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Discussions

Interactive comment on "Global synthesis of forest cover effects on long-term water balance partitioning in large basins" *by* Daniel Mercado-Bettín et al.

Daniel Mercado-Bettín et al.

daniel.mercado@udea.edu.co

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Response to Anonymous Referee 2

Interactive comment on "Global synthesis of forest cover effects on long-term water balance partitioning in large basins" by Daniel Mercado-Bettín et al.

"This manuscript presents a very interesting hypothesis about the impact of forest cover on long-term partitioning of water between evapotranspiration and runoff for 22 large basins around the world. However, as I outline below I believe the analysis, data and methods require further explanation and revision to justify publication and to strengthen

the case for the proposed hypothesis. I have chosen not to comment on the speculated causes of the proposed hypothesis as I believe this would best be done once the observational basis of the hypothesis is stronger."

Thank you for your interest in our manuscript and your suggestions. The additional suggested analyses were accepted and added in the new version of the manuscript. We also clarify some concerns related to data and methods.

Major comments

"Analysis: The key figure in this manuscript is Figure 2c (repeated in 4c), which presents a gradual increase in runoff ratio with increasing average forest cover until the runoff ratio reaches âĹij0.5. This figure is the basis of the proposed hypothesis. In the figure the individual catchment runoff ratios are presented as box-plots by basin and the basins are ordered by increasing average fraction of forest cover. It is not clear which two variables the LOESS smooth is applied to – one variable is the runoff ratio values, but the other variable could be either average forest cover for each basin or a dummy variable to indicate the different basins. My concern with Figure 2c is that the apparent levelling off of runoff ratio to âĹij0.5 when the fraction of forest cover reaches âĹij0.5 may be an artefact of grouping the runoff ratios by basin. I think a more convincing presentation of this data would be to plot each catchment individually, rather than group the catchments by basin, as each basin contains catchments that have a range of runoff ratios, forest covers, aridity (potential ET / P), P and R. Plotting each catchment on XY scatterplots of runoff ratio vs forest fraction and runoff ratio vs aridity (coloured by forest fraction) would remove the possibility of an artificial grouping influencing the results. I also think the plot of runoff ratio vs aridity (coloured by forest fraction) could present strong evidence to support, or contradict, the proposed hypothesis that high forest cover results in an even split of P between E and R. In this plot, if for a given aridity the runoff ratio is observed to increase with increasing forest cover then this would support the current conclusions of this manuscript. However, if for a given aridity the runoff ratio is observed to be unrelated to forest cover then this would not support the current conclusions of this manuscript. I think it is very important to compare runoff ratio and forest cover for catchments with similar aridity, to remove confounding the comparison by mixing water and energy limited catchments together."

Thank you for your appropriate and accurate suggestion. The LOESS smooth was applied to the mean values of k for the 22 basins (P4L20), to analyze possible general patterns in the k-values of the basins. We are aware that internal (to each basin) variations among k values are normal (P4L12-15). Our overall intention in the paper was to assess the effects of forest cover on large basins, even when internal variability in water balance partitioning occurs within them. Consequently, we decided to group sub-basins within larger basin categories. Yet, following the reviewer′s suggestion, we implemented the same analysis without grouping the data, and similar results were found. According to your suggestion and to support our results and discussion, we added 4 new figures (or one single panel-plot) using individual catchments to the revised version of the manuscript: 2 smooth-plots containing "runoff ratio vs forest fraction and runoff ratio vs aridity"; and 2 smooth-plots between forest cover vs. k in both, energy-limited and water-limited basins. The addition of these new figures improves clarification and explanation to our results, and provides improved support to our proposed hypothesis. We produced the figures using both the initial basins considered in the paper as well as only free flowing river basins. The results are, in general, consistent in both cases.

4 important facts of each figure:

- *The figures runoff/rainfall vs. Forest support the pattern found in Figures 2 and 3 in the manuscript. They are showing the E-dominated and P-halved patterns.*
- *The figures K (runoff/rainfall) vs PHI (ETP/Rainfall) reflect that the Budyko based hypothesis is not accounting to all processes responsible of the pattern found.*
- *The figures runoff/rainfall vs. Forest using only energy-limited basins*

(ETP/rainfall<1) support the found patterns (E-dominated and P halved).

