Response to Anonymous Reviewer

We very much appreciate anonymous reviewer for valuable comments that helped us to improve the manuscript. Note: The text provided in italics will be incorporated in the revised draft.

Suggestion # 1.

The authors used equation (4) to evaluate the impact on streamflow from precipitation, potential evapotranspiration, and storage change. In this equation, the authors estimated storage change as "(DSt - DS)/DS". I don't think that it is a good choice. According to the definition of this manuscript, DS is the long-term average of storage change, and it means that DS generally approaches zero in many basins (if there is no storage change). Therefore, it will lead to infinity for the third term on the right side of equation (4). In addition, the sign of storage change elasticity depends on the sign of DS. Consequently, we can't judge whether increasing storage leads to decreasing streamflow according to positive storage change elasticity. In that case, I suggest using storage replacing DS, or using storage change replacing "(DSt - DS)/DS".

Author's reply: Yes, we do agree that at annual scale (equation 4), the DS may tent to zero. In this article, we have hypothesized that, if DS is zero, then we can always go back to equation 2 (two parameter equation), which is defined for situations were DS is zero. But, the assumption of DS =0 is not always valid. For example, in regions were the anisotropy ratio (Vertical hydraulic conductivity/Horizontal hydraulic conductivity) is not negligible, the ground water losses do occur, indicating that DS ≠0 (Wang, 2014). However, we do agree that the DS is calculated as a residual (P-Q-AET) and likely to have uncertainties due to usage of data from different sources. So, this may result in either underestimation or overestimation of storage change than it is derived in this article. Hence, until more high quality climate information is available, this can be deemed as a hypothesis that remains to be tested. However, in this study we do neglect the regions for which DS =0 at all scales. But, when it comes to seasonal scales, the DS would theoretically not be zero since, in a particular season; water balance would contain deficits or excesses depending on occurrence of rainfall events and change in temperature.

Suggestion # 2. The structure of this manuscript. In Section 3, the first paragraph represents how to obtain the results. It is better to remove it into Section Methodology. Similarly, first paragraph of Section 3.5 should be removed. In P.4, the sentences from line 9-21 review the researches on the elasticity, and it is better to remove them into the Section Introduction.

Author's reply: Thanks for the suggestion. These changes would be incorporated in the revised Manuscript.

Suggestion # 3.

Figure 8 shows that the potential evapotranspiration elasticity is larger than 0 in some basins and less than 0 in the other basins. It indicates that increasing potential evapotranspiration leads to increasing streamflow in some basins but leads to decreasing streamflow in the other basins. On the causes for the opposite impacts on streamflow, more explanations and discussions are required.

Author's reply:

This is a very relevant comment which requires a dedicated and separate study. However to support our findings we are including the following discussion in the revised manuscript

"*In previous studies also, certain catchments have shown positive streamflow elasticities due to potential evapotranspiration [Andréassian et al., 2015, Yang et al., 2014]. The positive PET elasticity may be caused by the local climate feedback. According to previous studies (e.g., (Koster et al. 2004; Guo et al., 2006 Mei and Wang, 2011), the central USA has strong land-atmosphere coupling strength. The PET plays an important role in the linkage of soil moisture and precipitation in the land-atmosphere interactions. Based on the positive land-atmosphere interactions, the increased soil moisture would lead to a cascading effect of increase of temperature (indirectly PET) and precipitation. The increased precipitation would therefore lead to the increase of Streamflow. In this notation, the PET has a positive relationship with precipitation, which would lead to a positive PET elasticity. The positive PET elasticity are within these hotspots in summer season".*

Suggestion # 4.

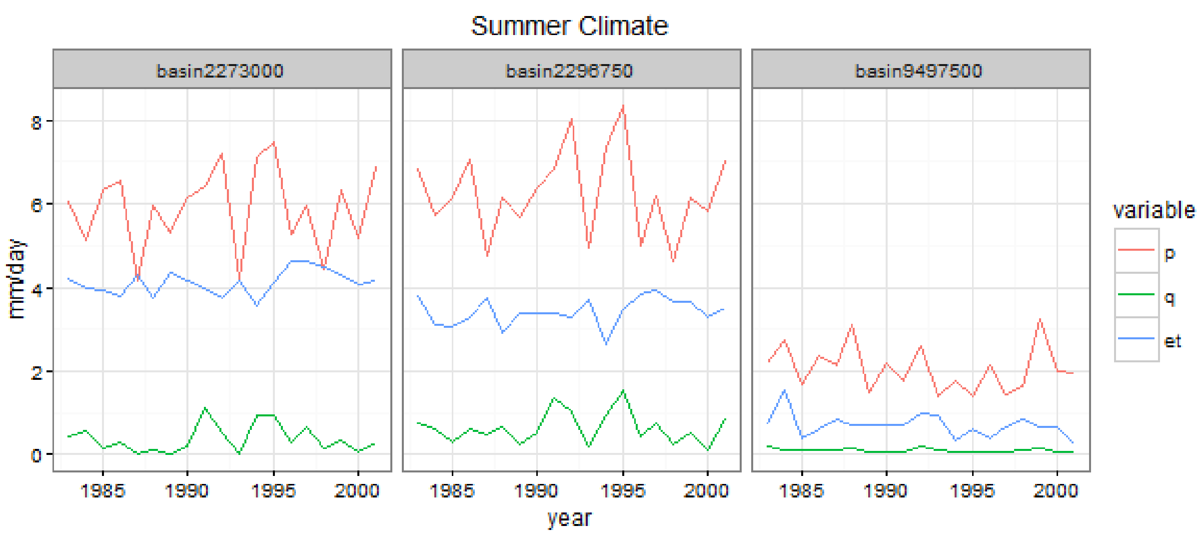
Figure 9 shows that the storage change elasticity is larger than 0 for many basins in spring and summer. It means that declining storage will lead to a decreasing stream- flow in those basins. At the same time, the storage change elasticity is less than 0 for other basins in spring and summer, which means declining storage resulting in increasing streamflow. The underlying mechanisms of the phenomenon should be explained and discussed.

