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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.

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Response to A.C.E. Neefjes

Note to the editor and authors

As part of an introductory course to the Master programme Earth Environment at Wageningen University, students get the assignment to review a scientific paper. Since several years, students have been reviewing papers that are in open online discussion for HESS, and they have been asked to submit their reports to the discussion in order to help the review process. While these reports are written as official (invited) reviews,

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they were not requested for by the editor, and we leave it up to the editor and authors to use these reports to their advantage. While several students were asked to review the same paper, this was not done with the aim to provide the authors with much extra work. We hope that these reports will positively contribute to the scientific discussion and to the quality of papers published in HESS. This report/review was supervised by dr. Ryan Teuling.

Thank you very much for your comments, they are certainly helpful for improving its scope, clarity and discussion points. We appreciate your interest in our manuscript

Short abstract

Knowledge on the effects of forest cover on water balance partitioning is becoming more and more relevant with ongoing forest loss. Forest cover is shown to be an effective descriptor for characterizing the water balance partitioning in the 22 largest basins around the world. The long-term water balance is evaporation-dominated with both the runoff coefficient and forest cover below 0.5, while evaporation and runoff are equally divided when the forest cover fraction becomes above 0.5. Understanding the responses of the continental water balance on changing forest cover is necessary in a changing environment, but there was still no consensus on this topic. This paper provides new insights in the determination of water balance partitioning using forest cover as a proxy.

General overview with recommendation

The article suits the scope of the journal well, since it seeks to understand the interactions between water, ecosystems and alterations of ecosystems by human influence. The objective of the research is clear and researched in a good manner. Furthermore, the methods are clearly written. The best method for water balance partitioning (runoff coefficient) is chosen. Sen and Altunkaynak (2006) investigated different versions of the runoff coefficient and thereby rainfall-runoff simulation and concluded that the runoff coefficient shows the least relative error compared to other methods. Besides, the choice of using different types of statistical methods increases the credibility of the results. In this manner strengths and weaknesses of all types of methods can be taken into account to come to a solid conclusion. Furthermore, using basins all around the world and the complete range of forest cover fractions increases the credibility and usage of the method worldwide. The figures are clearly presented and can directly be understood. The discussion section elaborates greatly on the processes that could be of importance in the relation between forest cover and water balance partitioning. Besides addressing the role of water- or energy-limited systems, also the role of forests is discussed. Because of the broad discussion, the mechanisms behind the results become very clear, which makes the final conclusion more convincing.

Thank you for these comments.

However, I am still not fully convinced by the conclusion that forest cover is an effective descriptor of basin attributes describing the long-term water balance for the high forest cover fraction (>0.5). Since 5 out of 14 basins fall outside the 95

We used the LOESS regression as an exploratory tool to test for the presence of a statistically significant trend in the data. The results of the regression analysis (for which we used a non-parametric test) show that, overall, the relationship between forest cover and K is statistically significant at an alpha-level of 0.05. However, as illustrated in the comment, not all data points fall strictly on the regression line or its uncertainty bands (as expected). As we highlight in the paper, these regressions are not aimed to be predictive in nature, but rather they are used to test for general trends in the data. We appreciate the point and we will make sure to acknowledge that a portion of the basins do not fall within the confidence boundaries of the regression.

Besides, the authors state that forest loss will force a system into a evaporation-dominated system. However, this is not well explained and other papers do not agree with this statement. Furthermore, the structure of the paper should be improved, in order to make the readability better. Some major revisions need thus to take place in order to be able to accept the paper, which I will elaborate on in the next paragraphs.

We appreciate the comment and expect to improve structure in a revised version of the manuscript. Based on our results, we propose the hypothesis that by losing forest cover, a basin can potentially move to an evaporation-dominated water balance partitioning. This is the case of, for example, basins in arid and semi-arid areas, where ET dominates the water-budget. However, we are aware that other papers do not agree with this statement (as discussed in P2L3-5). We discuss this by differentiating small vs. large scale (in both space and time) effects of forests on water balance partitioning, such that short-term effects of forest loss can lead to a general increase in streamflow while generally decreasing in the long-term in the absence of forests (leading to increased evaporation losses). Similarly, forest loss over large areas can affect atmospheric moisture transport processes that result in generally drier conditions, under which evaporation tends to dominate water balance partitioning. We use section 4.2 to describe a suite of mechanisms through which forest cover influences long-term water balance partitioning. However, and in response to RC1, in the revised version we will explicitly include an explanation of these mechanisms and how they relate to a potential increase in evaporation.