- *The figures runoff/rainfall vs. Forest using only water-limited basins (ETP/rainfall>1), in general, support the found patterns (E-dominated and P halved). The pattern is not so clear when using free-flowing rivers. This is likely related to the fact that there are no basins representing forest fractions between 0.13-0.25 (this is not the case when using the original data).*

"Data: I have several concerns about the data used in this study outlined below. River regulation: The results presented in the main body of the manuscript are based on catchments that include regulated and heavily modified catchments. The authors do provide a set of largely similar results for "free flowing" catchments in the Supplementary Material. However, given the aim of the manuscript is to understand the role of forest cover on long-term partitioning of water in catchments, I would have expected that only "free flowing" catchments would be used in this analysis. The use of regulated or heavily modified catchments adds an extra level of uncertainty to the results that is best avoided. Since the authors have free flowing catchments, I strongly recommend they base their analysis on those rivers only."

We selected these 22 basins (some of them, heavily modified basins) because the mainstream is big enough to analyze the spatial behavior of k among the basins. However, we understand your concern in the way that, using these data could "add an extra level of uncertainty". Accordingly, following your recommendation, the revised version of the manuscript includes only free-flowing rivers in the main manuscript, while the entire data set is presented in the supplementary material.

"Precipitation data: The authors use TRMM-3B42 and ERA-Interim reanalysis data to estimate mean annual precipitation for the period 2001-2012. However, the authors do not cite any evidence that these data sets are consistent with catchment average precipitation estimates based on observed station data for the catchments investigated. How representative are these two products of catchment average precipitation for these catchments?"

In the revised version of the manuscript, we include appropriate references to support the use of each particular precipitation dataset on each region.

"Snow-melt equivalent: discharge data were modified for snow-melt equivalent in three basins (Mackenzie, Lena Vitim). How was the snow-melt equivalent discharge identified? The contribution of snow-melt to mean annual runoff in these catchments could be very high. Even if the contribution of snow-melt equivalent can be estimated accurately, I am not convinced that removing the influence of snow-melt from these catchments is reasonable for this analysis. The presence, or absence, of forest cover influences snow accumulation and melting, so forest cover plays a role in the long-term water balance of catchments that receive snow. The role of forest cover in catchments that receive snow should not be ignored in a global synthesis, so I recommend that the influence of snow-melt equivalent not be removed from the discharge data. Accepting this recommendation would also remove the issue of how to identify snow-melt equivalent discharge."

In the original version, we had decided to subtract the snow-melt equivalent (accumulated in winter) from discharge on these basins to exclude the potential regulatory effect that this process has on base-flow. However, we agree that forest influences snow accumulation and melting, exerting effects on the long-term water balance. Following the reviewer′s recommendation, in the revised version, we do not remove the snow-melt equivalent from discharge.

"Catchment area: the discharge data from the various source data sets will have a reported catchment area for each catchment. However, the precipitation and potential evaporation data are estimated for catchment areas derived from GTOPO30 and STRM DEMs. Deriving catchment areas from these products is perfectly reasonable. However, it is important to report whether the DEM based catchment area differs from the reported catchment area associated with the discharge data. As the discrepancy

between the DEM and reported areas increases, the precipitation and potential evaporation data becomes less representative of the true area over which the discharge was generated. How large is this discrepancy? If it is $< 5\%$ then that would be re-assuring. If it is $> 10\%$ then that would call into question whether the data from that catchment should be used in the analysis."

Thank you for your suggestion. We made sure that all differences between the source data and calculated drainage areas were lower than 10%. In addition, as a part of the basin delimitation process, we adjusted the DEMs in some regions with large flat areas (e.g, the Amazon basin), using the original streamflow network from the data source, such that errors were minimized. These clarifications are included in the revised version of the manuscript.

"Selection of basins: I believe the hypothesis should be tested over a wider selection of catchments, particularly catchments in energy limited environments. If largely forested catchments in energy limited environments demonstrate runoff ratios âĹij0.5 then the evidence for the hypothesis would be more convincing."

We agree with the reviewer that more data would be desirable to further improve the strength of our conclusions. We use the largest collection of basins with topographic, climatic, hydrologic and vegetation cover data available for us when the analysis were performed. We include multiple data sources (as described in the methods section). In the revised version, we include only free-flowing river basins and, therefore, the number of basins decreases. However, even with a more limited number of basins, the main observations remain valid, which leads us to believe that when more data becomes available, the overall behavior should hold.

