Interactive comment on "Catchment-scale groundwater recharge and vegetation water use efficiency" *by* Peter A. Troch et al.

Peter A. Troch et al.

patroch@hwr.arizona.edu

We thank the reviewer for his/her thorough and insightful review. See below our response. Words in between parantheses are reviewer's comments and our response is in regular print.

"Does the Horton Index actually indicate vegetation water use efficiency? The Horton index has been used in several past studies, but I am unsure if it really can/should be interpreted as catchment-scale vegetation water use efficiency. The paper states that ET dominates total terrestrial evaporation, but most estimates indicate that ET is ~60% of global terrestrial E (thus ~40% has little to do with transpiration). Second, the HI definition is very closely related to a catchment's water balance; a ratio of V/W seems

almost equivalent to using a ratio of E/P because E ≈ P- Q, and P ≈ P-Qd (because quick flow Qd is only a small fraction of total P for many MOPEX catchments). This tight relationship between E/P and HI (and AET/P) is also shown in the inset panel in figure 2 of this manuscript. I guess the question that I struggle to answer myself is: how can I be confident that HI actually informs about water use efficiency, and are results attributed to water use efficiency not largely the result of correlations with known controls on the water balance such as climate aridity?"

To this reviewer's first point about the term vegetation water use efficiency and whether T dominates in ET, we are of the believe that T is the dominant component in catchment vaporization, as many studies indicate [Coenders-Gerrits et al., 2014; Evaristo et al., 2015; Scott Jasechko et al., 2014; Maxwell and Condon, 2016; Schlesinger and Jasechko, 2014]. But, since our study is not about whether T does or does not dominate catchment vaporization, which also includes interception, bare soil and open water bodies evaporation, we will change the term vegetation water use efficiency into Catchment Vaporization Efficiency (CVE). Hopefully this removes the issue.

To this reviewer's second point on the definition of the Horton index, it is true that HI is related and strongly correlated with AI (=PET/P), the HI accounts for the loss of water through direct runoff and the removal of water through vaporization from the root zone, excluding that infiltrated water from producing streamflow through deep percolation and lateral groundwater flow. This definition is in line with the definition introduced in Horton (1933). Whereas AI expresses the competition between available energy and available water, the HI defines the second step in the partitioning of water at the catchment scale, viz the amount of water actually vaporizing vs. the amount of water wetting the catchment. Therefore, the AI and HI serve different purpose in analyzing water balance dynamics. The former quantifies the driver, whereas the latter expresses the response in the root zone and determines how much water is available for deeper storage. This response is not only a function of climate, as this reviewer suggests, but depends also on soil (e.g. water holding capacity) and topographic (e.g. slope) properties. Our work

clearly illustrates that climate aridity, along with mean catchment slope and elevation are the dominant factors affecting the CVE and long-term groundwater recharge.

"The link between groundwater recharge/baseflow and the Horton Index seems trivial thus are reported correlation really meaningful? In line 98-188 the Horton index is outlined, and its link with expected GW recharge is clarified. While I found this useful to read, it also appears to say that the definition of the Horton Index is directly connected to baseflow (see lines 104-133, and later stated in the discussion "the HI is estimated from baseflow separation and is then used to estimate average baseflow conditions". The argumentation that the statistical relationships shown in the study are not the result of spurious correlation is not convincing (to me). It would be helpful if you actually line out the "several arguments that go against this statement". In addition, the used argument that "we use predicted HI from climate and landscape properties to estimate average baseflow and long-term recharge." is not a clearly explained argument, but a statement without explanation (but when I try to formulate an argument using this statement myself I end up in a similar circular problem as you stated yourself earlier in this paragraph)."

Thank you for sharing your concern with us. Our proposed approach is broadly based on two steps. First we illustrate that HI and average long-term baseflow, assumed tantamount to average long-term groundwater recharge, are statistically significantly correlated and the mathematical form of the relationship is estimated in this step. Second, given the current known difficulties in estimating long-term groundwater recharge for both ungauged and gauged catchments, we have an opportunity to estimate long-term groundwater recharge if we know HI, as during step 1 we found the two are strongly correlated. How should we independently estimate HI? For addressing this issue, we found a strong and statistically significant relationship between HI and aridity index, mean catchment slope and elevation through multiple linear regression analysis. Now using aridity index, mean catchment slope and elevation (for knowing these three parameters, baseflow information is not required), we propose a relationship that predicts

HI using only aridity index, mean catchment slope and elevation. Important to note that, in step 2 we predict HI without knowing baseflow. We then use the predicted HI to estimate long-term groundwater recharge, which is otherwise hard to estimate. This is precisely the contribution we make to the literature. This is the argument that go against the statements of spurious correlations, and we will add text to the revised version of the paper to make this clearer. Âă "Why does it matter that maximum total and deep catchment storage are mainly positively correlated? This finding is stated as one of the main results of the paper, but it is not clear to me what we learn from this relationship?"

