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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Influence of climate variability on water partitioning and effective energy and mass transfer (EEMT) in a semi-arid critical zone" *by* X. Zapata-Rios et al.

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This paper presents a detailed analysis of how climate patterns are changing for a New Mexico watershed and estimates the impact of these changes on net energy inputs (as water or carbon) into the system. The detailed presentation of the trends over the past decades for multiple climate related drivers (precipitation, air temperatures, snowpack dynamics) makes a strong case that the system is changing. By estimating how these changes translate into EEMT trends, the authors suggest that these changes may have broad implications for the structure and function of the watershed. The clear presentation of how multiple trends combine to impact EEMT is interesting and takes the 'next

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step' towards assessing the implications of climate trends. However the presentation of EEMT relies heavily on previous papers and it is not always clear in this paper what the implications of changing EEMT at the timescales assessed in this study would be. A more thorough or perhaps nuanced discussion of what changing EEMT at these timescales might mean would strengthen the paper. The methods are generally appropriate but I do have some concerns with spatial interpolation of precipitation data and with explanation of vaporization trends - I will detail these below.

pg 7935 line 20 - if you have not read prior EEMT papers this might not be obvious- effective precipitation in some fields is defined as P-surfaceE so not P-E-Transpiration. Its also unclear how EEMT integrates water and carbon. Since EEMT is not, as yet, widely used and given that EEMT is discussed at length in the following sections, some additional explanation of EEMT (a few sentences) here would be helpful.

Page 7941 Line 10-20- It is not clear why the Horton Index is presented here if the goal of this section is to compute EEMT - which relies only on U - which is directly derived from hydrograph separation (Eqn 3). This adds unnecessary complexity to the methods section. I see later that the Horton Index is used - it would be useful to introduce this so that the reader understands why the Horton Index is being presented. In general, the paper could be more focused - in several places patterns are discussed without being necessarily connected with the goal of the paper that was set up in the introduction.

Line 15 A simple statement that Eppt is the energy input through precipitation would be helpful here for clarity

Page 7942 - Add a bit more information here on what "explored" meant - there are some complexities in correlating MODIS with an annual climate metrics. Annual relationships typically cannot account for multi-year effects and disturbance history (and the Southwest is a highly disturbance prone environment). Thus is would be useful to know how good (in a sentence or two) these regressions from Rasmussen and Tabor

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(2007) in order to evaluate their use here

Pg 7944 - line 2 - State whether these are significantly different given the confidence bounds on trends

I have some concern here re: the impact of errors in scaling precipitation across the basin from two precipitation stations or PRISM. Errors in precipitation interpolation in this region can be large - and spatial patterns of precipitation may also be changing - Note that the analysis of precipitation trends found that the precipitation trend at the Señorita Divide station was substantially less than trends at the other station (59mm/decay vs 73mm/day). Basin-scale precipitation is used for both EEMT modeled and EEMT empirical and for many other metrics that are computed in the paper. Some discussion of how errors in precipitation interpolation and changing precipitation patterns might influence results should be included.

Section 3.4 - What is the motivation for this section - while I certainly can understand why looking at correlations with discharge is of interest to hydrologists - it isn't clear how this fits with the overall goal of the paper - (of course discharge is indicative of EEMT_prc patterns and so you are implicitly getting at those by looking at discharge - but then to go back and look at correlations with variables such as P that are included in calculating EEMT precip seems a bit circular). In general the paper needs to be more focused so that the goal of each step in the analysis is clearly set up in the introduction

MaxSWE and length of snow on the ground are likely to be highly correlated which is problematic for multivariate regression how was this dealt with?

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line 10-15 - The explanation of evapotranspiration trends is somewhat unsatisfactory. It is worth noting that trends in pan evaporation noted in Barnett (2005) occur in both snow and non -snow dominated systems, thus it is not clear how this citation supports the point that in snow-dominated systems ET is expected to go down. Barnett

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(2005) explanation include feedback to the atmosphere that might not be expected to occur at the scale of this study. Other studies in snow dominated system have found the opposite (increasing ET with increasing temperatures) (Goulden et al., PNAS) and modeling studies show why ET may go up or down with increasing temperatures in snow-dominated systems (Tague and Peng, 2014). While I agree with the point that changing the timing of snowmelt plays a role, it is not the only thing going on. It is also worth noting that decreased vaporization could also be due to declines in vegetation biomass which alters both interception evaporation losses and transpiration. Declines in biomass might be expected given observed declines in NPP reported. This explanation is different from declines due to improved water-used efficiency associated with rising CO2 and is also a likely explanation. In general the explanation of evapotranspiration declines given here could be better developed.

Pg 7953 My sense is that the key question here is where these rates of change in EEMT are significant with respect to landscape change - and a what scale - are these big numbers or little numbers? I'm not sure I am convinced that the time scale of these trends actually results in a substantial effect. The supporting correlations between EEMT fluxes and landscape structural characteristics do not imply causation and in particularly they do not say anything about the time -scales over which this causality would occur. Perhaps these are longer term effects. I do not disagree with the point that changing EEMT is interesting but I think the explanation of what this means could be better developed

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