Author's reply: We will include the following discussion in the revised manuscript to address your suggestion.

"The seasonal DS elasticities indicate that ground water storage act as a natural reservoir and subsequently supply and store the streamflow during various seasons. For example, during summer when the temperatures are high and water requirement is more, ground water supplies water to the streamflow resulting in a positive elasticity in most of the MOPEX basins. Whereas, in winter and spring soil recharges itself with water indicating negative elasticity values. However, we observed that in western USA, the negative elasticity magnitude peaks during winter unlike the rest of US MOPEX basins. This may be mainly because groundwater

contribution to streamflow is inversely correlated to snowmelt runoff (Huntington and Niswonger, 2012). Hence, it possibly has high negative elasticity values when the snow accumulates in winter. Whereas, when the snowmelt runoff starts in the spring it starts contributing to streamflow indicating positive elasticities."

We selected one MOPEX basin in the southern region of Florida and two basins in the state of New Mexico with the following basin ids, 94975000, 2273000 and 2296750 respectively. We investigated the summer season flows, since we suspected some anomalous behavior due to their negative elasticity values. We plot the seasonal averages of the selected time period. The streamflow and evapotranspiration are lower than rainfall amounts. The values seem normal and do not indicate an anomalous behavior. However, we do acknowledge the fact that the streamflow in those catchments is influenced by storage facilities (Wang and Hejazhi, 2012), therefore additional research is expected to address whether this is a natural behavior of the catchment.



Minor comments: 1. On the meanings of AIC and BIC, more explanations are required, i.e. why "the preferred model is the one in which the AIC value would be minimum."

Author's reply: We have modified the methodology section to be clearer on AIC and BIC definitions. The revised text is as follows:

We evaluated our trivariate elasticity model (Equation 4) against the bivariate elasticity regression model (equation 2) using Akaike information criterion (AIC) (Akiake, 1973) and Bayesian Information Criterion (BIC) (Schwarz, 1978).

AIC is given by equation as

$$AIC = -2\sum_{i=1}^{n} \log\{g(x_i \mid \theta_k\} + 2k$$

(5)

Where n is the number of observations; g(x) can be either equation (4) or equation (2); θ_k *are the streamflow elasticities of the corresponding models and k is the number of parameters. In our context, AIC offers a relative estimate of the information lost when elasticity model is fitted to the data to represent the processes involved. As, when building any statistical model, our aim is to model the processes with minimum information loss (better goodness of fit), the preferred model is the one in which the absolute value of AIC value would be minimum. As evident from the equation (5), we can see that the first term in the equation tends to decrease with the model parameters, whereas the second term increases. Hence, AIC penalizes for the increase in number of parameters.*

Another metric useful for calculating information loss similar to AIC is called Bayesian Information Criterion (BIC). It is computed using following equation:

$$BIC = -2\sum_{i=1}^{n} \log\{g(x_i \mid \theta_k)\} + \log(\mathrm{n})k$$

(6)

As we can see that the BIC is similar to AIC except that the second term is multiplied by a factor of 0.5ln(n) with respect to AIC. As a result, BIC leans more towards less parameterized models. Hence, BIC should also be interpreted in a similar way as AIC. The only difference is that BIC gives more weightage to the number of parameters in a model and penalises more for the modified trivariate modeling our context. Overall, the preferred model would be the one which has both minimum AIC and BIC value.

Minor comments # 2. P.2, line 5-6, Wand and Wang (2011) should be Yang and Yang (2011).

 # 3. P.3, line 8, please check the reference Jiali et al., 2014.

 # 4. Figure 1, the unit of the legend is missing. 5. P.2, line 19, P.7, line6, and so on, “lesser” should be “less”.

Author’s reply: The following suggestion would be implemented in the revised manuscript

References:

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