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Hydrology and Earth System Sciences
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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

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Reviewer No. 1 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 their current data permit.) Our response: Thank you for your comments; they are all justified. To address your concerns we have completely rewritten the manuscript, from beginning to end. To strengthen our assertions that oases EVI and evaporation are indeed causally connected to the production of precipitation in the high-mountains we conduct a convergent cross mapping (CCM) of relevant variables. As described in the manuscript, CCM is a relatively new method (2012) that allows for the examination of causation between variables. In our case, we see that the causal links are in fact bidirectional, indicating feedback between the variables. We provide explanations for why bidirectional causality is present. This feedback supports our argument that evaporation in the oases is responsible for the production of precipitation in the mountains, and ultimately the return flow to the oases.

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 fails 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

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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. Our response: We have rewritten the Introduction, as requested. We eliminated all non-relevant parts of the original Introduction. We added many new references and texts concerning the description of the problem.

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. Our response: We have removed the section of soil types. We have moved some of the other material (particularly, material related to the landcover types to a new Appendix, addressing the landcover details of the manuscript). With all of the changes to this section, the study area description is now more to the point.

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. Our response: We significantly reduced the methods section. We added a new sub-section related to CCM. Part of the old method sections on landcover was relegated to the new Appendix. Description of data now appears in a new Table (i.e., Table 2 of the new manuscript).

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

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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? Our response: We hope to have remedied this by the complete rewriting of the manuscript. We now address issues of wind direction and other pertinent subject matter. CCM validates our assertions that processes in the oases are responsible for the production of precipitation in the mountains (and vice versa). This validation coincides with statements by other researchers concerning the role of oasis vegetation in recycling of water in the region.

Technical/specific comments 1. The title: "...vegetation and land cover. . .". What's the difference between vegetation and land cover in this case? Our response: We have changed the title to "Relating seasonal dynamics of enhanced vegetation index to the recycling of water in two endorheic river basins in northwest China", and, therefore, eliminate the reference to "vegetation cover". In a later section of the manuscript, however, we refer to "vegetation cover types" as a subset of "landcover types".

2. P.1154, L. 7: DEM is not explained. Our response: We define DEM (digital elevation model) in Table 2, where it is first introduced.

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. Our response: We now use "evaporation" instead of "actual evapotranspiration" or "evapotranspiration".

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4. P. 1154, L. 22: the word “revealed“ seems too strong given the evidence presented. Our response: We think with a CCM analysis and the other changes we made to the manuscript, “revealed” is now not so strong.

5. P. 1155, L. 6-8: Please reformulate the sentence “In endorheic basins,...”. Difficult to understand what is meant at present. Our response: We have revised. We hope the changes will help clarify what we meant.

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? Our response: The sentence was removed and replaced with more relevant sentences.

7. P. 1159, L. 3: There are two different references to the Penman-Monteith equation. Which of the equations is used? Our response: Thank you for pointing this out. The Penman-Monteith equation is addressed in Monteith (1965); we now eliminate the reference to Penman.

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 landcover 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 landcover map, please state this more clearly and at the beginning of the paragraph for clarity. Our response: Yes, that is what we had meant. We have changed the offending text to clarify our meaning. Again, thank you for pointing this out.

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 landcover 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. Our response: We no longer

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include the equation. Hopefully, the text conveys what we mean.

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? Our response: The values are based on an examination of actual values and changes in EVI. In turns out that the value used for the other vegetation cover types were not so different.

11. P. 1163, L. 21: It's not clear what is meant by "the complementary method". Our response: The "complementary method" relates to a method of calculating evaporation (see Matin and Bourque, 2013b). We no longer refer to it in the manuscript.

12. P. 1164, L. 1: Is "yield" the same as "runoff"? If so, please use only one term for clarity. Our response: We had intended "yield" and "runoff" to have different meanings; "yield" is the water volume after within-zone evaporation is subtracted from sum of precipitation and snowmelt within the same zone, and "runoff" is the water volume flowing downslope from the mountains. We modified the text to make that distinction clearer.

