Response to referee #2.

We thank the anonymous referee for the constructive comments that helped to improve the paper. All further questions/comments are answered/annotated in the following, the manuscript is changed accordingly. (Referee comments are emphasized in *italics*.)

This paper contains some interesting analyses regarding transport dynamics associated with the Asian summer monsoon that could potentially be important scientific contributions and worthy of publication in Atmospheric Chemistry and Physics. However, as it stands, the paper has severe weaknesses that make it unpublishable in its current form. The most glaring problems are that it is poorly organized, lacks focus, and the logic of their analysis is often lost in descriptive details that are not relevant to the main points of the paper. An example of the lack of focus concerns Sec. 4 that discusses the 'assessment' of atmospheric models from CCMVal to reproduce temperature, water vapor and ozone distributions. This section is awash with details and confusing analysis that neither produces a finding significant enough to state in the abstract nor one that is (apparently) used in subsequent analysis.

Section 4 is intended to summarize the main climatological features of the Asian summer monsoon (ASM) circulation, water vapour and ozone mixing ratios as simulated by the CCMs in comparison to the ERA-Interim re-analyses or MIPAS. This task can be seen as an addition to the CCMVal report (SPARC CCMVal, 2010). We think that it is important to show the climatological analyses before showing the results of the multiple linear regression. We tightened Section 4 by removing Figures 3 and 4 from the manuscript and revised the text as proposed by the reviewer.

There are also problems with the logic of the arguments themselves – although the organizational problems of the paper make it difficult to properly assess the logic of these arguments. Consider the contention that the Tibetan plateau is the primary source of water vapor for the monsoon anti-cyclone. The sole evidence for that seems to be a peak in the correlation between temperature variability and their monsoon index found in the atmospheric models over the Tibetan plateau (at the 360 K potential temperature surface). Not only does this argument fail to show any dynamical relationship to water vapor variability, but the reliability of temperature signal is questionable; in comparisons to ERA-interim, the authors do not show values at 360 K, but they do show values at 380 K that are not in good agreement.

At the 360 K isentropic level there is a strong and significant increase in ERA-Interim water vapour (H2O) mixing ratios and temperatures above the Tibetan Plateau (TP) with increasing monsoon circulation index (MIDX). To support our argument, the ERA-Interim MIDX regression patterns at 360 K are included in the revised manuscript. Temperature and water vapour variability are closely linked in the respective region. To show this we composited the July–August 360 K ERA-Interim temperatures and H2O mixing ratios (1979–2013) according to wet and dry extremes in a region above the TP defined as 70–100°E/30–40°N (Fig. 1, also included in the supplement as Fig. S6). There is a clear indication that months with H2O mixing ratio anomalies enhancing by more than one standard deviation above the TP are connected with higher temperatures at 360 K above the TP and vice versa for the dry composite with months where the H2O mixing ratio anomalies are decreased by more than one standard deviation.

... the authors do not show values at 360 K, but they do show values at 380 K that are not in good agreement.

This is deliberate. We highlight that the MIDX regression pattern of the 380 K temperature is different from that at 360 K, as it shows the two areas of decreasing temperatures connected with the anticyclonic circulation patterns at the western edge and to the north-east of the anticyclone.

A second example concerns the contention the anti-cyclone transports water into the mid-latitude stratosphere, but not into the tropical pipe. It seems that the sole evidence for this is the large water vapor mixing ratios found to the north of the anti-cyclone, but not to the south. Such a water vapor distribution could certainly arise for the reason that the authors contend, but they provide no evidence for a dynamical link that shows that the high water vapor concentrations are due, at least in part, to transport by the anti-cyclone. Furthermore, the results from Sec. 4 show large discrepancies between water vapor distributions *from the CCMVal models and satellite observations.*

There are several studies analysing isentropic water vapour transport (e.g. Dethof et al., 1999; Ploeger et al., 2013) that come to the conclusion that the ASM region is a significant moisture source for the lower stratosphere in northern high latitudes. The process responsible for this water vapour transport is described by Dethof et al. (1999) as an interaction of synoptic disturbances with the Asian monsoon anticyclone (AMA) that pull filaments of tropospheric air from the northern flank of the AMA. Our analyses are based on the monthly mean data base available for the CCMs participating in CCMVal-2, therefore it is not possible to study processes occurring on a much smaller time scale. On the other hand we have CCM data spanning 45 years which is much longer than the time series used for most studies related to the ASM.

