Peer-review "Global synthesis of forest cover effects on long-term water balance partitioning on large basins"

Daniel Mercado-Bettín, Juan F. Salazer, Juan C. Villegas

By 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, 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.

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.

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% confidence interval and these outliers are not discussed in the paper, it does not seem a very good

method to describe the water balance partitioning in these basins. These outliers should be discussed as well in order to become fully convincing. 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.

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% confidence curve, which is 35% of all basins. This is a large fraction of basins deviating from the pattern. Besides, Sava and Solimo Jav. are just on the edge of the interval. These basins are not addressed in the results, but only 5 out of 22 basins are discussed that are consistent with the pattern they want to show. Although the broad pattern of the water partitioning and the forest cover has been discussed, the basins that not showing this pattern should be addressed as well.

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.

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% of the basins is deviating from the pattern.

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.

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.

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".
- 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.

- 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.
- 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.
- Page 1, line 21: "For instance" can be deleted.
- Page 2, line 1: change "for" to "in".
- Page 2, line 1-2: "Panta Rhei – Everything flows" debate is not known by everyone and there should be elaborated on this.
- Page 2, line 10-11, In the Amazon ~40% of precipitation is recycled. In the paper they cited an article from 1994 (Eltahir and Bras, 1994). This is relatively old and the number could have changed in the meantime, Bosilivich and Chern (2006) state that the recycling ratio is 27.2%. It could thus have changed between 1994 and 2006, decreasing the recycling ratio by 13%. This should be changed.
- Page 2, line 22: change "otherwise" in "the other way around".
- Page 2, line 30: change "We choose to focus on" in "We focused on"
- Page 3, line 3: delete "," in "precipitation, (P)"
- Page 3, line 20-21: include the units of R, Q and A.
- Page 3, line 27-30: provide the assumptions made using the snow-melt equivalent and the consequences of this.
- Page 7: change "either" in "neither".
- Page 9, line 30: delete "of".
- Page 11, line 8: change "this" in "these".
- Page 11, line 9: delete "the hypothesis".
- Page 13, line 10: delete "." after "environments".

References

Bosilovich, M. G., & Chern, J. D. (2006). Simulation of water sources and precipitation recycling for the MacKenzie, Mississippi, and Amazon River basins. *Journal of Hydrometeorology*, *7*(3), 312-329.

Debortoli, N. S., Dubreuil, V., Hirota, M., Lindoso, D. P., & Nabucet, J. (2017). Detecting deforestation impacts in Southern Amazonia rainfall using rain gauges. *International Journal of Climatology*, *37*(6), 2889-2900.

Ellison, D., Morris, C. E., Locatelli, B., Sheil, D., Cohen, J., Murdiyarso, D., ... & Gaveau, D. (2017). Trees, forests and water: Cool insights for a hot world. *Global Environmental Change*, *43*, 51-61.

Ellison, D., N Futter, M., and Bishop, K.: On the forest cover–water yield debate: from demand-to supply-side thinking, Global Change Biology, 18, 806–820, 2012.

Eltahir, E. A., & Bras, R. L. (1994). Precipitation recycling in the Amazon basin. *Quarterly Journal of the Royal Meteorological Society*, *120*(518), 861-880.

Mahe, G., Paturel, J. E., Servat, E., Conway, D., & Dezetter, A. (2005). The impact of land use change on soil water holding capacity and river flow modelling in the Nakambe River, Burkina-Faso. *Journal of Hydrology*, *300*(1), 33-43.

Şen, Z., & Altunkaynak, A. (2006). A comparative fuzzy logic approach to runoff coefficient and runoff estimation. Hydrological Processes, 20(9), 1993-2009.

Zhang, M., Liu, N., Harper, R., Li, Q., Liu, K., Wei, X., Ning, D., Hou, Y., and Liu, S.: A global review on hydrological responses to forest change across multiple spatial scales: importance of scale, climate, forest type and hydrological regime, Journal of Hydrology, 2016.