13. P. 1165, L. 7-11: The sentence starting with "Asynchrony..." is unnecessarily long and difficult to understand. Please reformulate. Our response: We have rewritten the text as suggested.

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 illustrating this result in one single figure, rather than making the readers guess based on Fig. 3 (which isn't even referred to in Sect. 4.1) and Fig. 5. Our response: The "one month" is an average; we clarify this in the manuscript.

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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. Our response: Correlations are basin-specific.

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. Our response: We have removed them; they were not needed.

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? Our response: We rewrote the section that addresses these points. We believe that recycling ratios should be much larger than the 5% reported in van der Ent et al. (2010). We address this in the revised manuscript.

18. P. 1166, L. 26: Please specify which water flux or fluxes the word "water" refers to.

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Does it refer to runoff from the mountains? Our response: Yes, we clarify this in the revised text.

19. P. 1167, L. 16: What is meant by "biotic trigger"? Please be more specific in explaining the mechanisms. Our response: We replace the terminology with a more direct statement, "This suggests that vegetation growth in the oases, through the production of water vapour, provides an initial triggering of the precipitation season in the mountains." 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. Our response: We have eliminated all abbreviations in the main body of the manuscript. However, we retain some in the figure captions for convenience. Abbreviations are defined at their first usage in the figure caption.

21. Please avoid multi-letter variable names. For example, actual evaporation should preferably be written as Ea instead of AET. See HESS manuscript preparation guide: http://www.hydrology-and-earth-system-sciences.net/submission/manuscript_preparation.html. Our response: Thank you. Variable names have been simplified throughout the manuscript, as suggested.

22. Please consider making colorblind friendly figures. Our response: Because colour blindness occurs across a spectrum of intensity from monochromacy to less extreme, we are unsure of the standard to use.

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. Our response: We have

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adjusted some of the Figures, as suggested.

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. Our response: We no longer use this Figure.

Reviewer No. 2 The title looks novel and interesting. Moisture recycling in inland river basins may be important for understanding the local water cycle. However, while reading the text, I got disappointed. The paper is weak in its conclusion that evaporation in the oases triggers precipitation in the higher source areas. For the substantiation of this conclusion, the authors use correlation (which is not necessarily based on a causal relationship) and the timing of the vegetation growth, which in the oases predates precipitation in the mountains. But this time lag is quite normal in many places in the world. Vegetation development often predates the onset of rain. Moreover temperature depends on elevation. Vegetation will only start to develop when the temperature is above a minimum value. The temperature in the lowland is several degrees higher than in the mountains where vegetation starts later in the season. Moreover, if the authors had studied the literature on moisture recycling (e.g. Van der Ent et al., 2010 and several follow-up papers by this author) then they would have known that the atmospheric moisture source is from the West and that the length scale of recycling is in the order of several 1000 km. Our response: Vast deserts in the area tell you that very little moisture from the far west or south of the study area (due to the blocking of the southeast monsoons by the Qinghai-Tibet plateau) actually reach the study area. The Introductory section of the manuscript has been rewritten to give a better account for why external atmospheric moisture is not a significant component of the water budget in the area. The Introduction introduces new references that point to that fact.

This paper could potentially be interesting to demonstrate the effect of EVI on evaporation and water yield, but then the paper should be completely re-written. An alternative

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title or story line might be: 'The vegetation phenology and its relationship with precipitation and evaporation in two endorheic watersheds in northwest China', or any others representing the content more properly. To support the authors' original argument, the authors would have to collect isotopic and meteorological data, such as wind directions etc. A regional moisture cycling model might also be required to draw the original conclusion. I suggest you study and refer to Van der Ent et al. (2010). Our response: We used CCM to show cause-and-effect. We think this gives much more support to our conclusions.