I suggest that the paper be rejected but that the authors be encouraged to resubmit after a thorough overhaul that provides clear logical arguments backed up with strong evidence. For the revision, I suggest that the authors choose some small number (e.g., 3-5) of important points, choose the results that best illustrate those points and how uncertain they are, and then rewrite Secs. 4 and 5 accordingly. In particular, there are important aspects of the circulation that are not well reproduced by the CCMs (for example, water vapor distributions) that could easily undermine the results. A more careful and logical discussion will help determine just how strong of a case the authors actually have.

Specific Comments: Many of the figures are crowded into too small of a space and not well labeled, making them difficult to understand. Please add explicit labels on the figures that help distinguish the different panels. For example: Fig. 1 should label the top panel MIDX and the bottom panel WIDX and Nino 3.4, Fig. 2 should label the top panels as 150 hPa stream function and the bottom panels velocity potential, and so on. Also, consider dividing the individual panels in a way that does not crowd them into such small spaces. Perhaps some of the panels can be left out of the paper.

We reduced the number of figures in the revised manuscript to focus more on the interannual variability of the AMS. Remaining figures are now more clearly labelled.

Page 1, lines 4-7: State briefly what the CCM assessment is.

The word 'assessment' might be a misnomer in this context and therefore we replaced it by 'comparison'. As the reviewer correctly states, an assessment is a much deeper evaluation of CCMs, which also includes the rating of individual models.

P. 1 L. 15-16: Be more clear: what is meant by 'consistent'? Weaker than what?

The QBO regression results in water vapour and ozone are consistent in the sense that they are in agreement with the understanding of the QBO modulation. The downward propagating QBO west-phase is generating an anomalous meridional overturning circulation that is directed downward near the Equator and directed upward in the subtropics. The resulting temperature anomaly is positive, thereby leading to increasing water vapour mixing ratios. The reduced upwelling near the Equator is leading to higher ozone mixing ratio. The term 'weaker' was intended to evaluate the QBO regression patterns for water vapour and ozone in comparison to the QBO regression pattern for temperature. The statement is rephrased to be more obvious.

P. 7 L. 11-12: It is over-simplified to state that the 3 terms in equation (1) 'represent' the Hadley, Walker, and Monsoon circulations. That is, each term contains more than just those circulation features. It would be, for example, better to say something like 'Chi-star-prime is influenced by the monsoon circulation'.

The revised manuscript now includes a statement about the limitation of the Tanaka et al. (2004) method to separate tropical circulations.

P. 7, L. 17: Explain why you add an artificial seasonal cycle to MIDX (by changing from using maximum values of Chi to minimum values). Given the strong seasonality of the monsoon, it should be possible to find an index that has a strong seasonal *cycle without any artificial inflation.*

The seasonal cycle is not artificially added to the MIDX time series, as it is a consequence of the reversal of $\chi^{*\prime}(t,x,y)$ over south-east Asia from being positive to negative during the months from October to April. To reflect this reversal in sign, the method of Tanaka et al. (2004) defines the value of the MIDX to be the maximum in $\chi^{*\prime}(t,x,y)$ in a region limited to south-east Asia from May to September and to be the minimum during the remainder of the year. As we are concentrating on the interannual variability of the ASM during July/August (JA), we use the JA average of the MIDX as a basis function in the multiple linear regression model that does not include the seasonal cycle.

Figures 3 and 4: The discussion of these figures is particularly chaotic and confusing.

The figures 3 and 4 are now removed from the manuscript.

