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Discussions

Interactive comment on "Variability of moisture recycling using a precipitationshed framework" *by* P. W. Keys et al.

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Response to Anonymous Referee 1

Note: Referee comments are in bold, while Author responses are italicized.

1) EOF Analysis issues
a. In the EOF analysis it is not clear what variable you are analyzing. Is it the seasonal average evaporation within the precipitationshed? Please be more explicit about this.

Thank you for your comment. For this analysis, we compute the EOF of the growing-season mean evaporation contribution (in units of mm/growing season) for each precipitationshed. This was unclear in section 2.5.2, and we will reword the sentence to state this explicitly. We will also repeat this in the results section of the text as well (section 3.3) to remind the reader:

Section 2.5.2
The second measure of precipitationshed variability uses empirical orthogonal function (EOF) analysis to quantify the growing-season average evaporation variability over the precipitationshed.

Section 3.3
Next, we employ empirical orthogonal function (EOF) analysis to reveal the spatial patterns that explain the most variance in the three precipitationsheds. As stated earlier (in section 2.5.2), the variable we are analyzing is growing season average evaporation.

b. The patterns that you are obtaining are quite strange. The most usual result for an EOF analysis should be a dipole pattern (much like your second EOF of Western Sahel). The fact that most of your EOFs contain a spatial pattern of only one sign leads me to believe that there might be a problem in your analysis. The easiest way to diagnose it is to look at the timeseries of the principal components. This timeseries would enable you to see what years are associated with the patterns that you are seeing in your EOFs. Please show all the PC timeseries for your three modes.

We agree with the reviewer that in many atmospheric science applications, the leading EOF results in a dipole (shifting) pattern of the anomalies. However, this is not always the case. For example, the leading EOF can also exhibit a pulsing, rather than

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shifting pattern, as found for the eddy-kinetic energy in the Southern Hemisphere by Thompson Barnes, 2014.

We have confirmed that the presented EOFs have been calculated correctly, and thus, they suggest that the leading mode of variability of the evaporation contribution to a specific sink region is not a shifting of the contribution region, as one might expect, but is rather a pulsing of the contribution. That is, the leading EOF describes that some years the total contribution is low and other years the total contribution is high from the precipitationshed.

We did not include the PC time series in our submitted manuscript, because we are intentionally emphasizing the spatial pattern of variability, rather than the temporal behavior of this variability. However, future work is aimed at examining the relationship between precipitationshed variability and modes of climate variability, and timeseries analysis will be a critical component. For the reviewer, however, we have included PC1 (red line) for the Western Sahel below in Figure 1. The PC shows clear year-to-year variations that will be investigated in future work.

c. When you remove the interannual trend, you first calculate a trend based on the area average and then remove it from each pixel? This might be the problem. It might be best not to remove the trend, and then do the EOF analysis – if the dominant EOF is a trend (this you can diagnose using the principal component timeseries), then don't analyze this mode and move on to the next mode.

We thank the reviewer for their comment and suggestion. We have calculated the EOFs both with and without removing the trend, and the resulting patterns are qualitatively similar (Figure 2). The PC time series associated with these patterns are shown in Figure 1, and once again, confirms that the PCs are overall similar, but not identical.

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We would like to keep the analysis as is (i.e. with the trend removal) for two main reasons: (1) we hope that this preliminary precipitationshed analysis will serve as a framework for future research, where it is not guaranteed that the results with and without the trend will be similar, and (2) EOF analysis searches for the pattern that describes the most variance in the data. By keeping the trend in the data, the leading EOF is a combination of the trend and the pulsing mode, which may confuse the interpretation. Furthermore, we are specifically interested here in the year-to-year variability, not the trend, and so we have chosen to remove it.

However, due to some confusion by our earlier wording, we will re-write the text as follows:

"Before performing the EOF analysis, we remove the long-term linear trend in evaporation contribution at each grid point. This is done by taking the total precipitationshed evaporation contribution for each growing season, calculating its linear-least squares fit, and removing it from the data. We remove this long-term trend to ensure that the variability we are capturing is representative of interannual variability and not simply due to long-term trends."

d. Also, when you are looking at the PC timeseries, you can evaluate if the EOFs are related to interannual modes of climate variability such as ENSO. This is done by analyzing the correlation between the PC timeseries and the index of ENSO (or other atmospheric patterns that are affecting your region).

