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Earth System Dynamics Discussions

Interactive comment on "Seasonality and spatial variability of dynamic precipitation controls on the Tibetan Plateau" *by* Julia Curio and Dieter Scherer

Anonymous Referee #1

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Overview –

This manuscript investigates the influence of dynamic factors on precipitation in High Asia. Findings on the seasonality and spatial variability of precipitation controls (and precipitation type) in the region using horizontal and vertical wind speed, atmospheric water transport, and boundary layer height are an important contribution to the field. The research is presented in an intuitive manner and uses unique data to answer major questions that have not been previously addressed. I do have a few questions/concerns about the applicability of HAR, and am suggesting considerable revisions, but after these issues are addressed I believe the manuscript will be fit for publication.

General Comments –

HAR is a great data source that can help us address many questions that could not previously be asked in High Asia, but there are still many issues in modeling precipitation (especially convective precipitation) using 10km resolution with a convective parameterization. Rather than discourage the use of these unique data, I believe it is important for the current manuscript to address the uncertainty in HAR in a more substantial way. Referencing the papers is not sufficient in my opinion. Given that so much of the present analyses are based on correlations with convective precipitation in regions that have limited validation, it is important to thoroughly explain what the caveats are and why the precipitation controls describe here are robust despite uncertainty in the data.

Moisture variability is not sufficiently investigated in this manuscript. I would like to see atmospheric water vapor treated separately from advection where possible. Because the TP is so high, winds are generally very strong and moisture is minimal. Thus, the water vapor transport variable is much more heavily influenced by wind speed than water vapor. AWT figures in the manuscript confirm this bias by exhibiting heavy influences of the wind speed terms, which seem to mask any water vapor signal (even though the aren't independent). Furthermore, the opposite seasonal cycles of wind and moisture, and their respective sources of variability, may be illustrative of changes in precipitation controls through the year.

The precipitation/pblh relationship over the TP is something that I do not understand well, and would like to see more about, however, this discussion elicits questions about the PBL parameterization that was used in HAR, and it's validity over the TP that can't be answered in this paper. Though Maussion et al. (2011) performed a few PBL sensitivity tests, validation was not done to specifically evaluate PBLH and thus it is impossible to determine how representative this variable is of the actual conditions. Though this is somewhat true for the other precipitation controls used in this study, I am especially skeptical of PBLH because it is not resolved explicitly, and is so poorly represented in other parts of the world. In my opinion, the discussion is an interesting but non-essential part of the paper, and ultimately, I think it is best to remove it.

There are a number of careless grammatical errors and poorly structured sentences in this manuscript that distract the reader. I've noted a few of instances of sentences that were difficult to read in the "technical comments" section, but I encourage the authors to carefully edit the full text.

Specific Comments –

Page 2, Line 10: Why was vertically integrated water vapor transport analyzed and not column integrated water vapor? Since you are already investigating winds, it makes sense to investigate moisture without accounting for wind so that you can determine whether moisture availability is due to advection or local changes in temperature/evaporation? The two aren't completely independent, but they do have opposite seasonal cycles as well as their own sources of variability.

Page 2, Line 49-51: Referencing a study of deep tropical convection over the ocean hardly seems relevant to precipitation controls over the TP. It would be better to have a reference for mountain environments, or at least for land areas. If not, it may be necessary to show this relationship using HAR. Furthermore, this sentence is at odds with the negative effect of high horizontal wind speeds on convective precipitation that is described in following sentence.

Page 3, Line 4: Note that the real benefit occurs because of orographic forcing of the moist flow. Without topography the relationship would be different.

Page 3, Line 15: It may be interesting to look at atmospheric stability (beyond the PBLH). In convective precipitation, vertical motions occur for different reasons than during frontal precipitation. Looking only at stability (theta or theta-e) would be instructive as to environmental conditions that support vertical motion irrespective of horizontal wind against the orographic barrier. This is important for both summer and winter. (this is only a suggestion)

Page 3, Line 41: This raises a number of questions for the reader: Were multiple PBLH schemes tested for WRF? What confidence is there that the PBLH from the configuration with the chosen parameterization is performing well? Were PBLH scheme sensitivity experiments performed in the development of HAR and if so, what observations are they validated with? If PBLH is to be included in this paper (which I've already argued against in the general comments section) it is necessary to address these very important questions in the manuscript rather than only providing the reference for HAR.

Page 4, Line 10: Because this study focuses on convective precipitation, some discussion of the validity of the HAR dataset, which uses a convective parameterization, should be included here. Beyond the reference, the authors should clearly note that precipitation during summer is highly sensitive to the choice of convective parameterization. Furthermore they should note that using higher resolutions and resolving precipitation explicitly may alter the spatial and temporal distribution of precipitation. Despite the issues associated with HAR precipitation, I do believe that it is very useful for understanding the mechanisms presented in this paper and the spatial and temporal differences in precipitation across large regions of the TP. I am simply encouraging the authors to further discuss the caveats here so that the reader is aware of these issues. After all, they may not be familiar with the Maussion articles or may not read them to better understand the data that you have used.

Page 4, Line 32-33: I am curious as to how many days are included in the precipitation analysis. It seems that a threshold of 0.1 in the Karakoram and western Himalaya would remove very few days from consideration through the winter months. Why was a percentile-based threshold not used in addition to this low threshold to further exclude days with light precipitation that don't really contribute to seasonal totals in an area such as the KH. Including these dates masks the signal of precipitation controls during events that contribute strongly to overall precipitation accumulation (e.g. in the Karakoram and western Himalaya only a few dates during the winter season contribute the majority of seasonal (and in some cases, annual) precipitation).

Page 4 Line, 43-44: Do correlations in regions of sparse precipitation exhibit considerable sensitivity to a few extreme events? If you threshold out the largest few events (99th percentile) would the correlations change?