P. 21 L. 4-5: Please clarify this discussion. It seems to me that Fig. 10a indicates positive regressions for MIDX onto OLR over BoB, Myanmar, and Taiwan. Doesn't that indicate weaker convection over these regions – instead of stronger as you state? Or do you mean to say 'we also get a decrease in convective activity over the BoB ...'

The negative regressions for MIDX onto OLR (indicated by the colours ranging from yellow to read) show an increase in convective activity over wide areas of Myanmar, the southern part of BoB, the Indian subcontinent, southern China, Hanai and Taiwan (at the eastern edge of this region). This indicates that the convective activity is increasing with increasing MIDX in large areas where the JA OLR is usually at its lowest values.
We included a more detailed description of the locations in the revised manuscript.

Appendices: The Appendices are too terse to be useful. The autocorrelations discussed in Appendix B are not referred to in the main text (except at the end of Sec. 3 which merely states that autocorrelations were treated) and should be removed unless a more articulate discussion of how the autocorrelations affect the analysis is provided.

Removed.

Appendix A is also not necessary. You could simply mention that the criteria for significance are derived from the Z-test and refer the reader to Stouffer et al. (1949) and Whitlock (2005) for details.

The Appendix A is removed from the manuscript.

Fig. 1 caption: Add the term 'WIDX' to the description. For example, 'bottom: index for the Walker circulation (WIDX; solid)'

The figure 1 is now redrawn and does no longer include the WIDX time series. It is replaced by a figure showing the time series of the JA basis functions MIDX, QBO, and ENSO, used in the regression analysis.

P. 6 L. 4: Regarding 'graduate'. Do you intend this word to mean 'to make more gradual'? If so, this is an awkward use – if not, it's difficult to understand the meaning of the phrase. It would be better to use a different word.

The term 'graduate' is a statistical/mathematical term to indicate the ability of the multi-model average to balance the extremes of individual CCMs. But we agree with the reviewer that it is not commonly used. We replaced the term 'graduate' by 'level out', that hopefully is better understood.

P. 6, L. 8: Change 'indication for the' to 'indication of the'.

Done.

P. 6 L. 10: No comma after 'model'.

Done.

P. 7 L. 5: Change 'allows to express' to 'allows us to express' or some other grammatically correct wording.

The sentence is rephrased.

P. 7 L. 24: The clause 'whereas the nino3.4 ... variability' is a non sequitur – it implies contrasting behavior but no source of the contrast is given. Perhaps you intend to say something like 'the regression onto WIDX emphasizes the west Pacific circulation response to inter-annual SST variations whereas regression onto nino3.4 describes the larger (scale) response to ENSO variability.'

It was intended to emphasize the ability of the nino3.4 index to better reflect the ENSO variability compared to the WIDX time series, as only the largest ENSO warm events are equally good captured by both indices. We are not talking about the regression of any of the indices in this section of the paper.
As Figure 1 is redrawn and does no longer include the WIDX, the discussion about the differences to the nino3.4 index is no longer included.

P. 10 L3: Change relative to 'relatively'. Also you should state what you are comparing to when you say it is relatively small (i.e., relative to what?).

We intended to express the smaller spread in JA velocity potential maxima of the CCMs over southeast Asia compared to the spread in stream function maxima within the AMA of the CCMs. Relative to the stream function maxima the velocity potential maxima deviate less about the multi-model average. We rephrased the sentence.

P. 12 L. 6: Add (e.g., in parentheses) that the heating rates are displayed with red lines.

Done.

P. 21 L. 13: Change 'round' to 'around'

We changed the term.

P. 21 L. 25: Change 'South to the AMA' to 'South of the AMA'.

Done.

P. 22 L. 5: Remove 'in' from 'temperature from in nine re-analysis datasets'.

Done.

