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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 #1

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General comments

The manuscript "Global synthesis of forest cover effects on long-term water balance partitioning in large basins" used observed data world basins worldwide and found a striking pattern in water balance partitioning depending on forest coverage. The paper is in general well-written and addresses a topic of great interest for the HESS readership. The research question and results are intriguing and elegant. However, there are several issues related to the authors' method explanations and results interpretations that makes the paper an unconvincing read:

- **Unclear explanation for observed relationship between forest cover and**

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river flows. The authors suggest that forests increases river flows, although their data and analyses do not support this, being neither based on time series in basins undergoing forest cover change nor on analyses of absolute river flow amounts. Such claims are made e.g. at P8L10-11: "our results are consistent with the contrasting view that the presence of forests enhances the long term capacity of river basins to maintain large river flows", and at P13L29-31: "our results support the view that the presence of forests enhances continental water availability through an improved capacity in major river basins to maintain large river flows in the long term". The authors' thought process is not clearly explained. One could imagine that the authors think that an increase in forest cover attracts P (e.g., through the still debated biotic pump mechanism), and therefore a stable P-half pattern would mean increased runoff. However, the authors' explanations in other parts have rather argued that forests can both increase and decrease both E and alter P through moisture recycling. If moisture recycling is the main mechanism, it seems more intuitive to think that forest cover does not have a large influence on river flows at all, since simultaneous changes in E and P in the same direction should result in a dampened change in river flow. The authors makes a long list of different mechanisms through which forest regulate the surface water balance on p. 11, but it is not clearly explained how these processes act together to result in the observed pattern and fall in with the authors' claim that forest cover increase can increase river flows. I would suggest the authors to better guide the reader through their thought process step by step, and include clarifying conceptual diagrams of the processes and interactions addressed.

- **Invalid to substitute space for time.** The authors use their observations in space (i.e. comparison across different river basins) to draw conclusions about how river flow changes with changes in forest cover (i.e., temporal changes in river basin). I would suggest the authors to refrain from making such jumps in their conclusions. If the authors insist to discuss the possibility that spatial

comparisons can be indicative of temporal changes, this limitation needs to be highlighted much more and the level of certainty in the claims need to be toned down. Criticism of space-for-time type of research can for example be found in (Berghuijs and Woods, 2016; Ratajczak and Nippert, 2012). The authors seems to recognize this (e.g., from P8L33), but still jumps into rather spectacular conclusions with formulations like “A critical implications of our results is that forest loss . . . []. . . can force a basin from the P-halved to the E-dominated partitioning pattern” (P13L31).

- **Basin selection rationale unexplained.** The results are highly dependent on the basin selection. Therefore, it is of great importance how basins are selected. With 22 basins, even a relatively small bias in basin selection could seriously affect the results. Please provide information on how the basins were selected.

- **Additional analyses could support a more satisfying explanation.** The manuscript shows a pattern, but does not provide a satisfying explanation. The authors make an attempt to explain the observed pattern through Budyko curve analyses (which did not provide an explanation), and subsequently make several rather speculative explanations in the discussions, pertaining to e.g., moisture recycling (P12L34-35), forest reservoir concept (P12L16), basin size (P12L31) etc. I found it somewhat disturbing that such large parts of the manuscript discussions are solely based on speculative interpretation, rather than discussion of performed analyses results. I think the manuscript would feel more complete, if the authors could perform a few more analyses to test some of these suggested ideas: e.g., what are the approximate moisture recycling ratios in the P-halved basins? How is the P-halved pattern optimal for regulating water flows? Is basin size correlated with the forest’s regulation capacity? While true that there are times when interesting observations should be published even when no satisfactory explanation can be put forward, I think there is room for a few more, not overly demanding analyses.

- **Please refer to criticism and controversies.** The paper cites several papers, whose validity is questioned. For example, (Ellison et al., 2012) is mentioned several time throughout the paper and cited unchallenged despite serious issues with the paper have been pointed out by (van der Ent et al., 2012). Another case is (Zhou et al., 2015) that has been criticised by (Berghuijs and Woods, 2016). The biotic pump theory put forward by (Makarieva et al., 2012) is controversial in terms of its very physics (Meesters et al., 2009). Please check.

Specific comments

- P1L20. Please provide reference supporting the view that forest can lead to an increase in river flows due to e.g. precipitation recycling.
- P1L21. Please note that (Ellison et al., 2012) is a review paper, and the interpretation of observation/modelling results within have been challenged by (van der Ent et al., 2012).
- P1L16-21. (Wang-Erlandsson et al., 2017) provides a process based explanation of land-use change effects on river flows that includes the moisture recycling mechanism and could be useful to cite.
- P3L26. Please clarify how potential evaporation here is defined and calculated.
- Figure 1 shows some interesting patterns: there are several basins where high forest cover coincide with low R/P ratio (e.g., Lena, Mackenzie); there are basins where runoff ratio seems unaffected by forest cover (e.g., Tapajos); and in e.g., Parana, a high forest cover close to the basin outlet seems to have brought down the k value away from the P halved pattern. Please discuss how this relate to the overall basin wide k value patterns, and how it fits into the narrative of forest cover being the explanatory factor of the P halved pattern.

- The two final “Results” paragraphs starting at P8L24 reads like “Discussions”. Please consider re-allocation.
- Section 4.1 reads partly as “Results” rather than “Discussions”. Please consider to re-organise.
- At P11L23-24, the authors write “the increase of forest cover in a basin does not always”. It is not clear if the authors refer to a temporal change in forest cover (which then should be supported by references) or a comparison between basins in the paper (which then should be supported by a cross-reference and formulated as a comparison rather than an actual “increase”).
- P11L23-24. (Teuling et al., 2010) sheds some lights on contrasting hydrological behaviour between European grassland and forests and could be worth citing.
- The authors contradict two views, on p.12 from l. 17. One view is that “forests tend to grow in regions with relatively high water availability”, but is “contradicted by the increasing scientific evidence that forest cover change can significantly alter precipitation regimes in many regions of the world”. This statement is problematic because the references listed, while showing that forest has the ability to alter precipitation, never contradict the notion that forests tend to grow in regions with high water availability.

Technical corrections

- There are in several cases an erroneous em-dash at the end of sentences (e.g., P12L9, P13L8), please check.
- In Fig. 1, the subplots are not always well-aligned. Please check.

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