Major revisions

The largest concern of this article is that the conclusion is not fully supported by the observations. The conclusion that forest is an effective descriptor of the basin attributes that are relevant for characterizing the long-term water balance in large basins of the world is not fully convincing, since the results (figure 2) show that 5 out of 14 basins (Vitim, Madeira, Mackenzie, Lena and Purus) with a high forest cover all outside the 95

As mentioned previously, the regression analysis that we present is not predictive in nature, but rather it is used to explore the presence of a general pattern that relates forest cover to water balance partitioning. Our results show such pattern, and are supported by their statistical significance. We are aware that forest cover cannot be used as a sole *predictor of water balance partitioning, as hydrological regimes depend on other factors (including basins physical attributes, geological/geomorphological features, large-scale atmospheric circulation, among others). Therefore, we use our results to discuss potential mechanisms in which forest cover can affect hydrological partitioning and how those could be affected by forest loss, though examples of river basins in which the relationship is clearer. Discussing each basin′s behavior is out of the scope, as this behavior can be influenced by each basin′s particularities. However, in a revised version of the manuscript, we will explicitly acknowledge this point and highlight that even with statistically significant relationship between forest cover and hydrological partitioning, not all basins are expected to fall within the confidence bands of the specific model.*

A possible reason of the deviation could be found by Ellison et al. (2012), who evaluated the existing controversy about the influence of forest cover on the downstream water availability. They concluded through a review that the scale at which the influence of forest cover have been investigated matter. Forest cover influences the global precipitation by the evapotranspiration they produce. Removing forest has transboundary implications for local and global ET and the water regime. Demand-thinking looks only locally into the needs of a forest (water consumption) rather than larger scale at the creation of water supply to another area. Both processes are important and cannot be seen separately. Water supply through evapotranspiration raises precipitation in other regions as well, affecting the water balance partitioning in another catchment. This article put emphasis on the effect of forest cover loss on transboundary regions, which has not been investigated in this paper and could influence the investigated water balance partitioning in the basins.

We thank this comment and appreciate the reference suggestion (which we included in our manuscript). One key point of our manuscript is to illustrate how long-term precipitation is partitioned between streamflow and evaporation, and conclude that increased forest cover leads to a relatively even partitioning of precipitation into these two fluxes. We acknowledge the role of continental evaporation in maintaining precipitation *regimes. We include basins in a region in which this process has been particularly highlighted (the Amazon). In this study, we have selected large basins in which precipitation recycling can be accounted for. In a revised version of the manuscript we will make sure to explicitly acknowledge the role of local-to-regional evaporation in continental moisture transport and its effects on continental precipitation.*

The lack of explanation of the basins deviating from the general pattern in figure 2 has a big impact on the credibility of the conclusion and should thus be explained well. From the paper it should become clear why these basins are showing different patterns regarding the water partitioning. If there is no clear reason, the conclusion for basins with a high forest fraction is not convincing, since 35

As mentioned in a previous response, we are not proposing that forest cover as the only (or main) control of water balance partitioning. We acknowledge that other attributes are needed to provide a full explanation of a basin′s behavior. However, we highlight the presence of a statistically-significant pattern and the potential implications of this, given that forest cover is highly vulnerable to global environmental change. We do not intend to explain each individual basin, as it is out of the scope of this paper, and we lack the tools to assess each basin.

