clear PV-gradient maximum can be determined for many days during summer 2011. In this sense the 6 July can be regarded representative for air mass confinement during the main monsoon period with a strong anticyclone.

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?*

We agree that a more extended comparison with observations would significantly increase the value of the determined transport barrier. However, a main problem when comparing to existing satellite measurements is the density of the sampling and the coarse vertical resolution. If the sampling is not frequent enough and data points over a long period have to be collected and averaged to reach a suitable coverage of the monsoon region, the large variability of the anticyclone spoils the barrier calculation as very different dynamic situations are mixed together.

Furthermore, because the PV-gradient maximum can be determined only in a shallow layer around the tropopause (around 370–390 K), a very good vertical resolution of the data is necessary. The vertical resolution around the tropopause in CLaMS is about 400 m. MLS ozone has a vertical resolution of about 3 km, which is significantly lower than the model resolution. Nevertheless, MLS ozone shows maximum gradients coinciding with the PV-barrier for several days during summer 2011. MLS ozone gradients are now presented for the entire summer season in the new Fig. 14. Given the large differences in vertical resolution between CLaMS and MLS, we think this partial agreement is encouraging and provides further confidence in the meaningfulness of the PV-gradient maximum as a measure of confinement (see also reply to Reviewer

1/Issue 1). Vertical resolution of CLaMS and MLS are now given in Sect. 2, as suggested.

MLS CO has a worse vertical resolution than ozone, of about 4.5 km. Mapping MLS CO versus PV (as done for MLS ozone in Figs. 2/14) generally yields very noisy maps and no clear gradient maximum. Therefore, we decided to focus on ozone. Satellite observations of a better vertical resolution (about 1 km) and an, at least, similar frequent sampling than MLS would be highly advantageous for the analysis of confinement inside the anticyclone. At the moment, MLS provides the best data source.

Indeed, agreement between CLaMS and MLS maps (as in Fig. 2) could be improved if the model data was treated in a similar manner to the satellite data (e.g., mapping to MLS locations and applying averaging kernels). This procedure had been applied recently to CLaMS water vapor (Ploeger et al., 2013) and CLaMS CO (Pommrich et al., 2014). Because for the gradient analysis a frequent sampling and a high vertical resolution are prerequisite, we refrain from applying this procedure here and from degrading the model data.

Specific comments:

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

We here followed Fueglistaler et al. (2009) who relate the monsoon systems to the TTL. We think this is a reasonable picture as the monsoons are very relevant to upward transport in the tropics, related to convection and upwelling. However, this is no strict definition and we reworded the sentence slightly.

-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.*

All suggestions have been adopted.

-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.*

We agree and therefore reworded all corresponding sentences, presenting more references and avoiding citing only Nash et al. (at least with an "e.g.").

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

Indeed, 100 hPa is located between 370 and 380 K in the core region of the Asian monsoon anticyclone. However, we think that 380 K and 100 hPa are close enough to keep the formulation as is.

-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?*

The advantage of Montgomery stream function is that it is a much smoother quantity in the monsoon region than PV. However, small-scale variations of trace gas mixing ratios along the anticyclone edge are much better captured by PV. In particular the

shedding of the smaller eddy to the east on 6 July 2011 can be clearly seen in PV but not in Montgomery stream function contours, which don't show an isolated eddy. We carefully checked that including more contours does not improve the agreement between trace gas and Montgomery stream function contours. Regarding this better agreement of trace gas confinement with PV than Montgomery stream function, the 6th of July is representative for the entire summer season. We would like to keep the few selected contours for the sake of clarity of the figure.

-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?*

As much of the small-scale variations are only visible in the higher resolution model data and not in MLS observations, it is difficult to proof that these are indeed realistic. However, as the MLS sampling is still not frequent enough to be comparable to the horizontal model resolution (about 100 km) and the vertical resolution in MLS (about 3 km for ozone, 4.5 km for CO) much coarser than in the model (about 400 m around the tropopause) it is not surprising that the model shows smaller scale features than MLS. Recent comparisons between CLaMS and MLS water vapor (Ploeger et al., 2013), MLS ozone (e.g., Konopka et al., 2010), MIPAS mean age (Ploeger et al., 2015), and various in-situ observations (e.g., Konopka et al., 2007) show that the model generally simulates the observations, and even small-scale variations therein, well. To achieve more confidence in these small-scale features, high-resolution in-situ observations from the Asian monsoon region would be highly beneficial, but these are not existing hitherto. We slightly extended the related discussion paragraphs in Sect. 2 and Sect. 7.

-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.*

Indeed, there may be some equatorial low PV-values included in the selected monsoon region. But also shifting the low latitude boundary to 15°N would not entirely solve this problem, and further exclude some of the high PV values at the equatorial edge of the anticyclone. Therefore, we decided to use 10°N, but checked that 15°N does not change our results substantially.

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

See our reply to the previous comment.

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

This choice is indeed somewhat arbitrary. By visual inspection of the PV($\phi_{\mathbf{eql}}$) function we chose 30% in order to count only clear maxima. For a single date the existence of a PV-barrier could depend on the exact percentage value. However, 30% turned out to be a value with only a very few number of such critical dates, when slightly varying the percentage value. The main conclusion of the paper, that a maximum in the PV-gradient exists and is related to the confinement of trace gases, would not change when using another percentage.

