

Reply to Reviewers

General comment:

We thank the reviewers for again carefully considering of the revised manuscript. In the following, we address all comments and questions raised (Reviewer's comments in italics). Text changes in the manuscript are highlighted in red.

Comments by Reviewer 1:

The paper is publishable as is. However, I think Fig. 12e and the corresponding discussion (starting at line 465) should be removed. While there certainly must be a connection between convective activity and the strength of the potential vorticity gradient within the anti-cyclone, this figure does, in my opinion, a poor job of demonstrating it. It is likely that this connection deserves a much deeper analysis and is beyond the scope of this paper.

Our intention with Fig. 12e was not to explain the connection between the strength of the transport barrier and convection, but more to discuss possible connections. However, we agree with the Reviewer that we did not present a scientifically sound analysis here. Such an analysis is certainly beyond the scope of this paper. Therefore, we decided to remove the respective parts (Fig. 12e and text parts in Sect. 5), as suggested by the Reviewer.

Comments by Reviewer 2:

P2, L82: *I am not sure if already is the best choice of word.*

We removed the 'already'.

P7, L368: *(Consequently) This sentence seems to be containing duplicated information with the previous sentence (L364-, Therefore). Therefore, I suggest considering simplifying this paragraph.*

We shortened the second sentence to remove the duplicated information.

P7, L378: *(To summarize) The meaning of this sentence can be misleading. I assume what the authors meant by minimum is the minimum in anticyclonic circulation. As actual horizontal wind vectors are strongest adjacent to the boundary of the anticyclone, the absolute magnitude of the circulation can be strongest at the barrier.*

In our opinion, 'minimum anticyclonic circulation' would be even more misleading, as the anticyclonic circulation is strongest at the boundary of the anticyclone (as the Reviewer also states about). We added 'largest negative circulation values' in brackets and hope that, together with the explicitly given definition of the circulation as the line integral in Eq. (2), the sentence is clear now.

P7, L389: *(possibly related to enhanced convective activity of the anticyclone during these days) I am not sure how the enhanced convective activity is related to not-well defined anticyclonic boundary. For monsoon dynamics, convection acts as a forcing and strengthens anticyclonic circulation itself. Therefore, I would expect stronger convection with enhanced (and well defined) boundary of the anticyclonic circulation.*

We agree that our discussion about the link between the transport barrier and convection was not sufficiently deep, as remarked also by Reviewer 1. As suggested by Reviewer 1, we removed the respective parts (Fig. 12e and text parts in Sect. 5), and leave a deeper analysis for future studies.

P9, L443: *(Hence, only during the main monsoon season) This can be one of the biggest caveats of the method introduced in this study. First, the definition of the main monsoon season is not clear. Second, if reliability of the method defining the transport barrier is subject to dynamical conditions of the monsoon anticyclone, it will consequently diminish applicability of this method to other studies. I wonder if the authors would have any comments on recommended use of this method.*

We agree that the biggest caveat of the method is the short period of applicability during the main monsoon season. The confinement in the monsoon anticyclone is simply not as strong as in the polar vortex, as has been previously noted by *Garny and Randel (2013)*. How applicable the method is with respect to 'real' data

(observations) needs to be further studied in the future, hopefully with in-situ data from aircraft campaigns in the monsoon region available. The fact that MLS ozone shows maximum gradients coinciding with the barrier PV-value at least during parts of the season provides already some reliability in the meaningfulness of the diagnosed barrier.

P12, L610: *taifoon* → *typhoon*

Done,

P12, L626: - *in in Fig. 3* → *in Fig. 3*

Changed to 'to Fig. 3', following the suggestion of Reviewer 3.

Version of the MLS data I am aware that the new version of MLS data, which is v4.2, has become available recently. I doubt using new version of MLS data will change the conclusion of this work significantly. I recommend for the authors to contact MLS team for their opinion whether to use v3 or v4 for this work. However, I'll leave this to the authors own choice.

We explicitly state now in Sect. 2 that we use MLS version 3 data in this paper, as version 4 data was not available when we carried out the analysis. In the meantime we processed also version 4 data and cross-checked that the results stay qualitatively the same (in particular Fig. 13 stays almost the same).

Comments by Reviewer 3 (Gloria Manney):

Line 82: *Suggest deleting "already."*

Done.

Lines 83 and 85: *Perhaps "cross-gradient transport" would be clearer than just "cross-transport".*

Done.

Lines 173-184: *Unless I missed it, you don't say here which version of MLS data you use – if it is version 3, then you should also cite the MLS data quality document (available at http://mls.jpl.nasa.gov/data/v3_data_quality_document.pdf), since Livesey et al (2008) validated version 2.*

Yes, we use version 3 data, and we included this information and the correct citation - Thanks for pointing this out.

Line 347: *"PV-anomaly" → "PV anomaly"*

Done.

Line 354: *"PV-gradients" → "PV gradients"*

Done.

Line 356: *"PV-values" → "PV values"*

Done.

Line 365: *"PV-gradient based transport barrier" → "PV-gradient-based transport barrier"*

Done.