Finally, the authors violate the important rule of using correct units. The web site of HESS on textual conventions and the correct use of physical dimensions should be followed. This same directive is used by all hydrological journals. All hydrological fluxes (precipitation, evaporation, discharge, etc.) need to be expressed in terms of fluxes: M/T, L/T or L/T. It is absolutely wrong to present a flux as a length! Although at some places in the text you do so correctly, you do it wrongly in lines 15-17 on p1165 (I guess the unit should be m3/year) and in the vertical axes of Fig 6, 7, 8, 9 and 10. This must be corrected. Our response: Thank you. We corrected the violations throughout the manuscripts.

Another issue, but this is a matter of taste, is the use of the term 'evapotranspiration', which although widely used, is considered bad jargon. Evaporation is the correct term, which is the physical term for the transition of liquid into vapour. For the combination of different evaporative fluxes (transpiration, interception, soils evaporation, open water evaporation) one could use the term 'total evaporation'. The addition of the term 'actual' is also redundant since evaporation from a catchment is always actual. Our response: OK, we made the changes as suggested.

Finally, please don't use the abbreviation AET, which in your text can be simply replaced by the term evaporation. There is no need for this jargon abbreviation. There is also no need for the abbreviation PET. This is the potential evaporation, which can be very well symbolized by Ep. Moreover, in equations it is not allowed to use multi-

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letter variables, as is explained in the 'symbols' convention of HESS. So in Eq.(2) for snowmelt, I suggest to use the symbol S, and for evaporation the symbol E. In the caption of Table 3: "Evaporation (E) as a percentage of the sum of precipitation (P) and snowmelt (S)". Likewise change Figure 2 and captions of Fig.7 and 9. Our response: OK, we made the appropriate changes.

Further specific comments: 1. The authors obviously neglected some important publications on tracing moisture origin by isotope in the Heihe River [Zhang et al., 2009; Wu et al., 2010; Zhao et al., 2011], and on topography-based landscape classification and hydrological modelling in the Heihe River [Gao et al., 2014]. I suggest the authors do refer to these relevant publications. Our response: In our Introduction, we refer to publications by Gates et al., 2008a,b; Ma et al., 2008; Ma et al., 2009; Ma et al., 2012; and Huang and Wen, 2014.

2. The authors shall use the proper and correct scientific terms. For example, in P1154, L7, it is better to change 'DEM-height values' into 'elevation'. In P1163, L1, the 'cumulative Our response: OK. We made the required changes.

3. The study area section should be separated into two sections. One is the study site section, and another is the data section. Our response: We incorporate reference to the data in a new Table 2. 4. Equation 1 and 2 use multi-letter variables. According to HESS's conventions, please use single-letter variables with subscript. Our response: OK. We made the required changes.

5. P1164, Equation (2): It is strange to put the variable k as an exponent. The k can easily be added to the subscripts: i,j,k. Our response: The equation is no longer part of the revised manuscript.

6. Section 4.3: The authors mentioned that 'Vegetation influences on precipitation'. However, it could as well be the other way around 'precipitation influences vegetation'. There may be interactions between vegetation and precipitation. But from the content of this section, I do not think the results support the authors' argument. Furthermore,

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in P1166, L10, the authors mentioned that 'water vapour production by the oases is responsible for the generation of precipitation in the Qilian Mountains'. This conclusion requires more supportive information, both observations and model simulation. Our response: We feel the use of CCM helps to support our conclusions.

7. In P1167, L15-17: '. . .vegetation growth in the oases provides a biotic trigger for the initiation of the precipitation season. . .". Do your results really support this conclusion? I am not convinced. Our response: All details in the manuscript from the correlation analysis, isotopic work by others, prevailing wind direction, CCM, timeseries plots, etc. are all consistent with the idea that oasis vegetation has a role in the production of precipitation in the mountains.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/12/C1679/2015/hessd-12-C1679-2015-supplement.pdf