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

The formulation in the submitted manuscript was wrong (see also our reply to Reviewer 1). We reworded the sentence.

-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.*

We reformulated the sentence (e.g., using "local maximum").

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

Yes, indeed - thanks for noticing this mistake!

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

This formulation was indeed a bit vague. We compare the transport barrier evolution to OLR and heating rates now (see new Fig. 12), and find some indication for co-variations with convective activity (see our general comment (i), the new discussion of Fig. 12 and also our reply to Reviewer 2). The respective paragraph here has been reworded.

-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.*

We agree that the agreement between PV and CO was slightly overtstated in the text. The paragraph has been extended to discuss also disagreements in the figure. Just a side note: the split on 2011-07-21 can be seen also in CO, but is somewhat hidden in the two highest values of the color code (red and dark red), and not well visible in the figure.

-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.*

Confinement in trace gas mixing ratios inside the anticyclone is visible from mid June until mid-end September, hence already 1–2 weeks before a clear PV-gradient maximum develops. Hence, the confinement needs to be sufficiently strong for a clear PV-gradient maximum to be detectable. After the last date when the barrier criterion holds, it takes a few weeks until the confinement really vanishes and the mixing ratio anomaly is mixed away (see also our reply to Reviewer 2/Major comment 3). We include a discussion of these issues now in Sect. 5.

-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".*

We agree that our discussion of Fig. 11 was not satisfactory. We include more tracers (also ozone and mean age) and also proxies for convective activity (OLR, integrated heating rates) now in the revised version of the figure (new Fig. 12). The related discussion in Sect. 5 has been substantially changed (see also our replies to Reviewer 2/Major comment 3 and to Reviewer 1).

-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?*

We did not analyse the differences in the meteorological situation between different years carefully, except visual inspection of daily maps and calculation of the PV-gradient maxima. A more detailed analysis of the interannual variability would indeed be very interesting and will be the subject of ongoing research.

-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".*

As the discussion of the use of PDFs for studying the transport barrier should not be

the focus of the paper and to improve the readability, we moved the respective section to the appendix. The respective text part has been reworded.

-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.*

We include an analysis of MLS ozone for the entire summer season now in the discussion (see general comment (iii) and the new Fig. 14). This analysis just uses simply the mapping of ozone to PV and calculation of the respective gradient, as the PDF-related section should only be a side remark and not in the focus of the paper (see answer to the comment above).

As discussed already in our reply to the overall comment 2, there is agreement between MLS and CLaMS based gradients for parts of the season, but also some disagreement (not unexpected).

-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?*

As discussed already above (see also reply to Reviewer 2), the new Fig. 14 and the discussion now relate the variability in the PV-gradient to convective activity, which seems to be the most important type of variability affecting the monsoon anticyclone (as found already by Randel et al., 2006).

-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?*

We discuss these issues now more extensively and critically at several places in the manuscript (e.g., Sect. 4, discussion).

-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.*

The sentence has been reworded.

-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"?*

We removed this part of the sentence.

-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.*

There is not much systematic difference evident from comparison of the three years 2011–2013, except a slightly broader distribution in longitude in 2012. This information has been added. Interannual variability of the anticyclone and related transport will be further studied in the future.

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

This information has been added.

-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?

We think the bimodality in the longitudinal PV distribution is partly related to the projection and hence partly an artifact of the geometry. To what degree the bimodality in the longitudinal GPH maximum distribution has a physical basis needs to be further studied. We reworded the respective paragraph.

-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"?*

The sentence has been reworded.

-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.*

The model and MLS resolutions are given now in Sect. 2. Indeed, a dense coverage of the monsoon region with high-resolution observations (vertical resolution at least similar to the model resolution, which is about 400 m) would be the best. However, to our knowledge such a dataset seems not available during the next years. But also in-situ measurements from aircraft flights could provide important information about the confinement of air (e.g., flights crossing the PV-gradient based barrier could be analysed for co-varying structure in trace gas mixing ratios). The whole paragraph has been reworded.

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

See our answer to the comment above.

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?*

The source of the data plotted in Fig. 1 is ERA–Interim reanalysis, which is stated now explicitly in the figure caption.

-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.*

This should indeed be due to the ACPD format. We will ensure that the figures appear larger in the final ACP version.

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

Corrected.

-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.*

We changed the symbols to improve the presentation quality (see also reply to Reviewer 2).

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

Done.

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

Corrected.

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

We removed the definition here.

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

Changed.

-p10598, L9: *Figures 2a and b show*

Corrected.

-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".*

We reworded both sentences avoiding "exemplarily" now.

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

Corrected.

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

Corrected

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

Done!

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

Thanks for pointing this out! Because later parts of Sect. 4 correspond to Fig. 5, we would like to keep the order. Therefore, we briefly introduce Fig. 4 now at the beginning of the paragraph, before turning to Fig. 5.

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

Corrected.

-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"*

Changed as suggested.

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

Changed.

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