Reviewer #2

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.

A: We appreciate the insightful and constructive suggestions. We have added another section to provide discussion of model uncertainties and carefully justified the related statements. The paper is not only about the climatology of moisture transport, but also the dynamic causes of changes in moisture transport. We think it is certainly within the scope of ACP. The point-to-point responses to the comments are listed below.

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.

A: 1) Although the full description of WAM should refer to the listed founding works (van der Ent et al., 2010; van der Ent and Savenije, 2011; Keys et al., 2012), the WAM method has been better described in the revision including discussion on the well-mixed assumptions.

2) Given the size of the study area, we believe the chosen resolution of the input data is appropriate. Our choice is also motivated by previous studies which successfully applied the same 1.5° ERA-I data (e.g., van der Ent and Savenije, 2011; van der Ent et *al., 2010; Keys et al., 2012; 2014; Zhang et al., 2017).*

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?

A: 1) The precipitation moisture is tracked monthly. Take July for example, we would track the moisture from 7.31 to 7.1. In fact, precipitation may occur at the beginning of the month on 7.1. If we stopped tracking at 7.1, the precipitation moisture would be left in the air and not have been allocated to its surface sources. Many studies show that the average residence time of water vapor in the atmosphere is around 10 days (Trenberth, 1998; Numaguti, 1999; Trenberth, 1999). But this measurement of residence time is e-folding based, which means that after 10 days, there is still 1/e (i.e., 36.8%) of the original water vapor left in the air. In order to track more precipitation moisture, we prolong the track process to more than 10 days, as in our case 30 days, to guarantee that most (>95%) of the monthly precipitation can be tracked. This is a Eulerian way of tracking moisture too. It is different from the Lagrangian method that usually tracks the precipitation events backward for around 10 days. The 10 days limit is also due to the accuracy of the trajectories (Stohl, 1998).

2) Take July as an example, the tracked moisture accounts for 65.1% of precipitation on average from 1979-2013 when the tracked time is from 7.31 to 7.1, while it accounts for 97.4% when tracked from 7.31 to 6.1.

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.

A: We use observation-based P and E estimates to rescale the P and E in the ERA-I data because ERA-I estimates have large biases (Trenberth et al., 2011). The blend of P and E does not affect the direction of moisture fluxes in the ERA-I data, rather it affects the magnitude of the contribution. We have conducted the experiments for comparison with E and P from ERA-I. The results are shown in Fig. r4 and r5. The essential pattern of moisture contribution is similar, except that the sources tend to contribute more with ERA-I E and P, since the SWC precipitation in JAS is higher with ERA-I than with CMA (see Fig. r6). The major region (enclosed by the 0.8 mm JAS^{-1} red line in Fig. r4a) contributing 88.3% of precipitation with CMA, contributes 89.4% with ERA-I. The trend patterns are generally the same with both datasets as more moisture comes from the East and less from the West (Fig. r5). The major results are thus unaffected. We have added a discussion section to evaluate the possible uncertainty caused by it.