Page 5, Line 20-22: The yellow and green classes may be very sensitive to the cumulus parameterization scheme that was used in HAR. Due to the uncertainty in how well WRF, and specifically HAR in this case, represent convective precipitation in the Himalaya, I don't think these classes can be so clearly defined. At the very least, better discussion of the caveats must be given, but it may be better to not distinguish these groups since the precipitation controls discussion does not.

Page 6, Line 1: It seems to me that the reason the high elevations of the eastern Himalaya (near the Brahmaputra Channel) exhibit considerable precipitation contributions outside of the monsoon season is due to the orographic locking of westerly flow and the large amount of available moisture in this area. See Norris et al. (2015; their Figs. 5&6), which are focused on westerly disturbances affecting other regions of the Himalaya, but in both cases, exhibit precipitation in the eastern Himalaya generated by terrain locking of westerly flow.

Page 6, Line 30-32: This should cite some of the more seminal work on westerly disturbances.

Page 6, Line 32-33: Higher wind speed does not necessarily mean enhanced moisture supply. There are many factors that modulate both wind and moisture in these systems (see Cannon et al. 2015). I would suggest just removing "which benefits from enhanced moisture due to higher wind speeds" and stating that the cyclonic/frontal precipitation is associated with westerly disturbances and then add a citation (e.g. Dimri et al. 2015 – review paper of westerly disturbances).

Page 7, Line 2: Figure 1 indicates that the western Himalaya receives more than 0-5% of annual precipitation during summer (June alone is over 5% for Cluster 0). Furthermore, some studies argue that about 30%-60% of precipitation in this region falls

during summer (e.g. Bookhagen and Burbank, 2010; their Fig. 4). Though the Bookhagen and Burbank estimate is probably too high because it is based on TRMM, which doesn't do well with winter precipitation, it is clear that considerable rainfall occurs in the western Himalaya during summer. Given that there is precipitation, what then could explain the lack of correlation?

Page 7, Line 7-8: Remove the linkage between higher wind speed and enhanced moisture. I would argue that higher wind speed more efficiently extracts moisture due to stronger orographic forcing. As mentioned before, there are many controls on moisture availability in the western Himalaya that are independent of the cross-barrier wind speed.

Page 7, Line 32-39: The alternating positive/negative correlations look more like a gravity wave (i.e. updrafts and heavy precipitation on windward side of the mountain, downdrafts and light precipitation in the lee; Roe et al. 2005) than an error associated with aggregation. There should be an easy way to show this using topography aspect and wind direction, or using higher temporal resolution data available from HAR.

Page 7, Line 41: I recommend looking at the correlation of precipitation and precipitable water rather than precipitation and water transport (or at least in addition to it). This is particularly important to do since convective precipitation requires enhanced moisture content, but not enhanced wind, while orographic precipitation requires both enhanced wind and enhanced moisture. Even though wind and moisture aren't independent, it would be interesting to see a figure of just the precipitable water correlations since it does not include the advection term, for which a similar variable was shown in previous figures (WS300, WS10). This could be particularly illustrative since the seasonal cycles of moisture availability and wind speed are opposite for this region (e.g. Cannon et al. 2015).

Page 8, Line 31: I don't see the need to introduce boundary layer height, which is controlled by other processes that have already been evaluated. I don't think this adds much to the discussion, which was already quite strong. Furthermore, PBLH is parameterized in WRF, so you're introducing another question about how well this parameterization works, rather than focusing on variables that are explicitly resolved (moisture, temperature, wind). Though Maussion et al. 2011 performed a few sensitivity tests with different PBL parameterizations, validation specific to PBLH using radiosondes or wind profilers over the TP has never been done, so there are a lot of unknowns here. The lakes discussion is certainly interesting, but given the uncertainties about WRF's ability to resolve these processes, and because this discussion is not a main topic in the paper, I suggest leaving it out.

Page 11, Line 33: The uncertainties in HAR should include the use of a convective parameterization, which directly affects the precipitation data (presence, timing and magnitude) that this study is based on. A better description of this particular issue is required since it is possible that some of the results in this work are sensitive to the choice of parameterization (or that if HAR were performed to explicitly resolve convection (<6km)).

Page 11, Line 48: Did you try using hourly or 3-hourly data to test what role aggregation errors play in your research? It seems that you have the necessary data to directly address this uncertainty.

Technical Comments –

Page 1, Line 7-8: Change "moisture" to "water resources"

Page 1, Line 32: Change "strengthen" to "strengthening"

Page 2, Line 1-8: A few of these sentences were difficult to read. A little restructuring would help the reader here.

Page 2, Line 25: When introducing the acronyms (PBLH, AWT and WS300), they should first appear next to their full names.

Page 3, Line 7 & 18: "luv side" should be changed to "windward" as that is the common

terminology in meteorology textbooks. (change in all instances)

Page 4, Line 24-25: Did you aggregate daily, or are you using a specific time-slice for each day?

Page 5, Line 21: Change "intensive" to "pronounced". Intensive makes it sound as though this relates to magnitude of precipitation values rather than the shape of the distribution.

Page 6, Line 13-14: remove "and therefore exhibit no coherent patterns". The patterns are coherent when considering mesoscale, synoptic and large-scale influences as well as topography. The patterns are complex and heterogeneous, but not incoherent.

Figure 2: The cyan and yellow lines are difficult to see. Perhaps all the lines could be thicker or darker colors could be used

Page 7, Line 12: "There are high positive correlations (over the Tibetan Plateau?) between WS10 and precipitation. . ."

There are too many small mistakes in word choice, grammar and sentence structure to identify each individually. I encourage the authors to carefully edit the full text.

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