Thank you for your suggestion. We previously performed the suggested analysis looking for relationships between the PC timeseries of the EOFs for each precipitationshed and the climate variability index of ENSO. However, the results were not easily interpreted. For this reason, we have chosen to leave this analysis *and discussion for a future paper where we may dive into more depth about how the variability of the precipitationshed (and its PC time series) is dynamically linked (or not) to these climate modes.*

e. Finally, I am not sure what you mean in the abstract by "most of the variance in the precipitationshed is explained by a pulsing of more or less evaporation from the core precipitationshed". You have not demonstrated that there is pulsing, it could be an oscillatory pattern, it could be an anomalous year, or even a trend. You must look at the timeseries to figure this out.

Thank you for your comment. In fact, we believe that we have demonstrated that the leading EOF indicates a pulsing. Perhaps the reviewer thinks that we are suggesting that the PC timeseries is periodic (e.g. sinusoidal, etc.)? If so, this is not the case. Rather, we are saying that the EOF anomalies are a physical mode whereby either more or less evaporation is contributed from the core precipitationshed. The word "pulsing" in this context is used to refer to the fact that the EOF is of the same sign over the domain, and that the EOF anomaly is co-located with the climatological evaporation contribution field. The word "pulsing" is very commonly used to describe an EOF anomaly of this nature in the atmospheric science community (e.g. Wittman et al. 2005, Eichelberger Hartmann, 2007; Thompson Barnes, 2014).

To illustrate this point, Figure 3 is a panel taken from Wittman et al. (2005), and depicts a "pulsing" EOF mode of the zonally-averaged zonal winds. Note that the leading EOF (panel E) only has a single-signed EOF and aligns with the climatological mean winds (panel B; bold line).

f. I recommend Statistical Methods in the Atmospheric Sciences: 2nd Edition by Daniel S. Wilks to improve the analysis.

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Thank you for the reference.

2) In the discussion, I think it is important to reflect on what actually happens when an upwind region is deforested. If region A receives 50% of their moisture from upwind region B, and region B is completely deforested... what actually happens? The answer is complex because the deforestation of region B will likely affect the atmospheric circulation patterns – not only the amount of moisture delivered to region A. I think it is important to realize that the problem is highly nonlinear, and likely a complex interplay between direct effects and non-direct effects (effects on the circulation patterns due to the changes in energy at the surface). A good analysis is the one by Goessling and Reick (2011). What do moisture recycling estimates tell us? Exploring the extreme case of non-evaporating continents. HESS.

We thank the reviewer for this very insightful comment. We fully agree that we need to acknowledge the nonlinear impacts of land-use change, and will revise our text on Page 5161, L21 (bold face text denotes the new text):

Recent studies have quantified how anthropogenic land cover change influences the hydrological cycle through land cover change impacts on evaporation rates (e.g., Gordon et al., 2005; Sterling et al., 2012), and the eventual precipitation that falls downwind (e.g., Lo and Famiglietti, 2013). However, land cover change has the potential to not only influence evaporation rates, but also the atmospheric circulation itself. In some cases, this effect has been shown to be small (e.g. Bagley et al., 2014) while in others, land cover change leads to significantly different circulation patterns (Goessling and Reick, 2011; Lo and Famiglietti, 2013; Tuinenburg et al, 2014). If one is to apply the precipitationshed framework to understanding how land cover change may influence downwind precipitation, then it will be important to address whether the circulation itself is significantly modified. If this is the case, new precipitationshed

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boundaries will need to be identified to reflect the modified circulation.

3) Page 5145, line 5: Please add the reference "Dominguez, F. and J. C. Villegas and D. D. Breshears, 2009. Spatial Extent Of The North American Monsoon: Increased Cross-Regional Linkages Via Atmospheric Pathways. Geophys. Res. Lett., 36, L07401, doi:10.1029/2008GL037012" Which deals with the impact of drought on terrestrial recycling.

We thank the reviewer for this suggestion. We will include this reference at the suggested location in the text.