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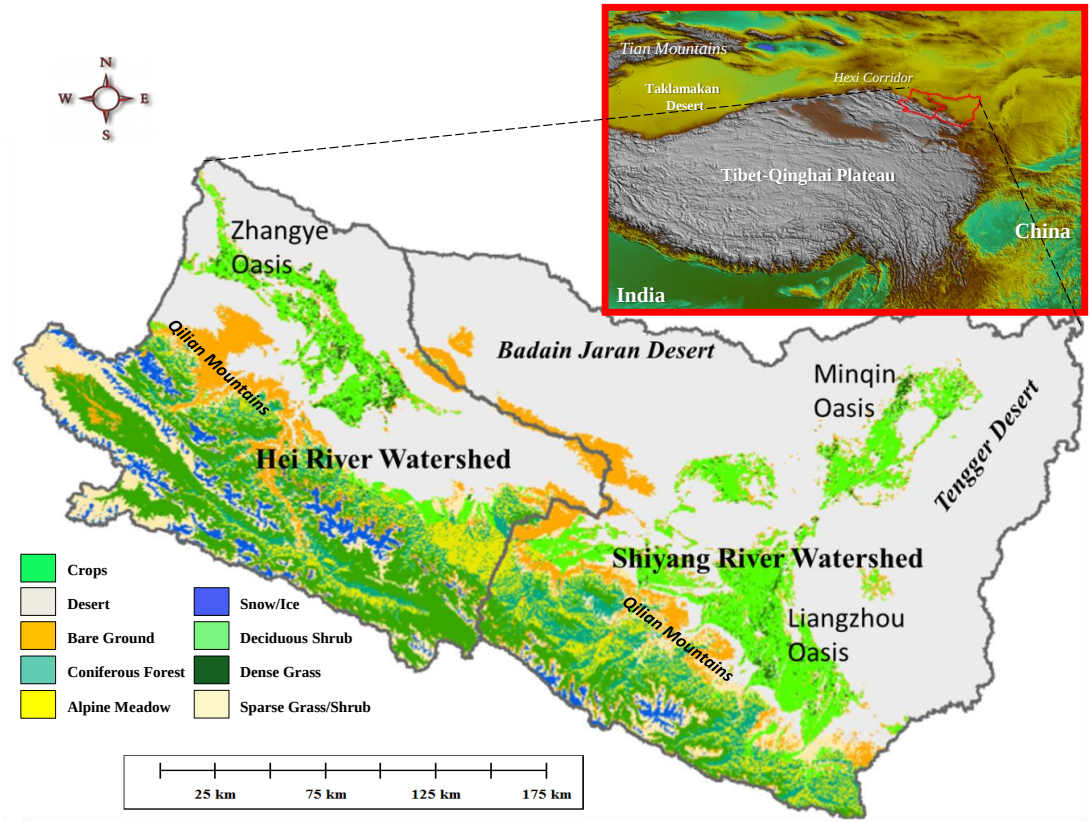

Fig. 1. Map of study area

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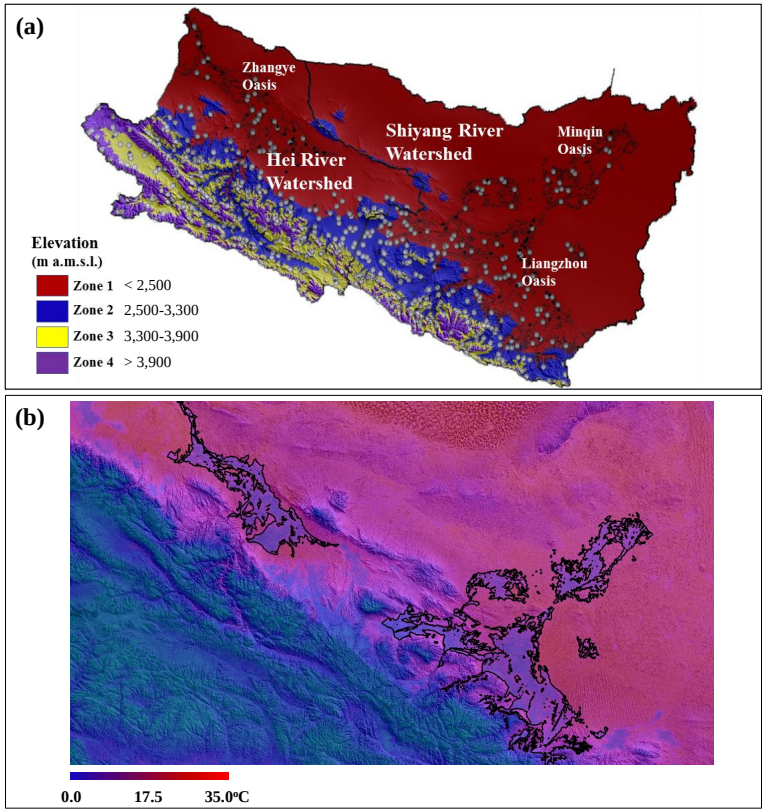

