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Hydrology and Earth System Sciences
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.

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Response to Anonymous Referee 1

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

"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 reader-

ship. 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:"

Thank you very much for your detailed and challenging review. All the suggestions and corrections were accepted. We also clarify some relevant concerns.

General comments

"Unclear explanation for observed relationship between forest cover and 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.""

We agree that our results are not directly supportive of the conclusion that forests increase or enhance river flows. Rather, our main purpose is to show a general pattern in which forest cover can be a descriptor of water balance partitioning. The mentioned sentence at P8L10-11 was written to highlight that our results, in general, do not support the "demand-side thinking" explained in Ellison et al. (2012). However, we agree that this sentence must be changed to avoid misunderstandings. A similar change must be applied to the mentioned sentence at P13L29-31. In the revised version of the manuscript, we modify both sentences to highlight that "the presence of forests coincides with a P-halved water balance partitioning (i.e. the amount of precipitation converted into streamflow is higher in more forested basins).

"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."

We accept the suggestion of improving our thought process through a better guidance for the reader. To properly achieve that, we will add a "clarifying conceptual diagram" (below) to support the possible explanations of our results and discussion. It is necessary to clarify that we are not supporting that precipitation recycling is the only or the main mechanism responsible for producing the water balance-partitioning pattern that we describe. We generally refer in the text to precipitation recycling as an example of a well-known mechanism that could influence this pattern. In the revised version of the manuscript, lines P2L6, P1L20 and P12L35 will be changed to avoid misunderstandings.

To explain how these processes (p. 11) "act together to result in the observed pattern" we will add a new conceptual model that highlights how basins located in arid and semiarid regions are usually controlled by water limited conditions, making radiation the dominant energy and therefore, resulting in a E-dominated partitioning; in basins where significant physical factors control drainage, for instance, a combination of abrupt topography and continuously snowmelt processes, the dominant energy is gravitational, probably resulting in a R-dominated partitioning. Finally, in forested

basins, the combination of all physical and ecological processes associated with the presence and function of forests (p. 11), favor a continuous combination of both types of energy (radiation and gravitational) that ultimately result in a P-halved pattern, as illustrated by the data (new conceptual figure).

"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)."

We agree and accept the suggestion to remove/change the sentences that could wrongly imply that time changes are explained from spatial observations. Accordingly, in the revised version, we modify/remove P13L31-35 from the conclusion section to say that "Our results illustrate that, under particular physical and ecological conditions, forest loss can affect water balance partitioning, such that the general P-halved pattern that we document can be altered".

"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."

In the revised version of the text, we include further detail on how basins were se-

lected. In particular we clarify that "basins were selected based on: (i) basin sizes large enough to account for internal spatial variations, (ii) availability of discharge data and reliable land cover data". Additionally, we used a number of other basins to test the same patterns, and found similar results, which are presented in the appendix section. Further, we add new figures in the revised version of the manuscript to support our results.

"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"

We agree that additional analyses to support our results must be added in the revised version of the manuscript. Following both suggestions, yours and Referee 2, we add new figures to support our results and discussion. Along with the new results commented before and in the response to reviewer 2, we also added a new figure (below) showing the moisture recycling ratios calculated for the 22 basins based on Berger et al (2014). This variable is called "Basin internal evaporation recycling (BIER) ratios", which denotes "the fractions of evaporated water returning to the originating basins via

precipitation." (Figure 2 in Berger et al (2014)). The other analysis suggested by the reviewer are also considered in the revised manuscript by either integrating them or commenting on them.

"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."

Thank you for the suggestion. In the revised version of the manuscript, we explicitly comment on the controversies around these papers and use the suggested references to support the criticisms.

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." *As we clarified before, this sentence is being adjusted/removed.*

"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)." *Corrected as commented in the last comment*

"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." *Included in the revised version*

"P3L26. Please clarify how potential evaporation here is defined and calculated." *Clarification is added to the revised version of the manuscript*

"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." *As commented before, several other features can also affect water balance partitioning, all related to the dominant form of energy explaining water transit through the basin. These is synthesized in a new conceptual figure described above. Such is the case of individual basins that do not follow the expected trend. A note on these particular examples highlighted by the reviewer is included in the revised version the manuscript.*

"The two final "Results" paragraphs starting at P8L24 reads like "Discussions". Please consider re-allocation." *Corrected as suggested*

“Section 4.1 reads partly as “Results” rather than “Discussions”. Please consider to re-organise.” *Corrected as suggested*

“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”).” *Corrected as suggested in the “space for time substitution” comment.*

“P11L23-24. (Teuling et al., 2010) sheds some lights on contrasting hydrological behaviour between European grassland and forests and could be worth citing.” *Reference added as suggested*

“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 precipita-

tion 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” *Corrected. In the revised version, we avoid the use of the term "contradict".*

Technical corrections

“There are in several cases an erroneous em-dash at the end of sentences (e.g., P12L9, P13L8), please check.” *We went through the entire manuscript to address these issues.*

“In Fig. 1, the subplots are not always well-aligned. Please check.” *Corrected*

Reference

Van der Ent, R. J., Savenije, H. H., Schaefli, B., SteeleâĂŘDunne, S. C. (2010). Origin and fate of atmospheric moisture over continents. Water Resources Research, 46(9).