The positive relationship between maximum total and deep storage is of great significance, given the objectives of our paper, which is to understand the dynamics of deep storage and develop a practical method to estimate long-term catchment-scale groundwater recharge estimate. We should, in general, expect the two storages be positively correlated. A negative or no correlation between the two storages may represent some external effects such as groundwater pumping or long-term snow cover affects that can affect an expected positive relationship between the two, and thus the long-term estimate of groundwater recharge. Âă "This study relies on linear fits for baseflow recession (and the inference of storage changes), but is this assumption realistic? Past studies that characterized baseflow recession using MOPEX data indicate that most catchments operate non-linearly. For example, Ye, Sheng, et al. "Regionalization of subsurface stormflow parameters of hydrologic models: Derivation from regional analysis of streamflow recession curves." Journal of hydrology 519 (2014): 670-682. Is assuming a linear relationship problematic for estimating storage dynamics or is this still accurate under non-linear conditions?"

We are not the first to use the linear reservoir assumption [Arciniega-Esparza et al., 2016; Peña-Arancibia et al., 2010; van Dijk, 2010]. A recent study [van Dijk, 2010] suggests the linear reservoir assumption is an acceptable assumption for a number of catchments located in varying climate settings and for catchment of varying sizes.

Furthermore, the author also note that the non-linear reservoir assumption sometimes leads to physically unrealistic results. Therefore, we used linear reservoir assumption for each of the 247 MOPEX catchments included in our study using their 23 years of catchment-scale daily hydrological fluxes. Having said that, it wouldn't be too complicated to relax this assumption and use baseflow recession analysis, as in Brutsaert and Nieber (1977), to estimate the maximum deep storage. We chose not to keep the method simple. The final results (the accuracy with which we can estimate average recharge rates) seem to support this decision.

The comparison with USGS data is not convincing. First, it is stated that the method for the estimates rely of different principles, which seems only partly true. The HI may not explicitly have baseflow index in its definition, however, it's definition is obviously strongly correlated with baseflow (as the paper already acknowledges). Therefore, I cannot really see this comparison with the USGS data as a validation using independent data. Second, I agree that regional differences in GW recharge are mostly explained (i.e. R^2 = 0.77). However, predictions at individual sites seem to diverge strongly from the USGS estimates. Thus, maybe site predictions of the HI baseflow are not so good. It would be fair to point this out better."

A fundamental misunderstanding is that the reviewer thinks that we predict HI from base flow. We predict HI using the aridity index, mean catchment slope and elevation. Then we use the predicted HI to estimate long-term groundwater recharge. On the contrary, USGS long-term natural groundwater recharge is estimated using the product of baseflow index- a ratio of baseflow to total flow- and long-term annual streamflow. Thus, the differences between the two methods are clear. ÂăÂă "The comparison with Q50 is unclear. It is assumed that long-term average baseflow conditions can be made from the 50th percentile of the flow duration curve (Q50), "but that this choice was more an intuitive guess than an informed decision". If this is all context given about the GW recharge estimate, I have no idea what this comparison means (because I have not idea what the Q50 estimate means)."
It seemed to us that Q50 was an intuitive guess, as we wanted to develop an easy to use method that is applicable in many catchments. When we analyzed the relationship between the long-term average baseflow and the FDC percentiles, it became clear that Q60 is a better estimate for average baseflow. We do not know how general this is, though, so more research is needed to figure this out. Meanwhile, the method based on Q50 still seems to work, at least for the MOPEX catchments used in our study. Âă "Total storage is a misleading term. This paper uses "total storage" to represent something that is more often referred to as "dynamic storage" (e.g. see Kirchner 2009, WRR). Total storage seems misleading to me, because total GW storage in the landscape is much larger. Something like "total dynamic storage" would be more useful."

Thank you for pointing it out. We qualified our terminology in terms of a catchment's total and deep storages. Both are suggested to represent dynamic storage components in the Introduction section. The total storage is now referred to in our work as total dynamic storage. Âă "The study relies on the assumption that baseflow is a good indicator if streamflow originates from GW. While the event water vs groundwater separation is a fixture in many hydrology textbooks, we also know that taking a closer look (using isotopic data) often tends to indicate that this assumption is not accurate. What are the implications for your work?"