Fig. 2. Sampling & air temperature map

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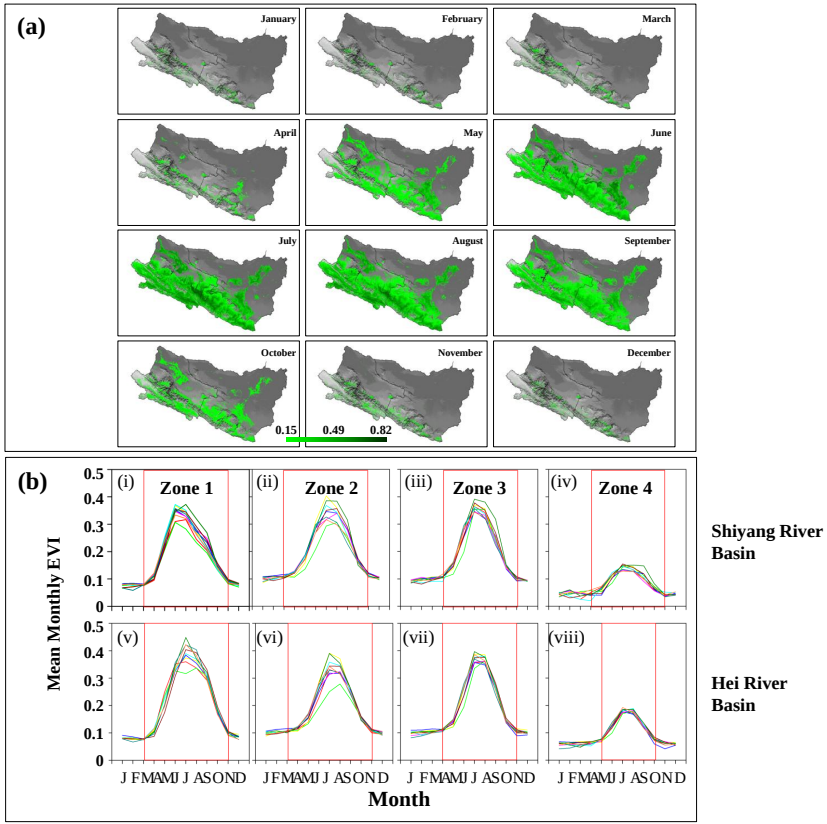