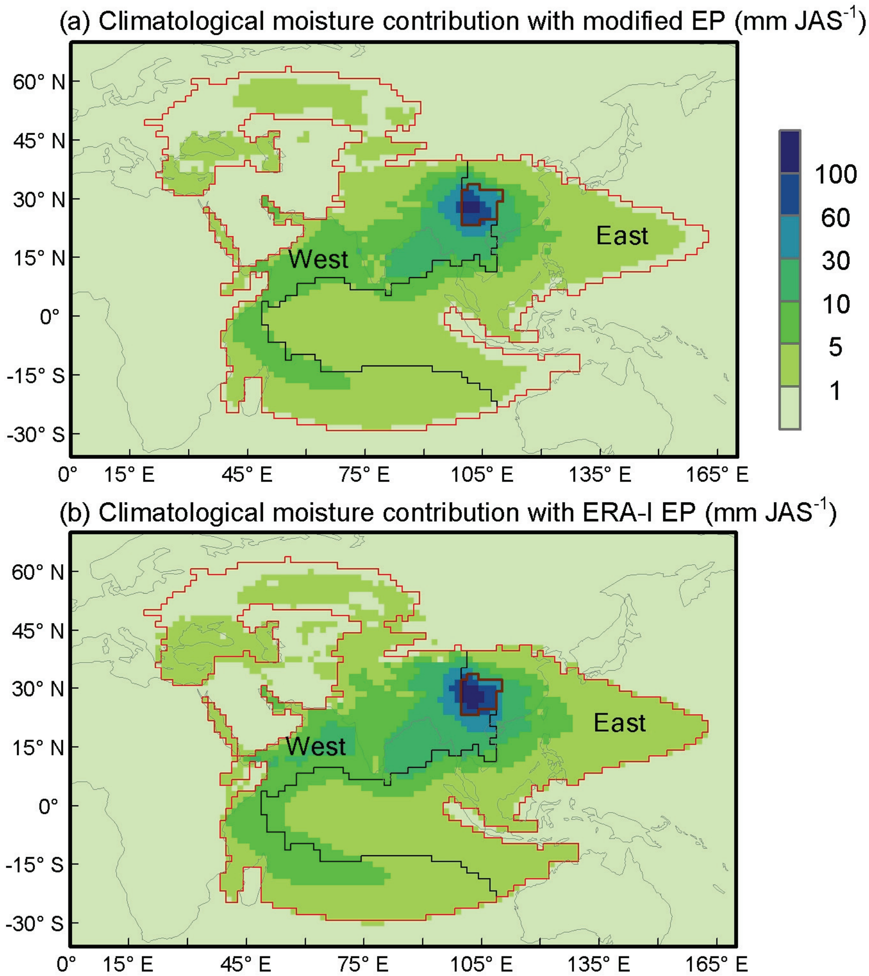

Fig. r4 Climatology of the summer moisture contribution to the SWC precipitation from 1979 to 2013. The red line delineates the major source region, (i.e., grids with value above 0.8 mm JAS^{-1}) which contributes 88.3% (89.4%) of the SWC summer precipitation moisture with CMA (ERA-I). The West and East division is unchanged as in Fig. 2.

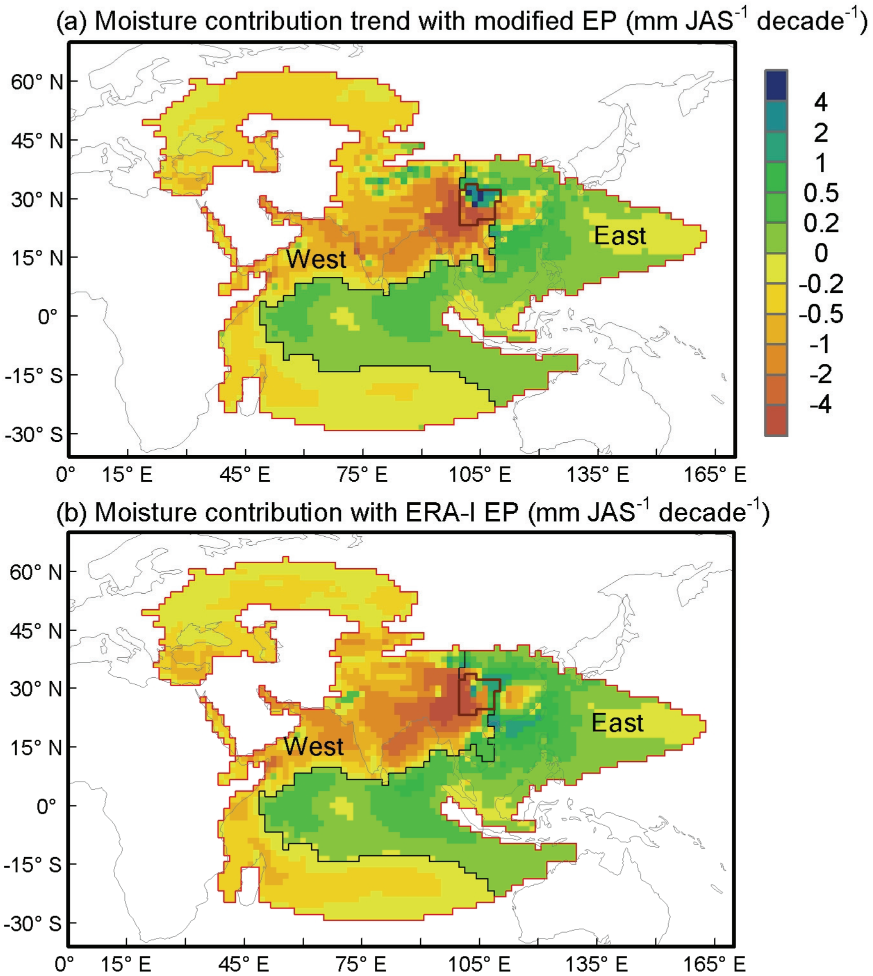

Fig. r5 The trend of the summer moisture contribution from 1979 to 2013 with modified E and P (a) and ERA-I E and P (b). Values outside the major region are not shown.