4) Page 5145, line 27 "In the Amazon, advection of oceanic moisture is the dominant source of precipitation, with relatively low interannual variation (e.g., Bosilovich and Chern, 2006)." Is a bit simplistic, as this is a place where local terrestrial recycling is very important, particularly in the southwestern part of the basin. Please add a few references to terrestrial recycling within the basin.

We agree with the reviewer here, and will adjust this sentence to the following:

"In the Amazon, many studies suggest that though advection of oceanic moisture is a very important source of precipitation, terrestrial recycling is also a very important process for sustaining regional rainfall (e.g., Eltahir and Bras, 1994; Bosilovich and Chern, 2006; Drumond et al., 2008; Gimeno et al., 2012; Spracklen et al. 2012)."

5) 5149, line 10: The reference to the WAM-2 model is incorrect. It is in the 2013, not 2014 paper.

We thank the reviewer for their comments. We are assuming that the reviewer's *suggestion that the 2013 reference for the WAM-2 model refers to van der Ent et al., 2013 "Should we use a simple or complex model for moisture recycling and atmospheric moisture tracking?".*

We referenced the 2014 paper (rather than the 2013 paper), because the WAM2-layers that is described in the 2013 paper was specifically a regional version, whereas the version described in the 2014 paper is a global version. However, for the sake of clarity we will add both references, since the WAM2-layers is technically used in both.

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Bagley, J. E., Desai, A. R., Harding, K. J., Snyder, P. K., Foley, J. a. (2013). Drought and Deforestation: Has land cover change influenced recent precipitation extremes in the Amazon? Journal of Climate, 130823112200002. doi:10.1175/JCLI-D-12-00369.1

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Dominguez, F., Villegas, J. C., Breshears, D. D. (2009). Spatial extent of the North American Monsoon: Increased cross-regional linkages via atmospheric pathways. Geophysical Research Letters, 36. doi:10.1029/2008gl037012

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Sterling, S. M., Ducharne, A., Polcher, J. (2012). The impact of global land-cover change on the terrestrial water cycle. Nature Climate Change, 2(10), 1–6. doi:10.1038/nclimate1690

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Tuinenburg, O. a., Hutjes, R. W. a., Stacke, T., Wiltshire, a., Lucas-Picher, P. (2014). Effects of Irrigation in India on the Atmospheric Water Budget. Journal of Hydrometeorology, 15(3), 1028–1050. doi:10.1175/JHM-D-13-078.1

Wittman, M., A. Charlton L. Polvani, 2005: On the Meridional Structure of Annular Modes, Journal of Climate, 18, 2119-2122

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 5143, 2014.

