REVIEW OF ZHANG ET AL

GENERAL COMMENTS

The paper of Zhang et al investigates quantifies the trends in rainfall decrease in Southwest China and investigates whether and how atmospheric circulation plays a role. The paper is easy to read and the figures generally support the text and vice versa. The study is novel in the sense that atmospheric tracking has not often been applied to trends in precipitation, but rather for climatologies or variability studies only. That being said, I have a few concerns with the manuscript, which I hope the authors can address in a revised version:

- Units are not used consistently up to (what should be) the scientific standard. Precipitation should always be per a unit of time, thus mm mon^{-1} and never just mm. Trends should always be per unit of time squared, thus mm yr^{-2} or mm mon^{-1} $decade^{-1}$ and never just mm yr^{-1}. Same holds for moisture flux divergence (or in fact any flux). The sister journal of ACP, HESS, has a good guide: http://www.hydrology-and-earth-system-sciences.net/for_authors/manuscript_preparation.html under mathematical requirements.
- The water accounting model (WAM) has received several updates since van der Ent et al. (2010), and it is not clear whether the authors use the updated version with two vertical layers (van der Ent et al., 2014), which is apparently open source now (van der Ent, 2016). This may be very relevant due to the wind shear present in the area under investigation, which will lead to biases when vertically integrated fluxes are being used (van der Ent et al., 2013; Goessling and Reick, 2013).
- There is limited background information on the ground-based precipitation dataset from CMA. It is always tricky to do trend analysis on interpolated data for which the stations on which the dataset is based might not be homogeneous. I suggest the authors give more information on the number of stations used, whether that is constant, are there data gaps, is it just stations or satellite information as well? And a reason why they think it is safe to apply trend analysis on this dataset.
- The decomposition of moisture transport is not well enough explained. The results seem relevant, but from the information in the paper I do not see how this could be easily reproduced.

SPECIFIC COMMENTS

P1,L19: "at a rate of -23.6 mm^{-1} decade"
This is just one of the many examples what I mean with the wrong use of units. Because the unit is incorrect it leaves the reader wondering whether this is -23.6 mm per year per decade or -23.6 mm per decade per decade or -23.6 mm per month per decade or -23.6 mm per day per decade. Admittedly, these mistakes can be found abundantly in the scientific literature, but it is no excuse, in my opinion, to take such issues lightly, rather I hope that the authors agree with me and start correcting themselves as well as others.

P2,L27-28: "The ERA-I data have a spatial resolution of 1.5° × 1.5° grid cell"
Apparently this is the resolution that the authors used (which is ok), but other (higher) resolution are also available, thus please rephrase this sentence.

P2,L30-P3,L2: Here, the authors explain that they have replaced the evaporation and precipitation fields from ERA-I with CMA precipitation and GLDAS evaporation, because of existing "limitations in the reanalysis estimates". The claim about limitations is, however, not being backed up with a reference or figures and nor is any proof given that the alternative datasets are any better. I suggest the authors to back up this choice of data better.

P3,L5: “backward in time”
As far as I know backward tracking with WAM has been applied by Keys et al. (2012) for the first time.

P3-P4: “Section 2.3 Decomposing moisture transport”
This entire section could benefit from equations and figures to explain the concept behind decomposition.

P4,L21-22: “As shown in Fig. 2, the farther away from the target region, the lower intensity of moisture is contributed to the target (Zhang C. et al., 2017)”
I think it is a bit misplaced to cite just an own paper here as there are literally dozens of other papers that used back-trajectory methods which have found this. Moreover, it is not even as simple as put here, because it naturally depends on the winds (otherwise we could just draws circles around the target region)

P4-P5, “Section 3.1 Moisture origin”
I think previous literature is not sufficiently cited in relation to the findings of this paper. A few papers that have source region figures for China or sub-regions of China that for example could be of interest (Keys et al., 2014; Wei et al., 2012, 2016).

P7,L5: “the Asian monsoon regions”
Which are exactly? Would it perhaps make sense to delineate them somewhere?

P7: “Data availability”
What about the data availability of the CMA product? This section should be expanded according to the ACP guidelines: http://www.atmospheric-chemistry-and-physics.net/about/data_policy.html

The summer months appear to be July, August and September, whereas the meteorological summer for the northern hemisphere is generally regarded as June, July, August. Why the difference? The fact that JAS is considered should be 100% clear in all figure and table captions.

Figure 2: the caption should include what the contribution to total precipitation the red boundary in Fig. 2a encompasses. I saw it mentioned in the text, but not in the figure caption itself.

Figure 2: why is the Tibetan Plateau relevant?

Figure 2: There are multiple black lines (also the target region), which makes the caption confusing.

Fig. 2b: The information between 0 and 1 and -1 and 0 seems quite relevant, could the authors add more colors?

Figure 2: Is the boundary between East and West expert judgement? The art of the modeler? Or is there some physical determining factor?

Figure S1: What do the colors mean? The color scale lacks units or explanation in the caption.

TECHNICAL CORRECTIONS

As mentioned before, units should be corrected throughout the paper.

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