Fig. 3. Vegetation dynamics

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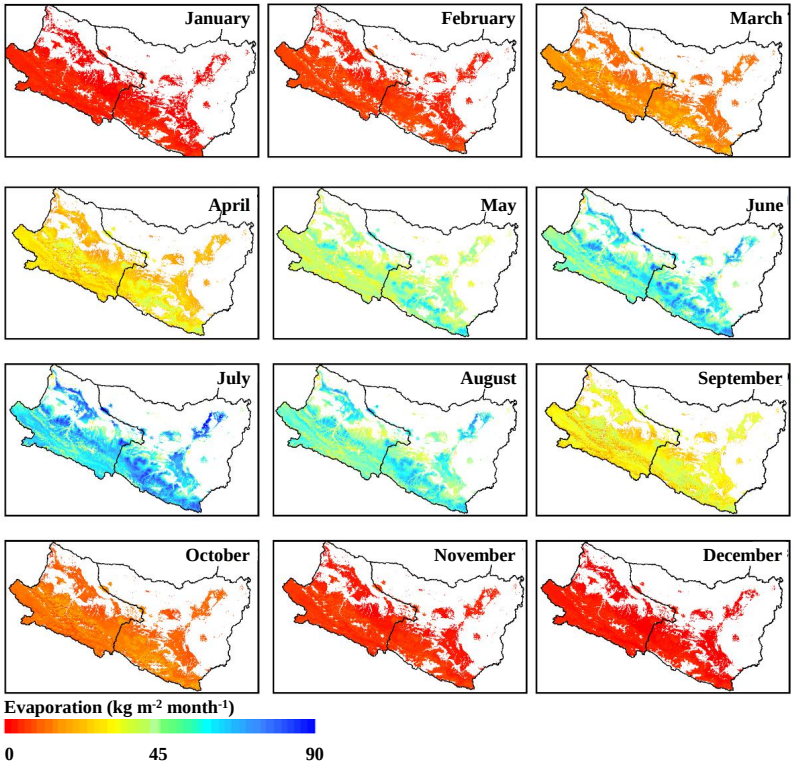

Fig. 4. Evaporation

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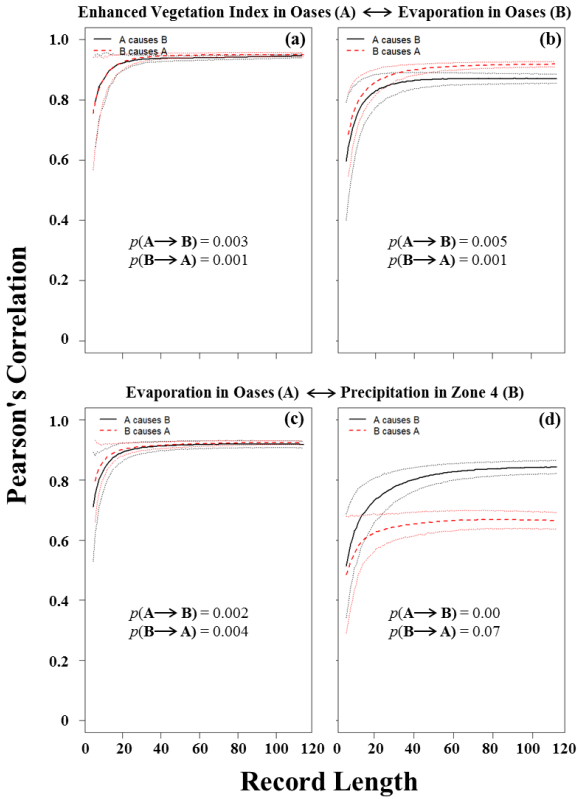