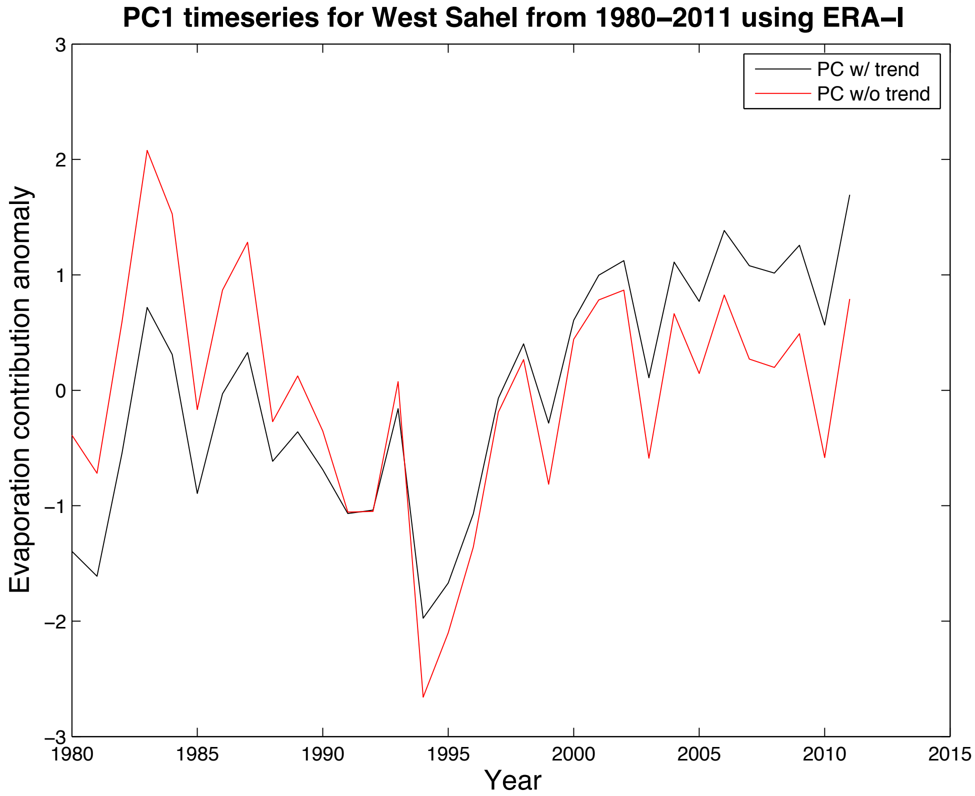

Fig. 1. Comparison of the PC1 timeseries with and without the trend, for the Western Sahel.

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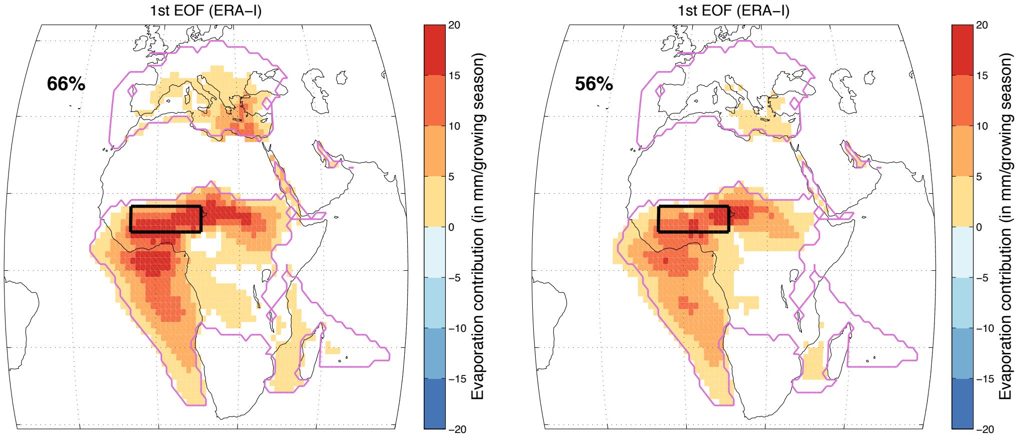

Fig. 2. A comparison of EOF1, where the trend is included (left) and the trend is removed (right; as in the manuscript). Note the difference in the amount of variability explained by the two EOFs (∼10%).

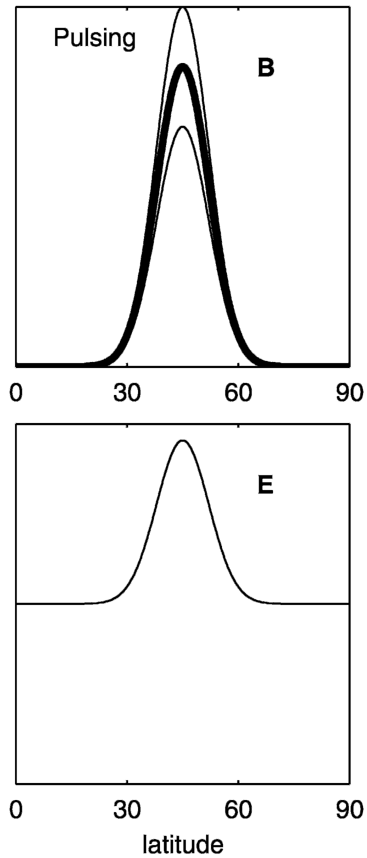

Fig. 3. From Wittman et al. (2005). The top panel shows mean wind (B; bold line) and two different states of the wind (weaker vs stronger jet); the bottom panel (E) depicts the leading EOF of jet variability.