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

Anonymous Referee #2

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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.

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 ∼0.5. 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 ∼0.5 when the fraction of forest cover reaches ∼0.5 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.

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

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Discussion paper

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.

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?

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.

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.

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 ∼0.5 then the evidence for the hypothesis would be more convincing.

Minor comments

Page 3: please note that the potential evaporation estimate from GLEAM v3.0a is based on Priestly-Taylor.

Figure 1: the Sava river has a runoff ratio (k) approaching 1 – is this physically realistic? I suspect not.

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.

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