Fig. 5. Convergence Cross Mapping

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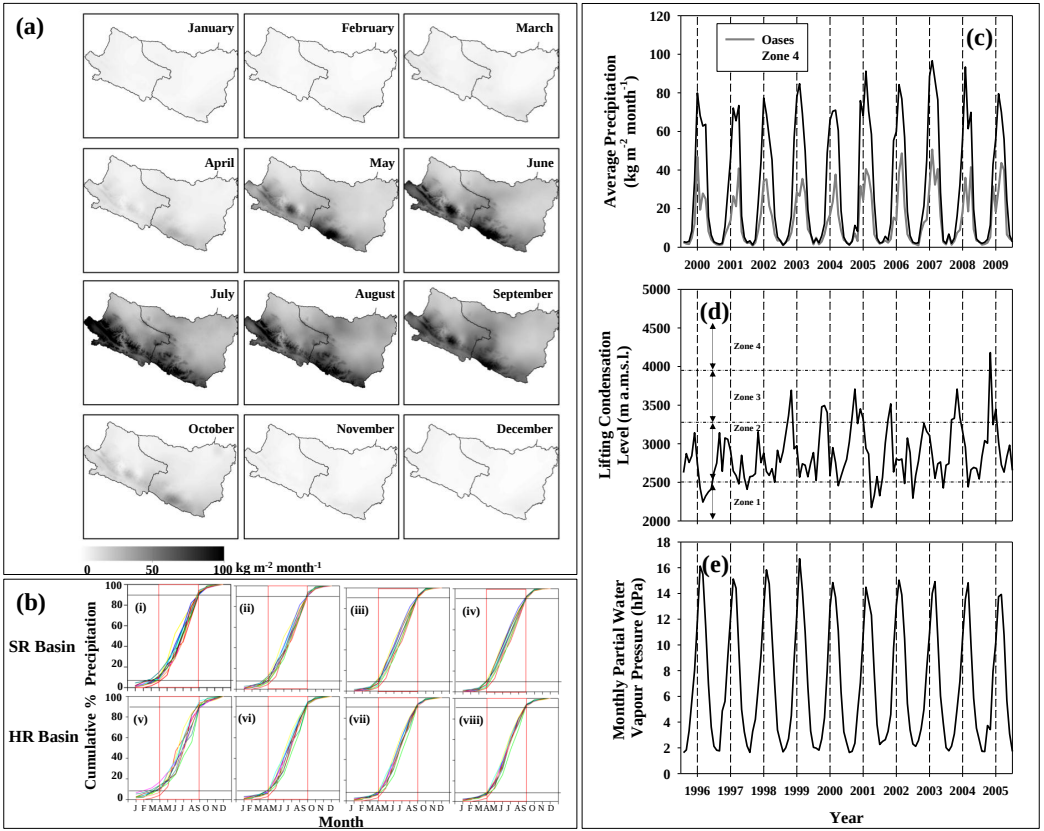

Fig. 6. Precipitation and associated variables

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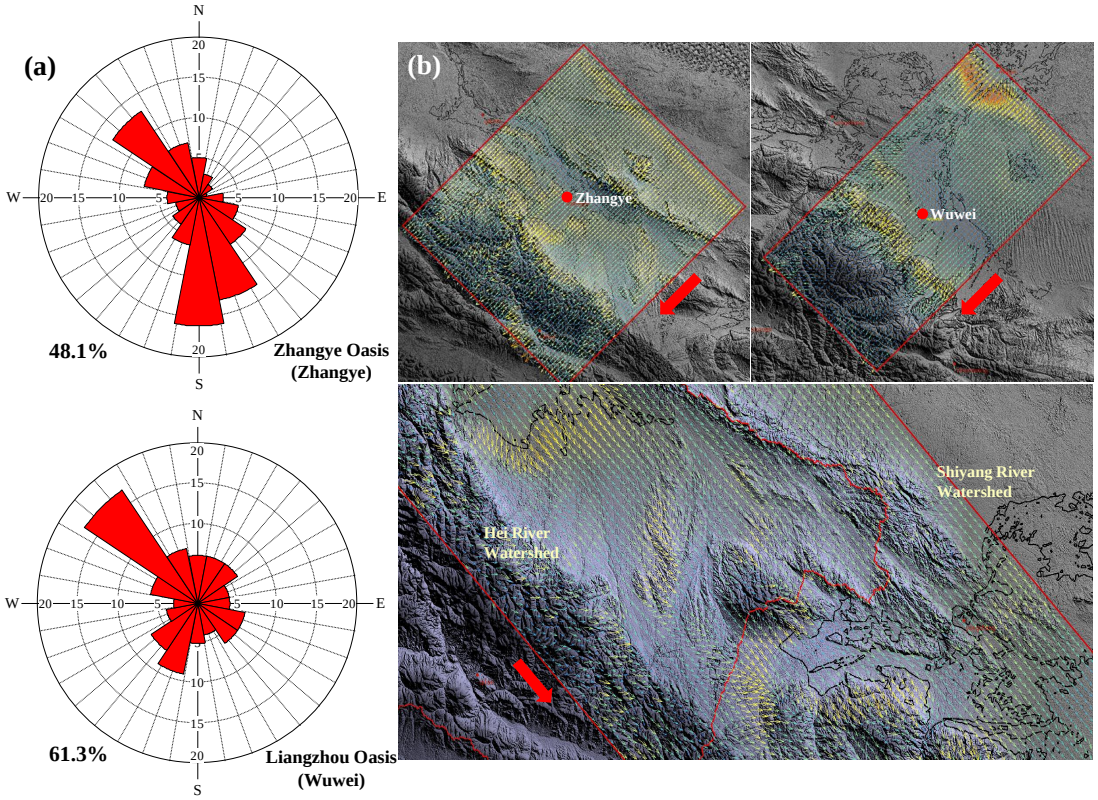

