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 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.

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.

We appreciate this comment as it helps us focus our discussion. We have revised our paragraph to reflect that while the correlation between EEMT and CZ/ landscape structure does not necessitate causation, previous work has shown that these correlations are widespread, strong, and thus have significant predictive ability (Pelletier and Rasmussen, 2009a,b; Rasmussen and Tabor, 2007; Rasmussen et al., 2005; Rasmussen et al., 2011; Zapata-Rios, 2015a). Although we do not know the times scales of CZ change, these results suggest that decadal differences in EEMT are similar to the differences between convergent/ hydrologically subsidized and planar/ divergent landscapes, which have been shown to be very different in vegetation and CZ structure (Pelletier and Rasmussen, 2009a,b; Rasmussen and Tabor, 2007; Rasmussen et al., 2005; Rasmussen et al., 2011; Pelletier et al., 2013; Zapata-Rios, 2015a). This leads to the question, will CZ structure change as predicted by EEMT-structure relationships, and if so how fast those will those changes occur? This question is still unknown but actively studied in the Catalina Jemez River Basin Critical Zone Observatory (Lines 443-469)

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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-surface E so not P-E-Transpiration.

Thanks for this comment that help us to clarify the variables that are considered in the calculation of EEMT. Eppt represents the energy of water that percolates into the Critical Zone as indicated in line 50 in the manuscript. Thus, there are different methodologies how Eppt can be quantified. In methods we present two methodologies (line 151-165). Eppt calculated as effective precipitation is used in only one of the methods to calculate EEMT. As explained in lines 210-220 in the manuscript, the quantification of Eppt in the $EEMT_{model}$ was estimated as the difference between Precipitation and Potential Evapotranspiration, known in hydrology as effective precipitation and traditionally used to quantify monthly water balances (Arkley, 1963). On the other hand, the quantification of Eppt in the $EEMT_{emp}$, was calculated based on baseflow (U) estimations, since baseflow is used as an indicator of water that has effectively percolated into the critical zone (manuscript Line 191-196). Our response regarding the spatial interpolation of precipitation and the explanation of vaporization trends has been organized and addressed in a paragraph below.

Arkley, R.J.: Calculation of carbonate and water movement in soil from climate data, Soil Sci., 96, 239-248, 1963

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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.

The main sources of energy that drive CZ development are: (i) solar radiation fluxes, (ii) water that percolates into the CZ (iii) carbon compounds from primary productivity (iv) landscape physical and chemical denudation v) additional material fluxes such as anthropogenic inputs, dust and atmospheric inputs (Phillips, 2009; Smil 1991; Rasmussen, 2011). From all of these energy sources it has been proved that the energy of water and carbon compounds are orders of magnitude larger than the rest (Phillips, 2009; Rasmussen, 2011). Therefore, for the EEMT quantification only the energy associated with water and carbon are considered. Energy from both water and net primary productivity are essential on CZ processes altering soil genesis, mineral dissolution, solute chemistry, weathering rates among others (Birkeland, 1974; Neilson, 2003). This explanation can be found in the lines 48-59 and lines 151-165.

Birkeland, P.W. 1974. Pedology, weathering and geomorphological research. Oxford University Press, London.

Neilson, R. P. 2003. The importance of precipitation seasonality in controlling vegetation distribution. P. 47-71. In J.F. Weltzin and G.R. McPerson (ed.) Changing precipitation regimes and terrestrial ecosystems. A North American perspective. University of Arizona Press, Tucson.

Anderson S.P., Von Blanckenburg F., White A.F.2007. Physical and chemical controls on the critical zone. Elements, 3, 315-319

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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.

Thank you for this comment. The Horton Index section has been erased from the methodology and results.

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

Done! (line 191)

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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(2007) in order to evaluate their use here. Pg 7944 - line 2 - State whether these are significantly different given the confidence bounds on trends

The word explored meant the correlation process between MODIS and climate metrics (lines 206-208). As with any spatial and temporal regression between climate and MODIS data, there are potential errors associated with disturbance as the review comment highlights, as well as interannual lag effects, interseason variability in timing of water availability, and other factors. We also note that the significant statistical relationship, albeit with variability and error, likely captures these effects on this time scale when no large scale disturbance occurred. (Lines 312-316)

Done! Lines 248 through 253 indicate significance of trends

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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.

The two SNOTEL stations were mainly used to study micro-climate variability and they were not used to scale precipitation across the basin. PRISM was used to estimate the mean basin scale Precipitation and mean basin scale EEMT as indicated in both the introduction in lines (96-100) and in the methodology (lines133-135; 158-165). PRISM is a weighted regression technique that accounts for physiographic factors affecting spatial climate variables and it has been extensively used in the US. (Daly et al., 1994; Daly et al., 2002) (lines 162-165)

Only $EEMT_{model}$ as indicated in the methodology was derived from the precipitation PRISM dataset (Daly et al., 2008). The assumption is that the 800 m PRISM data provides a reasonable spatial estimation of precipitation (of course that assumption is better for winter than summer).(lines 164-165) $EEMT_{model}$ was quantified as an average valued based on catchment scale long-term average records as indicated in lines (76-78) The EEMT model presented in the Chorover et al., 2011 paper has a relative mean prediction error of ~25% - relative to the predicted value. However, we are using mean trends in Precipitation and EEMT at the catchment scale so we believe that even though these variables may have errors the mean trends presented in this study are close to the true values. (Lines 228-231)

Daly, C., Neilson, R.P. and Phillips, D.L.: A statistical-topographic model for mapping climatological precipitation over mountainous terrain. Journal of Applied Meteorology, 33, 140-158

Daly, C., Gibson, W., Taylor, G., Johnson, G. and Pasteris, P.: A knowledge-based approach to the statistical mapping of climate, Climate Research, 22, 99-113, 2002.

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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?

We agree with this comment, and section 3.4 has been removed to focus the paper only in the EEMT trends

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Page 7950 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(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 CO_2 and is also a likely explanation. In general the explanation of evapotranspiration declines given here could be better developed.

We appreciate this comment that help us improve the discussion about vaporization in our study. We have revised our discussion and added to the manuscript the following text. The spatial and temporal variability in total evapotranspiration may exhibit significant variability (Tague and Peng, 2013) and contrasting evapotranspiration trends directions have been reported in different studies around the world (Barnett et al., 2005). In the Jemez River basin a snow dominated system the decrease in ET (45 mm/decade) is likely a result of the mismatch of the timing of energy and water fluxes. Earlier snowmelt, while plant water demand remains relatively low, may reduce evapotranspiration by reducing plant/atmospherically available water later during the growing season when demand is higher(Barnett et al., 2005). The decrease in vegetation biomass indicated from the MODIS data at this basin can also significantly contributed to alter transpiration water losses. An increase in forest water-use efficiency (ratio of water loss to carbon gain) with increasing concentrations of carbon dioxide can also contribute as another cause to the decrease of evapotranspiration fluxes (Keenan et al., 2013) Modeling studies over a hundred years support our finding that evapotranspiration has been decreasing in the west arid area of the US (Liu et al., 2013) However, ET may increase with temperature in some snow dominated systems if stored soil or groundwater remains available to plants either locally or at downslope locations (Goulden, et al., 2012; Brooks et al. 2015)" (lines 381-396).