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.

A: If 100% is set, the moisture contribution area will cover the whole globe. If a small portion, such as 50% is set, the moisture contribution area will be a small area around the study area. We certainly do not want to include the whole globe because the contribution become very small if the contributing area is far away from the study area.

We do not want to set a small number because only a small portion of moisture contribution was accounted for. Thus there is a trade-off because the contributing area and the portion of the moisture contribution. If we select 95%, it will cover most of the globe with many areas with contribution close to 0. Thus we set a number close to 90% which cover most area with relative large contribution. We have provided a 50% region in the revision but we did not analyze the data based on the region because it can only represent half of the moisture contribution to the study area.

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?

A: 1) The declining trend is also significant at 5% level when the two years are removed. The CMA precipitation product is based on ground observations and is quality controlled by CMA. The drying trends and drought in southwest China were reported by many previous studies using independent datasets (e.g., Tan et al., 2016; Barriopedro et al., 2012; Li et al., 2011). Thus, the trend of CMA data is taken as reliable.

2) In general, the precipitation trend in the reanalysis data is not very reliable since precipitation in reanalysis is model output which is not directly constrained by observations (Dee et al., 2011; Berrisford et al., 2011). In Fig. r6, we have plotted the precipitation data from ERA-I together with the ground observation. It is evident that ERA-I estimates have large biases which are much larger than those of CMA. However, the trends are all significant at 5% level and show similar decreasing patterns and magnitudes for both annual mean and summer (JAS) precipitations.

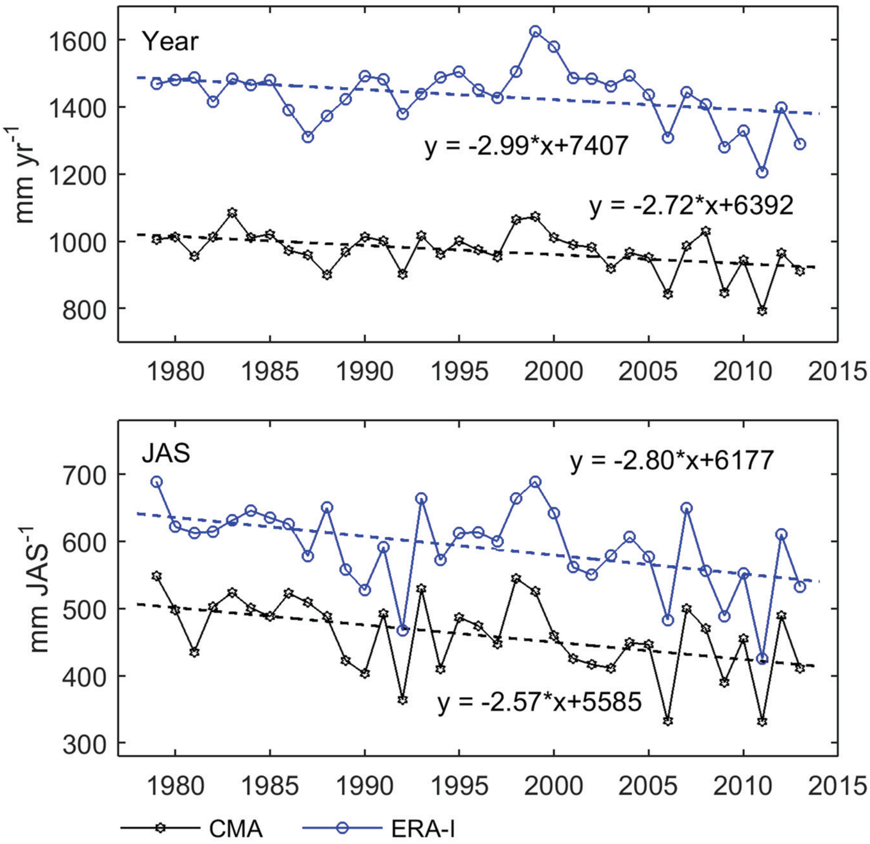

Fig. r6 The time series and trends of annual and summer (JAS) SWC precipitation from 1979 to 2013 with ERA-I and CMA.