Savenije, H. H. (1996). The runoff coefficient as the key to moisture recycling. Journal of Hydrology, 176(1-4), 219-225.

Ellison, D., N Futter, M., Bishop, K. (2012). On the forest cover–water yield debate: from demandâĂŘto supplyâĂŘside thinking. Global Change Biology, 18(3), 806-820.

Berger, M., van der Ent, R., Eisner, S., Bach, V., Finkbeiner, M. (2014). Water accounting and vulnerability evaluation (WAVE): considering atmospheric evaporation recycling and the risk of freshwater depletion in water footprinting. Environmental science technology, 48(8), 4521-4528.

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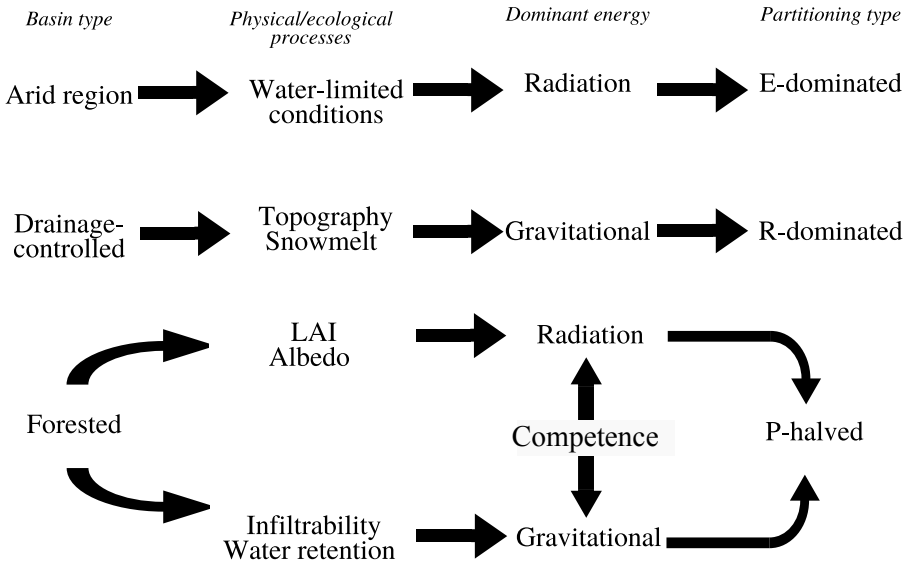

Fig. 1. Conceptual Model

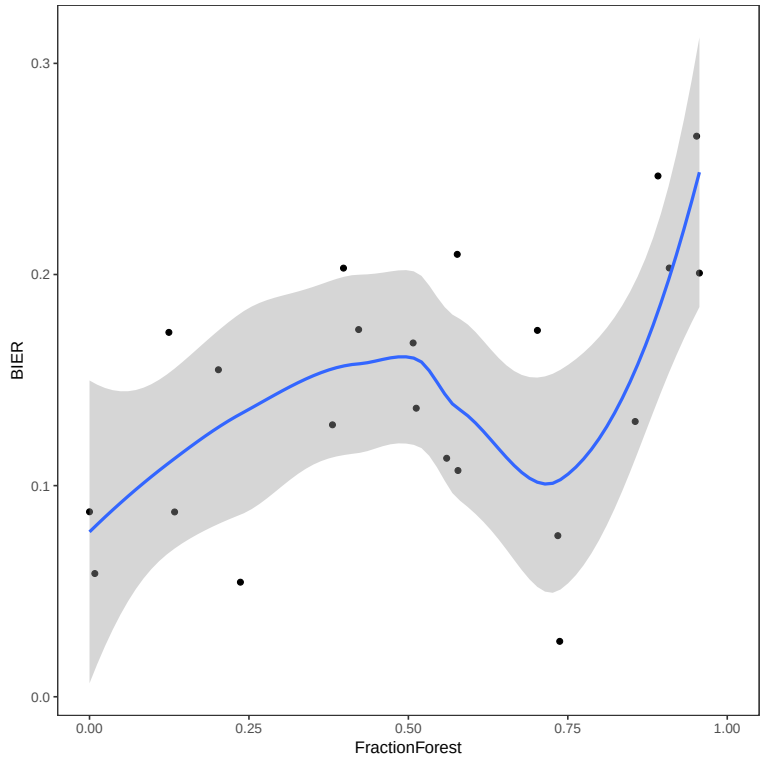

Fig. 2. Moisture recycling ratios