Besides, it is also concluded that the results provide insight in understanding and predicting potential consequences of forest cover loss on the continental water balance. It is addressed that forest loss can force a basin to go from P-halved to E-dominated, affecting the production of runoff and thus the river flow regimes. This conclusion is not fully supported as well. The first contradiction can be found in the discussion (line 23, page 11), stating that forest cover increase will not lead to increased evaporation and decreased runoff, but can be the other way around as well. This is in agreement with line 33, page 8 in the observations, that states that the relations between the partitioning patterns and forest cover are intended to be only descriptive and not predictive. Furthermore, Zhang et al. (2016) investigated the hydrological responses of forest cover change and to influence of spatial scale, climate, forest type and hydrological regime in large and small watersheds around the world. They achieved this by investigating the response intensity of annual runoff to forest cover change. Climate conditions were investigated through the Budyko Dryness Index in order to determine whether a watershed was energy or water-limited. Besides, they compared different forest cover types (broadleaf, mixed and coniferous) and hydrological regimes (rain and snow- dominated) to test for their significance. They concluded that an increase in annual runoff due to forest cover decrease is significant at several spatial scales. All in all, the discussion and observations and the paper thus concluded the opposite from the article's conclusion that a forest will change into an evaporation- dominated system. More explanation should be given in order to make the article's conclusion more convincing. It should become clear why these results give insight in the consequences of forest loss and why is will go into an evaporation-dominated system rather than runoff-dominated.

We appreciate these observations, but do not necessarily agree in that they are contradictory. With these statements, we acknowledge that forest cover change does not always result in the same kind of streamflow-evaporation response but rather that the response is also affected by a number of other processes. In our previous responses, we have detailed our analytical rationale behind using a Budyko-like approach to classify hydrological partitioning regimes into energy- and water-limited. We include Zhang (2016) and the Budyko index in our manuscript. However, a more detailed discussion will be incorporated in the revised version.

The last major concern is that spurious correlations could be present between the runoff coefficient and forest cover due to transboundary effects that are not taken into account in his paper. Although the runoff coefficient is shown to be the best method to partition the water balance (Sen and Altunkayak, 2006), this method does not take transboundary effects into account. Ellison et al. (2012) showed that these effects are relevant in forested basins. Other papers that put emphasis on this are e.g. Ellison et al. (2017) who reviewed a substantial amount of research on forest, water and energy interactions and Debortoli et al. (2016) who investigated the relationship between forest cover and rainfall patterns in the Amazon Forest using rainfall timeseries between 1971 – 2010 and forest cover from LANDSAT5 satellite data. They found that on regional scale forest cover and seasonal rainfall are correlated. Furthermore, forest loss resulted in a decrease in evapotranspiration and important implications for rainfall thousands of kilometres downwind. Focussing on the runoff coefficient only evaluates the effect of either changes in precipitation, runoff or evaporation. It is possible due to transboundary effects that the results can be affected by more precipitation in the upstream parts of the catchment that also has a large forest cover (Ellison et al., 2017). Forest could thus influence the water balance partitioning of another basin as well, resulting in more complex relationships between forest loss and the runoff coefficient. Spurious correlation between forest cover and runoff should be excluded. This can be done by showing that the same results can be obtained using average evapotranspiration (precipitation minus runoff). Otherwise, the signal of the correlation found has not been identified clearly.

We appreciate the suggestion to acknowledge regional scale effects of forest loss, particularly with respect to moisture transport and its potential effects on precipitation. We do not believe that these processes affect our results for multiple reasons: (1) Our precipitation data is based mostly on observations, which include (in case they occur) the effects of forest loss on recycled precipitations. (2) We include very large basins in the analysis, such that moisture divergence can be controlled for. (3) We use long-term statistics of hydrological partitioning and forest cover, such that inter-annual variability in both climate and forest cover can be accounted for. We are not completely sure about the use of "spurious correlation" in are results, as vegetation cover and runoff have been known to be related processes at the basin scale. We acknowledge that the effects of vegetation cover (particularly forests) on atmospheric moisture transport and continental dynamics need to be further studied, but believe that such study is out of the scope of this manuscript.

Finally, some minor issues should be addressed.