Minor comments

"Page 3: please note that the potential evaporation estimate from GLEAM v3.0a is based on Priestly-Taylor." *Clarification added in the revised version*

“Figure 1: the Sava river has a runoff ratio (k) approaching 1 – is this physically realistic? I suspect not.” *Thank you for the observation. This number was derived from the original data. However, since it seems physically unrealistic, we excluded this basin from the analysis.*

“Page 7, line 12: “receive a P-input that exhibits small variability and a similar mean value” – it is important to clarify that this statement relates to the small variability in mean annual precipitation between the catchments within the basin. This region of Australia actually receives precipitation with high interannual variability, so it is important to be clear about which variability is being discussed.” *We clarify in the revised version that we refer to spatial variability, as highlighted by the reviewer.*

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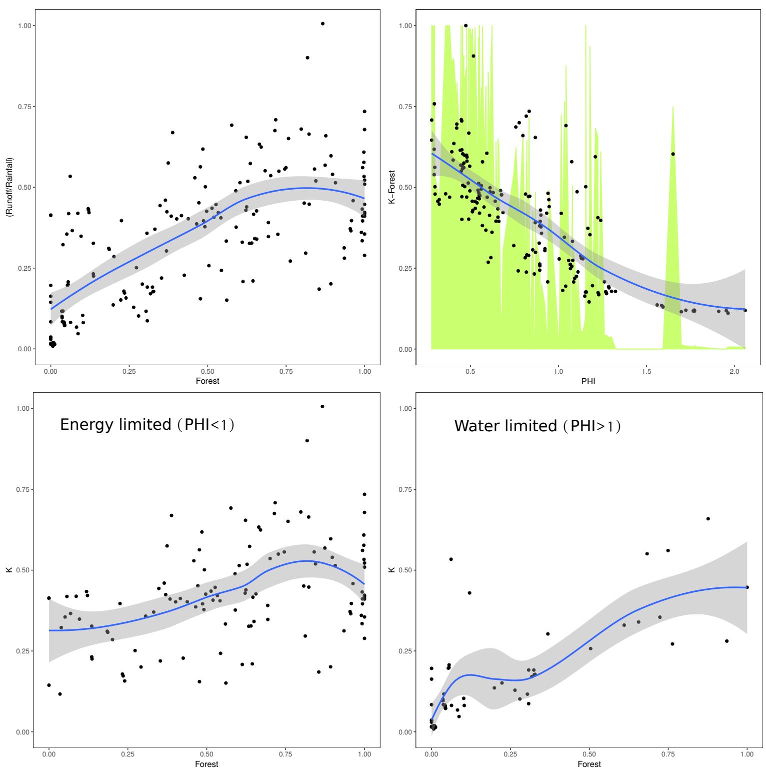

Fig. 1. Panel with original rivers

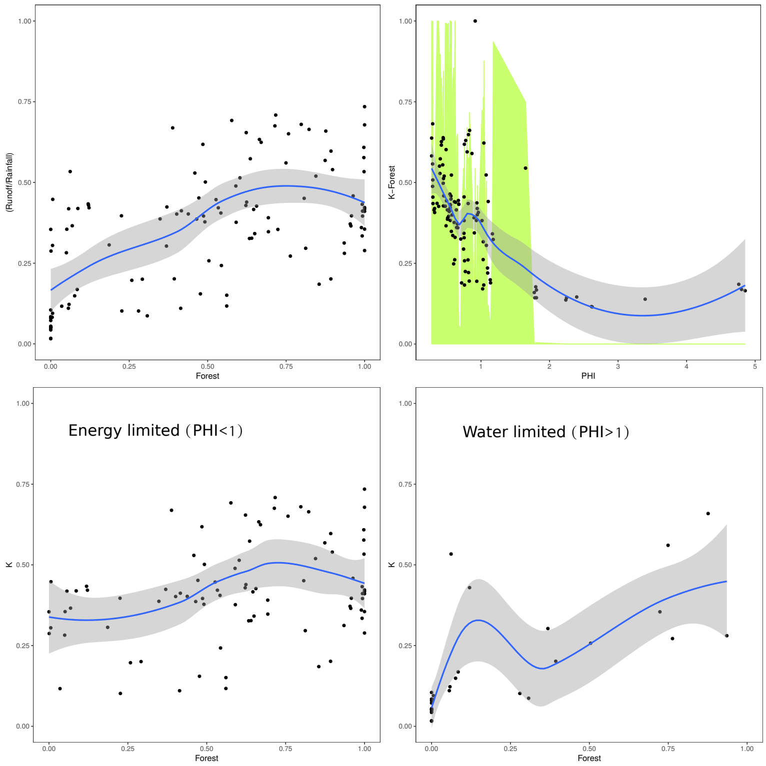

Fig. 2. Panel with free flowing rivers