Lines 380-380: *and again on lines 485-486 and 585-590, In what way does convection affect the PV gradients/transport barrier? That is, does more convection lead to a stronger or weaker transport barrier? (I'm sure Randel and Park say this, but the reader doesn't want to have to stop and look that up to get this essential piece of information.)*

Randel and Park discuss how the area of low PV is related to convection, such that strong convection increases the area of low PV. They do not discuss the PV gradients in detail. As stated already in our replies to Reviewers 1 and 2, our discussion about this relation was not deep enough. As suggested by Reviewer 1, we removed this discussion in the revised version.

Line 417: *for* → *of*

Done.

Line 438: *mid of June* → *mid-June*

Done.

Line 443: *mid September* → *mid-September*

Done.

Line 443-445: *Suggest rewording sentence as "Hence, the degree of confinement....to be detected only during the main monsoon season"*

Sentence reworded.

Line 461: *well coinciding* → *coinciding well*

Done.

Line 462: *Only* → *However*;

Done.

Line 475-478: *What is the justification for using only positive heating rates? I do understand that the positive values are indicative of deep convection, and thus by using them only you do get information on the intensity of the lofting from convection where it does exist, but it seems to me that you lose information on how extensive the convection is (that is, how much of the monsoon region it covers).*

Our intention was to create a proxy of the intensity of the existing convection. However, integrating over all values leads to the same conclusions. As suggested by Reviewer 1, we decided to remove the short discussion about potential links with convection (including Fig. 12e and the related description).

Figure 12: *The black/grey diamonds are much easier to see, but the blue crosses are still very difficult to discern. Also, it would help to point out in the caption that the righthand y-axis values in (e) run from highest to lowest so the reader doesn't have to stop and figure out why the correlation coefficient is negative when the lines vary together!*

We changed the color for the crosses to a clear green, to have a better contrast. Fig. 12e has been removed.

Figure 13: *I assume that the cyan line in (b) is the 0.5 PVU contour for the average field, but it would be good to put this explicitly in the caption. It would also help to give the values for the red contours in the caption, as the numbers on the contour lines are impossible to read.*

Done.

Lines 522-526: *These statements are unclear (this is part of what I was saying was unclear in my review, but I guess *I* wasn't clear about that). I can't visualize what you mean by "area of lowest PV rotates clock-wise [sic] with the anticyclonic flow" – where is the minimum PV with respect to the center of the "closed" anticyclonic circulation? Isn't it near the center where the winds are near-zero? And is the following sentence (about the children's roundabout) some sort of angular momentum argument? And then it is not clear how this picture relates to being "projected onto the longitude axis"? Perhaps I'm missing something obvious, but I just do not understand this argument.*

Our argument was largely based on visual inspection of daily PV-maps, which were not shown in the paper. We agree that this is not a good basis for a clear argumentation which the reader can follow. As the respective part is no central part of the paper, we removed all speculative sentences and keep only the descriptive parts of Fig. 13.

Line 593: *Suggest "...measure of *the degree of* confinement..."*

Done.

Line 598: *to separate → for separating*

Done.

Line 610: *taifoons → typhoons*

Done.

Line 626: *as in in → to*

Done.

Line 627: *"Both reliably agree" is not very clear – suggest something like "Maximum ozone and PV gradient agree well..." (if that is what you mean).*

Indeed, that is what we meant - and we changed the sentence accordingly.

Line 631-634: *If this is comparing Figure 14 with Figure 12c, this should be stated explicitly. Assuming this is the case, is the comparison also affected by using the simple PV coordinate in Figure 14?*

The sentence is related only to Fig. 14, which includes both the MLS ozone gradient and the barrier-PV values.

Lines 634-636: *My question on the previous version still stands: What would highly localized in situ measurements provide that would help understand these issues, given that not only is higher vertical resolution important but also better spatial coverage? In fact, it seems contradictory to argue in the following lines that the horizontal spatial sampling of satellite instruments is inadequate when the sampling of the supposedly desirable in situ measurements would be much more limited.*

The best to have would be high-resolution observations (e.g., vertical resolution less than a kilometer) of dense geographical coverage. However, this seems not available in the near future. Therefore, high-resolution in-situ observations could at least provide case studies of the sharpness of the transport barrier (e.g., from cross-barrier flights) or about the vertical layer where a barrier can be deduced.

It was not our intention to criticize the quality of MLS data here. Indeed, we think that MLS observations do an astonishingly good job in describing the confinement in the Asian monsoon, providing a good benchmark for the model transport. Therefore, we changed the respective paragraph accordingly.

Appendix A: *While I agree that it is appropriate to move this material to an appendix, I still think it would be good to add some additional references on the PDF method as I suggested in my original review.*

Sparling et al. provide an extensive review about the PDF method, including numerous references to older related publications. The PDF method is no critical part of this paper and we just want to give one reference where the reader can find the basics, and therefore we want to keep simply the Sparling-reference, and leave it to the reader to dig deeper into the original literature.