Minor Revisions

The title should be more attractive. Now it is too general and does not include the main conclusion of the article. This could be changed into: “Forest cover as descriptor for long-term water balance partitioning”. *We appreciate the suggestion and will consider refining the title in a revised version of the manuscript*

The structure of the article should be revised. In the discussion part 4.1 figure 4 is introduced, while in the methods they elaborate on the way how they obtained this data. In my opinion, this should be included in the observations and compared there, while the reason behind the possible connections should be discussed in the discussion. In the discussion session the observations should be discussed, while in the observation session no discussion should be started. Both parts of the paper should be clearly distinctive. Introducing new observations in the discussion is confusing, because one expects to already have received all information on the research done in this study. It is better to transfer the observations (line 10-18 and 26-30 of page 9) to the observational part of the paper and to transfer the challenges and discussion of the patterns showed in the observation part (line 24- 35 of page 8 and line 1-7 of page 9) to the discussion part, because this can be seen as recommendations for further research. However, what also could be done in order to be more clear, is to change the title of section 3 to something like: “Patterns in water balance partitioning” and section 4.1 should become 3.2, discussing the effect of water- and energy limitations. Then the discussion should only consist of the role of forests, bringing all observations together. However, the recommendations (line 24-35, page 8 and line 1-7, page 9) should always be discussed in the discussion section rather than the results. It is up to the authors to decide which method to use.

We appreciate the suggested reorganization of the manuscript, which we will consider along with suggestions from the reviewers and other comments in the revised version *of the manuscript.*

Argument why a particular method is used. More emphasis should be given to the reason why the runoff coefficient rather than another method is chosen. Furthermore, it should explained why they used a 12-year average for the calculation of the k-value and not another timeframe. It is stated that forest cover is decreasing, it would also be interesting to investigate the changes in partitioning throughout the years and see what influence this change has on the water balance. Besides, a metric of forest is provided by constructing a global land cover map using the temporal mode for each pixel in 2001-2012 map series of MODIS-MCD12C1 with five land use classes. They constructed the land use maps in temporal mode, because landcover is not static but changing through time. This seems logic. However, since they used the runoff coefficient averaged of 12 years, it is not clear why to use temporal landcover data. Furthermore, they divided the land uses into five classes using the 16 classes of IGBP and only investigated forest or non-forested areas. They could also have chosen to investigate the influence of other land use types as well dividing this way, since they have a significant impact on the runoff generation (Mahe et al., 2004). With the division, they put all forested areas in one class, so including evergreen and deciduous. Explanation of the choices made will clarify these questions.

The main focus of the manuscript is to identify general patterns in long-term water balance partitioning in relation to forest cover. The timescale of averaging water balance variables was selected based on data availability (a common period for all basins) such that is was large enough to control for year-to-year climate variability, which if not controlled for, could potentially affect the results. Since we used long-term averages of hydrological variables, we chose the most appropriate representation of vegetation cover for the same period. We decided to use the mode as the most representative description of vegetation cover. We agree that other vegetation cover types can have different effects on hydrological partitioning and that they need to be further investigated. However, including all vegetation types was out of the scope of this manuscript

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that focused primarily on the role of forests. We acknowledge that different types of forests differ in their ecology and function, but we find the emergence of a common pattern from forests is particularly interesting, and are used to produce a general hypothesis about their role in hydrological partitioning.

Ellison et al. (2017) should be included, because this paper reviews on the latest research done on the effect of forest cover on the water balance and vice versa. *We appreciate the suggestion, and will include the reference in the revised version of the manuscript*

Page 1, line 21: "For instance" can be deleted. *Corrected*

Page 2, line 1: change "for" to "in". *Corrected*

Page 2, line 1-2: "Panta Rhei – Everything flows" debate is not known by everyone and there should be elaborated on this. *We will expand context on the debate within the parenthetical note that refers to the supporting reference)*

Page 2, line 10-11, In the Amazon 40

Page 2, line 22: change "otherwise" in "the other way around". *Corrected*

Page 2, line 30: change "We choose to focus on" in "We focused on" *Corrected*

Page 3, line 3: delete "," in "precipitation, (P)" *Corrected*

Page 3, line 20-21: include the units of R, Q and A. *Corrected*

Page 3, line 27-30: provide the assumptions made using the snow-melt equivalent and the consequences of this. Expanded context will be provided in the revised version Page 7: change "either" in "neither". *Corrected*

Page 9, line 30: delete "of". *Corrected*

Page 11, line 8: change "this" in "these". *Corrected*

Page 11, line 9: delete "the hypothesis". *Corrected*

Page 13, line 10: delete "." after "environments". *Corrected*

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