

## ***Interactive comment on “Tracing changes in atmospheric moisture supply to the drying Southwest China” by Chi Zhang et al.***

### **Anonymous Referee #2**

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Review of "Tracing changes in atmospheric moisture supply to the drying Southwest China" by Zhang et al., submitted to ACPD

The authors present a climatological study of moisture transport to a region in China, analysing trends in precipitation from Reanalysis and gridded station data. Trends in moisture origin and moisture convergence are also analysed. The paper is generally well-written and the methods are mostly sound. However, there are several uncertainties in this analysis that need further discussion, and some statements need additional justification. I suggest to include another section providing a discussion of the method uncertainties and the possible impact on the results. Furthermore, I could imagine that the manuscript may possibly find more readership in a specific climate journal, maybe the Handling Co-Editor has some thoughts on this. Below I detail my major and specific comments.

## Major comments

1. The method description should be improved by explicitly naming some of the underlying assumptions. For example, the calculation of a proportion of precipitation relative to total column water implies that water vapour is well-mixed at every grid cell. Do the results depend on grid spacing of the input data? A figure would help to support the explanation of how the WAM method works.
2. The authors state in Sec. 2.2 that after 30 days, "a large amount of water may be left in the air" and that they continue for another 30 days. Please quantify how large an amount is left in the atmosphere after 30 days. What is the origin of this uncertainty? How realistic is it to assume precipitation water stays in the atmosphere for 60 days (2 months) in this region?
3. The method uses a combination of reanalysis and observational data that are blended together and partly rescaled. Could it be that inconsistencies between the ERA-Interim water cycle and observations bias the results, and impact the trend analysis? What are the uncertainties of this combination of data used? How do uncertainties in P and E parameterisations influence the results? This should be discussed and evaluated in more detail, maybe in a separate "method sensitivity/method discussion" section.
4. The display of moisture contributions in Fig. 2 is cut off at 0.8 mm, providing 88.3% of the total precipitation. Is there a justification for choosing this percentile? It would be helpful to add also the contours encompassing 50% and 95% of the total precipitation in the figure.
5. The trend obtained in the analyses seem to depend strongly on the years 2006 and 2011. Is there a significant trend observed if these two years were removed from the time series? How reliable are trends from reanalysis data in general?
6. Some conclusions seem not sufficiently based on evidence in the manuscript. This

includes the statement that "local recycling played a minor role" (pg. 5, L. 12), that the "dominant role of dynamic process ... prevails over a very large area" (pg. 6, L. 20), and the speculation of a possible role of SST anomalies in the changes (pg. 6, L. 30). Notably, the "might be related" in that line becomes a "likely related" in the conclusions (pg. 7, L. 16), even though no evidence to that end is presented in the manuscript. A clearer and more balanced argumentation, including alternative interpretations, should be formulated in all of these cases. The statement "the westerlies play a secondary role" (pg. 7, L. 5) has also no clear anchoring the the results presented before.

7. More references to the literature on the topic of moisture source analyses should be included in the introduction and in the discussion of the uncertainties of the results. In addition to the studies by Gustafsson and Zhang, consider some of the earlier founding work from Stohl and James (2004,2005), James et al. (2004), Sodemann et al. (2008), Sodemann and Zubler (2010), Baker et al. (2015), Winschall et al. (2014). Also relevant are the discussion of the uncertainties of the well-mixed assumption (Goessling and Reick, 2013).

#### Specific comments

Pg. 1, L. 14: "monsoon region" please specify which monsoon region

Pg. 2, L. 4: descend flows -> descending motion

Pg. 2, L. 21: seems somewhat circular, please rephrase. Analysing the moisture sources and transport appears to me as another way of looking at circulation patterns, but with a focus on one aspect of precipitation (the other one being lifing/condensation).

Pg. 2, L. 27: Unclear how the Trenberth (1991) citation fits in here.

Pg. 2, L. 28: grid cell -> degree

Pg. 3, L. 8: Is this vertically integrated moisture transport?

Pg. 3, L. 25: Please clarify how exactly the rescaling was done in order to ensure

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reproducibility of your results. What rescaling factor was used?

Pg. 4, L. 5: At what level where  $q$  and wind velocity considered for this analysis?

Pg. 4, L. 10: has experienced -> shows

Pg. 4, L. 22-23: lapses -> decreases

Pg. 4, L. 24: delete "precipitation"

Pg. 4, L. 24: the finding that humid regions provide more moisture than arid regions is quite obvious; the description could provide more quantitative detail

Pg. 5, L. 8: Figure 2 has already been introduced above

Pg. 5, L. 15: please define what you mean by "moisture supply"

Pg. 5, L. 15-25: How dependent are these results on the threshold of 0.8mm? In general, I find the moisture flux change vectors difficult to relate to the moisture contribution change, because the moisture flux is calculated for the entire atmospheric humidity, and not for the contribution to the target region.

Pg. 5, L. 30: how was moisture divergence calculated?

Pg. 5, L. 32: "the close correlation": is that the only possible conclusion? My understanding is that moisture divergence is related to precipitation by mass balance requirements, but does not provide insight into the roles of moisture transport vs. local evaporation. Please elaborate.

Pg. 6, L. 12: Obvious is a quite subjective term. Are the trends significant? How reliable are such trends from reanalysis data?

Pg. 6, L. 20: Data availability for the CMA data should be stated.

Figure 1a: Please provide a wider area in the figure panel, including some topography contours and maybe country names for orientation. A distinction between the national boundaries and province boundaries would also be helpful.

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Figure 2a: Does the green shading indicate that all areas shown in the figure panel contribute  $>0$  mm to the target area?

Figure 3: Is it possible to restrict the shading and moisture flux vectors to moisture arriving in the target region only?

## References

A. Baker, H. Sodemann, J. U. L. Baldini, S. F. M. Breitenbach, K. R. Johnson, J. van Hunen, and Z. Pingzhong, 2015: Seasonality of westerly moisture transport in the East Asian Summer Monsoon and its implications for interpreting precipitation  $\delta^{18}\text{O}$ , *J. Geophys. Res.*, 120, doi:10.1002/2014JD022919.

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P. James, A. Stohl, N. Spichtinger, S. Eckhardt, and C. Forster, Climatological aspects of the extreme European rainfall of August 2002 and a trajectory method for estimating the associated evaporative source regions, *Nat. Hazards Earth Syst. Sci.*, vol. 4, no. 5, pp. 733–746, 2004.

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[Interactive comment on Atmos. Chem. Phys. Discuss.](#), doi:10.5194/acp-2017-147, 2017.

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