We are not sure what the reviewer is trying to say here, but we think (s)he refers to the fact that often the water that is evacuated during rainfall-runoff events is typically older than one would expect. We agree with this general finding, but we don't see how that impacts our results. Our results are for long-term average recharge, and at these timescales, the issue of event vs. pre-event water doesn't play a role. Âă "L54: does Scanlon et al, actually study the long-term WB, or solely GW recharge rates?"

Methods such as chloride mass balance and tracer (tritium) pulse in the subsurface consider site specific water balance implicitly, the long-term water balance component in the Scanlon et al. study is also implicit.

"L94-96: I am unsure this assumption is really justified. Maxwell and Condon report $62 \pm 12\%$ ET. Jasechko et al. 2013 reports high ET ratios, but there are well known issues with this analysis: (see: Coenders-Gerrits, A. M. J., Van der Ent, R. J., Bogaard, T. A., Wang-Erlandsson, L., Hrachowitz, M., & Savenije, H. H. G. (2014). Uncertainties in transpiration estimates. Nature, 506(7487), E1.), whereby studies provide substantially lower ET ratios (see for example: Sutanto, S. J., van den Hurk, B., Dirmeyer, P. A., Seneviratne, S. I., Röckmann, T., Trenberth, K. E., Blyth, E. M., Wenninger, J., and Hoffmann, G.: HESS Opinions "A perspective on isotope versus non-isotope approaches to determine the contribution of transpiration to total evaporation", Hydrol. Earth Syst. Sci., 18, 2815-2827, Âăhttps://doi.org/10.5194/hess-18-2815-Âă2014, 2014. And Schlesinger, W. H., & Jasechko, S. (2014). Transpiration in the global water cycle. Agricultural and Forest Meteorology, 189, 115-117.)"

First of all, we would like to thank the reviewer for suggesting the three articles. A thorough review of the reviewer's cited publications, however, only supports the assumption made in our study that transpiration component is the dominant fraction of total ET water loss, rather than the issue raised by the reviewer. For example, based on a review of 81 studies, Schlesinger and Jasechko [2014], suggest transpiration is $61\pm15\%$ of ET on an ecosystem scale. Sutanto et al. [2014] suggest the ET partitioning in terms of T and E components depends on measurement method (similar to the findings reported in Schlesinger and Jasechko [2014]). The isotopic methods suggest T component represents 70% or higher fraction of ET. The authors also suggest that many land surface modeling studies report a lower fraction of T of the total ET, and based on this observation they suggest reassessment of the parameterization made in the current land surface and global climate models. Interestingly, in a recent study by Chang et al. [2018], it is clearly shown through simulation of catchment-scale eco-hydrological processes that the cause of predicting a lower T/ET ratio in many land surface models is the lack of thorough representation of lateral groundwater flow processes and the diffusion of water vapor into soil. When the authors represented the two processes more rigorously in their models, they were able to predict T/ET fraction

closer to the fraction estimated independently using the stable water isotopes. Finally, a review of S. Jasechko et al. [2013], comments made by Coenders-Gerrits et al. [2014] and response submitted by Scott Jasechko et al. [2014] only suggest that Jasechko et al. have defended and stood by their T/ET fraction estimates. Therefore, we have not observed the so called "well known issues with this analysis" by the reviewer based on his suggested citations.

Again, to be clear, the point we made in our work is that several studies note transpiration is the major component of the ET water loss. Furthermore, we would like to restate that our work builds on the existing literature (involving numerous publications in reputed journals, i.e. extensive literature review on the subject).

"L227: How variable where the determined k-values between years, within individual catchments? Were those variations somewhat realistic (since one can expect them to be fairly constant, right?)."

Figure 1 shows that the interannual variability of the obtained k values is quite small (all values of the coefficients of variance are much less than 1, except for one catchment). We mention this already in the manuscript and will consider to add this figure to the revised version.

"Line 235: What makes them independent? It seems that both are derived from streamflow observations, and not from independent sources (such as actual well measurements)?"

The total dynamic storage (previously referred to as total storage) is estimated using the water balance method, which involves all components of a water balance equation and the assumption that total change in catchment storage for any hydrologic year is zero. On the contrary, the deep storage is estimated from first estimating the baseflow time series from the observed streamflow time series using the one-parameter low-pass filter and then multiplying the maximum daily baseflow to the reservoir constant. Thus, the two storage estimates are independent.

