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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Role of vegetation and landcover dynamics on the recycling of water in two endorheic watersheds of NW China (Gansu Province)" *by* M. A. Matin and C. P.-A. Bourque

Anonymous Referee #1

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General comments

This paper aims at analysing the role of vegetation for moisture recycling within two endorheic catchments in China. The paper addresses a research question of relevance for the audience of HESS. The authors have done substantial work to prepare relevant data for the analyses, and explain their methods in detail. However, perhaps due to the great efforts to prepare the input data, the long sections on the input data is overly comprehensive in comparison to the limited texts and figures (and perhaps thoughts) dedicated to the core issue: the link between the vegetation and the moisture recycling. Furthermore, the authors simply take correlation for

causation, and do not make any efforts to back-up the correlation with physical, logical explanations. Thus, unfortunately, key conclusions are not adequately supported by the presented analyses, results, and discussions. For example, one of the paper's key conclusions is that about 90 % of the mountain runoff returns as precipitation from low land evaporation. However, this claim merely relies on the fact that the total water volume from oasis evaporation and mountain precipitation seem to match, and is not at all backed-up by mechanisms of precipitation formation, wind patterns, or comparison to the literature. Another insufficiently supported claim is that "vegetation growth in the oases provides a biotic trigger for the initiation of the precipitation season in the mountains", and that one month of active oasis vegetation is required to trigger the Qilian Mountains precipitation season. For publication, major revisions putting forward evidence to support the claims are necessary. (Alternatively, the authors could also consider addressing alternative research questions that the their current data permit.)

My general comments are as follows:

1. The introduction can be more focused. At present, it contains much information with little direct relevance to the paper, but fail to problematise the current research frontier and fully motivate the research in question. What exactly is not solved by previous research that this present paper manage to? The literature review is also quite absent of a systematic description of water recycling mechanisms and previous moisture recycling studies (e.g., (Gimeno et al., 2012; Goessling and Reick, 2011; Lawrence and Vandecar, 2014; Tuinenburg, 2013)), which can be expected given the research question addressed here.

2. The study area description is very lengthy and can be more succinct. Some information seems redundantly detailed in terms of its relevance for the scope of the study, e.g., the soil type descriptions. The authors could also consider the option to move some of the texts to an appendix.

3. The methods section is lengthy and mainly describes the input data preparation, and not the correlation and comparison analyses on the relationship between vegetation and water recycling. A suggestion is to substantially reduce the data input descriptions in favor of describing the core analyses. Data input processing descriptions could be partly removed and partly placed in for example an appendix. For increased readability and clarity, the authors could also consider adding a separate section called Data, instead of mixing data and data processing description with (currently insufficient) analyses description.

4. The results/discussion and conclusion sections are meagre. The limitations of the paper are not included, there are no comparisons between the authors' findings and that of others, and any future outlook or implications of the findings are also unfortunately missing. The authors also fail to include a discussion on the possible mechanisms that may corroborate their claim. The authors should preferably also include validation of their results, or at the very least a discussion of the possibility to validate their results. For example, what do wind data suggest? Can stable isotope measurements (e.g., Kurita, 2004; Risi et al., 2013; Yu et al., 2007) help validate the results? Are the results in line with modeling studies? Is the recycling ratio of the watershed suggested here exceptional in comparison to other similarly sized watersheds in the world?

Technical/specific comments

1. The title: "...vegetation and land cover..". What's the difference between vegetation and land cover in this case?

2. P.1154, L. 7: DEM is not explained.

3. P.1154, L. 22: Consider using the term "evaporation" instead of "evapotranspiration", see also Savenije (2004). "Evaporation" would also be more consistent to

the authors' later use of the term "evaporated water" to refer to evapotranspiration.

4. P. 1154, L. 22: the word "revealed" seems too strong given the evidence presented.

5. P. 1155, L. 6-8: Please reformulate the sentence "In endorheic basins, . . .". Difficult to understand what is meant at present.

6. P. 1155,L. 27-28: The sentence "The role of vegetation. . ." says nothing more than that scientific literature has described the role of vegetation on soil moisture and runoff. Please consider writing something more meaningful, e.g., what is the role?

7. P. 1159, L. 3: There are two different references to the Penman-Monteith equation. Which of the equations is used?

8. P. 1161, L. 5: Ambivalent what is meant by "Ten landcover maps. . .". Perhaps the authors meant "annual land cover maps. . ." (one for each year) and not ten land cover maps per year? Anyway, it doesn't seem that the ten maps are the end product. If the end product actually used in the analyses is the composite land cover map, please state this more clearly and at the beginning of the paragraph for clarity.