Fig. 7. Wind direction

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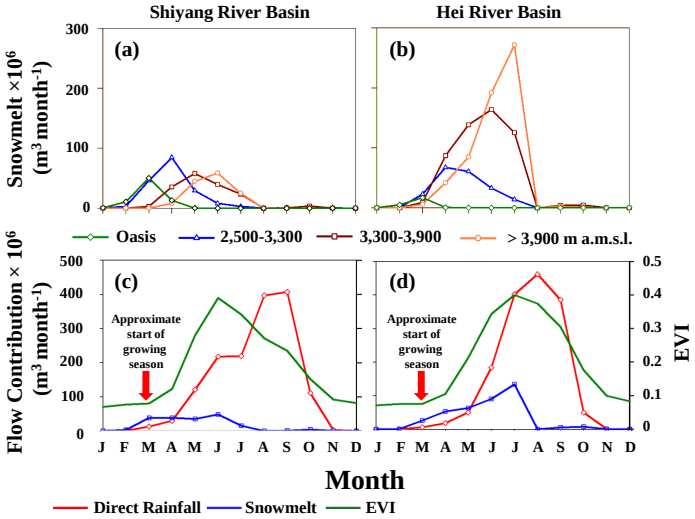

Fig. 8. Water-flow partitioning

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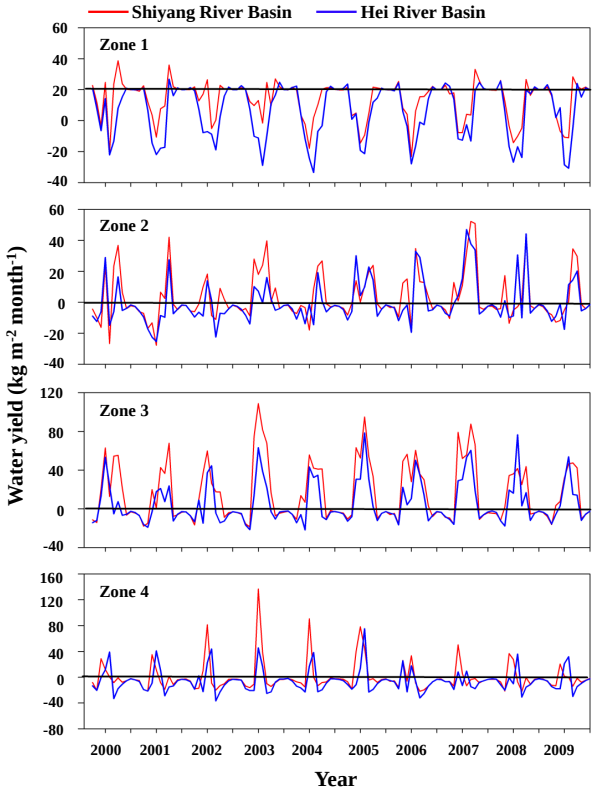

Fig. 9. Within-zone water yields