References

Arciniega-Esparza, S., J. A. Breña-Naranjo, and P. A. Troch (2016), On the connection between terrestrial and riparian vegetation: the role of storage partitioning in water-limited catchments, Hydrological Processes, 1-6, doi: 10.1002/hyp.11071. Chang, L.-L., R. Dwivedi, J. F. Knowles, Y.-H. Fang, G.-Y. Niu, J. D. Pelletier, C. Rasmussen, M. Durcik, G. A. Barron-Gafford, and T. Meixner (2018), Why Do Large-Scale Land Surface Models Produce a Low Ratio of Transpiration to Evapotranspiration?, Journal of Geophysical Research - Atmospheres, 123, doi: 10.1029/2018JD029159. Coenders-Gerrits, A. M., R. J. van der Ent, T. A. Bogaard, L. Wang-Erlandsson, M. Hrachowitz, and H. H. Savenije (2014), Uncertainties in transpiration estimates, Nature, 506(7487), E1-2. Evaristo, J., S. Jasechko, and J. J. McDonnell (2015), Global separation of plant transpiration from groundwater and streamflow, Nature, 525(7567), 91-94. Jasechko, S., Z. D. Sharp, J. J. Gibson, S. J. Birks, Y. Yi, and P. J. Fawcett (2013), Terrestrial water fluxes dominated by transpiration, Nature, 496(7445), 347-350. Jasechko, S., Z. D. Sharp, J. J.Gibson, S. JeanBirks, Y. Yi, and P. J. Fawcett (2014), REPLYING TO A. M. J. Coenders-Gerrits et al. Nature 506, http://dx.doi.org/10.1038/nature12925 (2014), Nature, 506(13 February), E2-E3. Maxwell, R. M., and L. E. Condon (2016), Connections between groundwater flow and Transpiration partitioning, Science, 353(6297), 377-380. Peña-Arancibia, J. L., A. I. J. M. van Dijk, M. Mulligan, and L. A. Bruijnzeel (2010), The role of climatic and terrain attributes in estimating baseflow recession in tropical catchments, Hydrology and Earth System Sciences, 14(11), 2193-2205. Schlesinger, W. H., and S. Jasechko (2014), Transpiration in the global water cycle, Agricultural and Forest Meteorology, 189-190, 115-117. Sutanto, S. J., B. van den Hurk, P. A. Dirmeyer, S. I. Seneviratne, T. Röckmann, K. E. Trenberth, E. M. Blyth, J. Wenninger, and G. Hoffmann (2014), HESS Opinions "A perspective on isotope versus non-isotope approaches to determine the contribution of transpiration to total evaporation", Hydrology and Earth System Sciences, 18(8), 2815-2827. Troch, P. A., G. F. Martinez, V. R. N. Pauwels, M. Durcik, M. Sivapalan, C. Harman, P. D. Brooks, H. Gupta, and T. Huxman (2009), Climate and vegetation water use efficiency at catchment scales,

Hydrological Processes, 23(16), 2409-2414. van Dijk, A. I. J. M. (2010), Climate and terrain factors explaining streamflow response and recession in Australian catchments, Hydrol. Earth Syst. Sci., 14, 159-169.

———————————————————
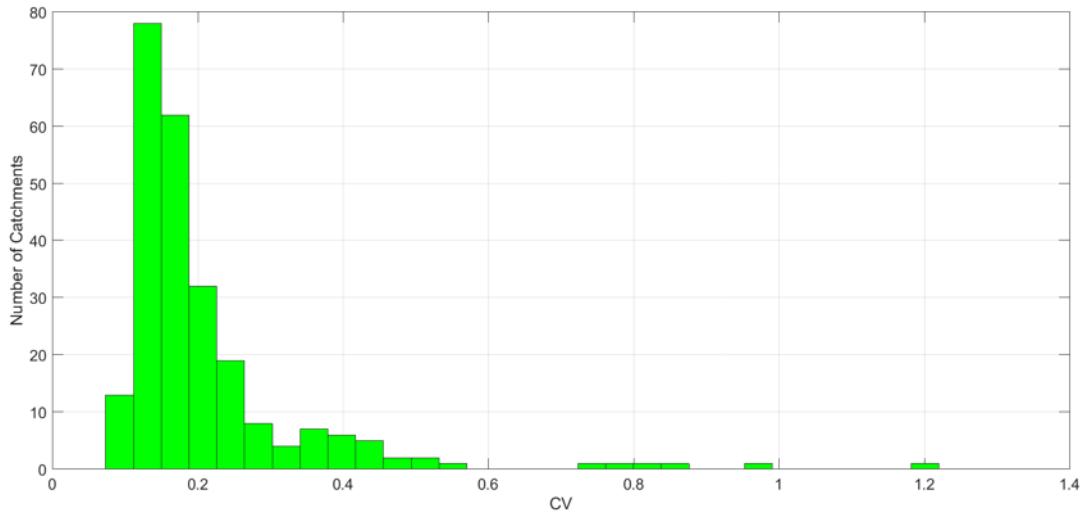
Fig. 1. Histogram of the coefficients of variance (CV) of the annual k-values for all catchments