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 in the results presented before.

A: We have carefully checked these statements, and kept only those with sound evidences in the revision. We have paid attention to the consistency of the statement in the results section and conclusion section.

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).

A: We have added more references in the introduction and discussion sections, to compare different methods and discuss the possible uncertainties.

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

A: It is the Asian monsoon regions, which in this paper specifically mean regions from the northern Indian Ocean to SWC and from South China Sea to SWC. We have specified the exact regions in the revision.

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

A: Corrected.

10. 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).

A: We intended to say 'providing insights on how the changes in atmospheric circulation may affect precipitation in SWC'. We have rephrased the statement.

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

A: We used the way Trenberth (1991) did to calculate the precipitable water and moisture flux, but the citation should not be placed here. The citation has been moved to section 2.2 in the revision.

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

A: Corrected.

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

A: Yes. We've also clarified it in the text.

14. Pg. 3, L. 25: Please clarify how exactly the rescaling was done in order to ensure reproducibility of your results. What rescaling factor was used?

A: At each grid, there is a CMA value and an ERA-I value for the monthly precipitation. The monthly CMA value is taken as the norm, thus producing a rescaling factor ε for the ERA-I value. Then, all the precipitation values (3 h) during a month of ERA-I are rescaled using the factor ε. But the ε varies with grids and months. We have clarified it in the revised paper.

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

A: All the levels are vertically integrated.

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

A: Changed.

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

A: Changed.

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

A: Deleted.

19. 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

A: We have rephrased the sentences to provide more quantitative detail.

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

A: It has been combined with the above description.

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

A: It has been defined in the revision.

22. 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.

A: The result is independent from the threshold of 0.8mm. The threshold of 0.8mm is use to delineate the key area which contributing moisture to the study area. Figure 3 shows the moisture contribution change in last and first 10-year time period.

We agree that moisture flux change vectors do not have to be related to the moisture contribution change. Figure 3 shows mainly the moisture contribution changes. The moisture flux change vectors were overlapped to provide clues on changes of moisture contribution. As more moisture is transported toward the target area, more moisture may contribute to precipitation in the study area. The relation between moisture divergence and precipitation in the study area is further investigated in Figure 4.

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

A: The field of moisture divergence is calculated directly from the field of moisture flux. We have clarified it the Section 2.

24. 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.

A: At a time scale longer than one month, the moisture balance equation of $P = E - div(Q)$

holds. So P is not only related to moisture divergence, but also E. However, the correlation coefficients between P and E are -0.18, 0.39, and 0.23 for July, August, and September, respectively, lower than those of P and moisture divergence (-0.87, -0.88, and -0.74). This makes moisture transport a more influential factor to P changes. We have added the analysis of the P, E, and moisture flux divergence change in the revision. Our finding is also consistent with that in Li et al. (2013). Li et al. (2013) performed a similar analysis in southwest China which is closely the same area as in this study. In the revision, we have discussed the relationships between P and E.

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

A: 1) The dynamic component of moisture transport shows an increasing trend significant at 5% level and we have changed "obvious" to "significant" in the text.

2) The reanalysis parameters differ according to whether they are produced by the analysis or the forecast. The analysis fields are constrained by the observations while the forecast are produced by the model (Berrisford et al., 2011; Dee et al., 2011). The moisture transport is derived from humidity and wind variables from the analysis fields. Though there are uncertainties, these observation-constrained fields are more reliable than those from model forecast such as precipitation, evaporation, etc. (Berrisford et al., 2011). In addition, ERA-I, as a modern reanalysis, has significantly improved in comparison to the older ERA-40 (https://climatedataguide.ucar.edu/climate-data/era-interim; Trenberth et al., 2011). The interannual variation in moisture transport with ERA-I is rather stable (Trenberth et al., 2011) which gives us more confidence in its application. Nonetheless, current reanalysis provide the best reconstruction of atmosphere records for the climate change research in the atmosphere field. In the revision, we have stated that the results are derived from the reanalysis data and provided a brief discussion on the data reliability of the reanalysis data.

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

A: It has been stated in the revision.

27. 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.

A: We have polished the figure in the revision.

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

A: Yes. We have revised the color bar to reflect it.

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

A: The shading is already restricted to moisture reaching the target region. For the moisture flux, however, it is impossible within the WAM framework to restrict it to the moisture reaching the target region yet.

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