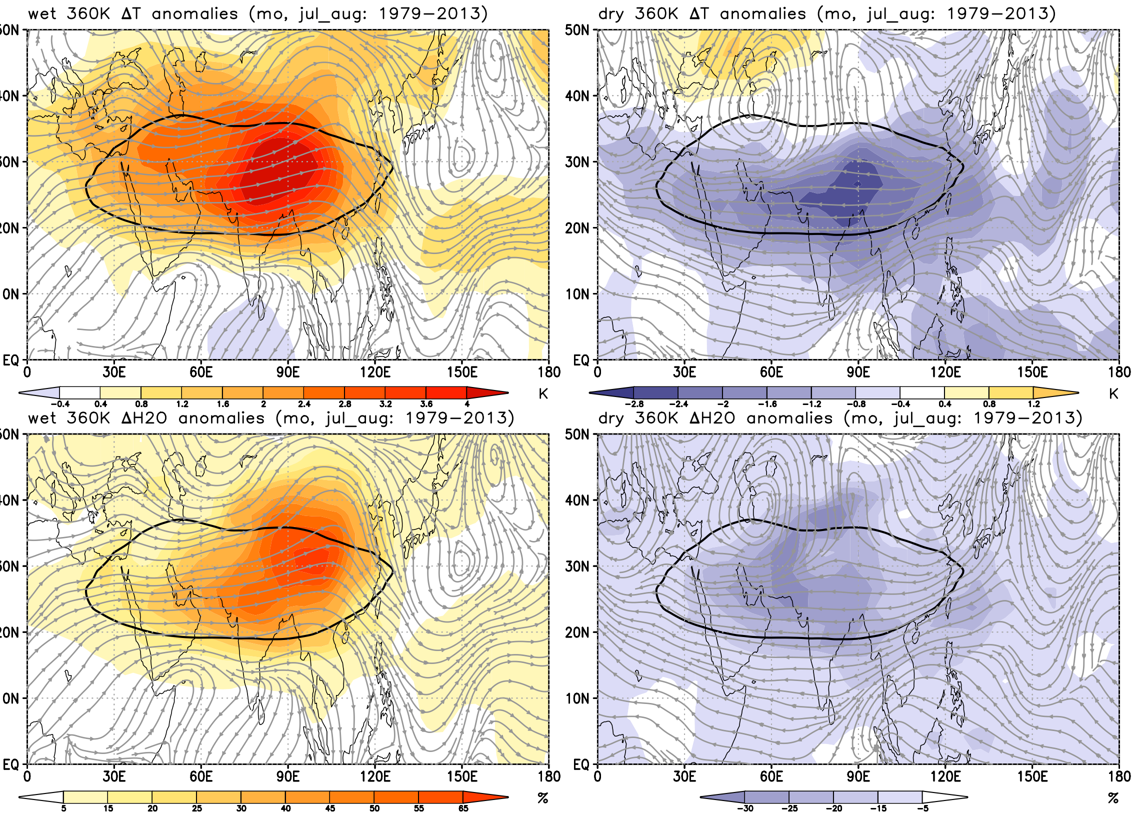

Figure 1. Composited anomalies for wet (left) and dry (right) 360 K monthly mean ERA-Interim water vapor extrema in July and August over the TP region (30–40°N, 70–100°E) analysed for years from 1979–2013; ERA-Interim temperature (top) and water vapour (bottom). The ERA-Interim data are preprocessed and do not include QBO and ENSO variability. Overlaid as streamlines in grey are the composited horizontal wind anomalies; the 3513×10^2 m^2 s^{-2} contour of the Montgomery streamfunction is overlaid in black.

References

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Ploeger, F., Günther, G., Konopka, P., Fueglistaler, S., Müller, R., Hoppe, C., Kunz, A., Spang, R., Grooß, J.-U., and Riese, M.: Horizontal water vapor transport in the lower stratosphere from subtropics to high latitudes during boreal summer, J. Geophys. Res.: Atmos., 118, 8111–8127, doi:10.1002/jgrd.50636, http://dx.doi.org/10.1002/jgrd.50636, 2013.

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