9. P. 1161, Eq. 1: This equation confuses. "Majority" is not a conventional function. Are the authors for example taking the maximum or mean of the majority land cover over the years? What counts as majority? If "majority" is defined as more than 50 %, what happens if no land cover type exceed 50 %? Does one pixel contains land cover fractions of different land cover types, or only one land cover type at a time? A better explanation could be better than the confusing equation.

10. P. 1162, L. 21: What is the rationale to have one threshold for sparse grass, one for coniferous forest, and 0.12 for the rest?

11. P. 1163, L. 21: It's not clear what is meant by "the complementary method".

12. P. 1164, L. 1: Is "yield" the same as "runoff"? If so, please use only one term for clarity.

13. P. 1165, L. 7-11: The sentence starting with "Asynchrony. . ." is unnecessarily long and difficult to understand. Please reformulate.

14. P. 1165, L. 7-11: It is stated that oasis-vegetation starts one month earlier than in-mountain precipitation, thus, suggesting that one month of active plan growth is required to trigger the precipitation. However, it's not clear whether the growing season is always one month ahead despite interannual variations, or if the "one month" is only an average. Please clarify. If the one month of triggering period is an important result of the paper, the authors might want to consider to illustrate this result in one single figure, rather than make the readers guess based on Fig. 3 (which isn't even referred to in Sect. 4.1) and Fig. 5.

15. P.1166, L.6-9: It's not clear whether the authors mean that the correlation between precipitation in the mountains and vegetation/evaporation in the oasis are found within each watershed individually, or if the analysis was independent of watershed borders.

16. A number of sentences in the results and discussion section are formulated as methods description. See for example P. 1166, L. 2-6; and P. 1166, L. 13-14.

17. P. 1166, L. 25-27: "This suggests that the bulk of water originating from the mountains is eventually returned to the mountains as evaporated water." Why is it not possible that the rainfall over the mountains originates from other places than from the watershed just because the volumes happen to coincide? In the next

sentence, the authors also write that this evaporated "water can travel across watershed boundaries", which should suggest that the authors also believe that precipitation in the mountains can come from elsewhere. Moisture recycling studies have shown that recycling ratios are in general low at the local scales, although higher in regions with for example strong orographic effects. Nevertheless, Fig. 5 in van der Ent et al. (2010) shows global maps of regional precipitation and evaporation recycling (i.e., recycling within 1.5 degree x 1.5 degree grid cells). In northwest China grid cells, precipitation recycling ratios are below 5 %, whereas evaporation recycling ratios can be higher. Since the authors claim that the watersheds are in principle hydrologically closed systems (with most of the evaporation returning to the mountains, and "once deposited, surface water is mostly confined to the watershed"), it seems that the authors also implicitly claim that the watershed precipitation recycling should be much higher than 5 %. Can the authors please compare and discuss their results in relation to these types of studies?

18. P. 1166, L. 26: Please specify which water flux or fluxes the word "water" refers to. Does it refers to runoff from the mountains?

19. P. 1167, L. 16: What is meant by "biotic trigger"? Please be more specific in explaining the mechanisms.

20. There are a number of superfluous and unconventional abbreviations that reduce the readability of the paper. For example, NW for northwest, RS for remote sensing, LCOV for land cover composite, and LSP for land surface phenology. They may be convenient for the authors, but cause much inconvenience for the readers.

21. Please avoid multi-letter variable names. For example, actual evaporation should preferably be written as E_a instead of AET. See HESS manuscript preparation guide: http://www.hydrology-and-earth-system-sciences.net/submission/manuscript_preparation.html.

22. Please consider making colorblind friendly figures.

23. With regard to all figures containing subplots, please add subtitles and/or legends in the figures in order to enhance readability. For example, in Fig. 3, put the watershed name to the left of the subplot rows, and add the zone name/number above each subplot column. Another example in Fig. 6: instead of writing "The first plot applies to the Shiyang River watershed and the second to the Hei River watershed." in the caption, add the watershed names to the subplot figures.

24. The authors show the maps for every year in Fig. 4, but do not discuss the interannual spatial variation. The differences between the years are difficult to see from the figures, and since the authors also do not consider the interannual variations important enough to discuss, Fig. 4 can perhaps be collapsed into one mean annual map.

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