Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-347-AC5, 2017

Hydrology and Earth System Sciences
Discussions
Open Access
EGU

Interactive comment on "The effect of northern forest expansion on evapotranspiration overrides that of a possible physiological water saving response to rising CO_2: Interpretations of movement in Budyko Space" *by* Fernando Jaramillo et al.

Fernando Jaramillo et al.

fernando.jaramillo@natgeo.su.se

Received and published: 18 August 2017

We thank the reviewers for their positive and constructive feedback to improve this manuscript. We appreciate that Reviewers 1 and 2 believe that our hypothesis and analysis are "well thought", "well executed" and "convincing". We also appreciate that Reviewers 1 and 4 think that the topic of the study is of "broad interest" and "well suited for the journal" and that our results are "important and should be communi-

Printer-friendly version

Discussion paper

cated". The reviewers express different methodological concerns, in particular the somewhat negative Reviewer 3. In the author response to each of the reviews, we have addressed each of the questions, recommendations and concerns that they have very kindly posed.

In general, Reviewers 1, 2 and 4 have suggested toning down the statements regarding the tree water saving response (to increasing atmospheric CO2 concentration) in the title, abstract and main body of the manuscript. We will do accordingly in a revised version. Furthermore, in order to address these reviewers concern, we have justified why we consider that the water saving response should still be present in the justification and discussion of a revised manuscript (see Response 3 to Reviewer 1). We will make sure that the overall message and conclusion is that increasing forest biomass due to forest management is an important driver of evapotranspiration change in this region. As the reviewers correctly remarked, our analysis does not explicitly detect or quantify the water saving response per se. What we discuss is rather that a possible CO2-induced plant water saving response must be small or even inexistent compared to the result of this study which is a positive effect of increasing forest biomass (since we have a positive change in the residual component of the evaporative ratio over time). We will state this message more clearly in the revised manuscript.

Reviewers 2 and 4 have also suggested a statistical analysis to more quantitatively describe our results. We have incorporated such statistical analysis and included it in the response to the Reviewers. In summary, we calculated the coefficient of determination (r2) of the linear regression between all obtained annual values of the residual component of the evaporative ratio ($\Psi r = \Psi - \Psi c$) and the annual values of the three mentioned attributes of forest structure for the temperate and boreal basin groups. The results of this statistical analysis confirm our previous conclusions: forest biomass (V) was the only forest structure attribute that could significantly ($p<0.05$) contribute to the variation in Ψr among years in both the boreal and temperate basin groups. In turn, forest cover (A) could only explain significantly ($p<0.05$) part of the variation in Ψr among

Printer-friendly version

Discussion paper

years in the temperate group, and forest composition could not significantly explain the variance in any of the two basin groups. We present these results in a new Table 1 as shown in the Author comments to the reviewers.

Reviewer 3 also thought that we should further explore if a change in the fraction of precipitation falling as snow (fs) had a significant effect on the residual of the evaporative ratio. In order to address this concern, we have also included this parameter in the statistical analysis previously described. We found that fs could not significantly explain the variation in Ψr among years in the two biomes. The reviewer has additionally requested an uncertainty analysis of our calculations. We have also done accordingly and shown in the Authors response to that reviewer with an modified Figure 6 that the incorporation of the uncertainty of precipitation (P), potential evapotranspiration (PET) and the landscape factor of the Chodbhury equation (n) does not modify our main finding of a general increase in the residual of the evaporative ratio in both basin groups during the period 1961-2012.

We hope that the new statistical analysis and uncertainty assessment previously described have increased the robustness of our analysis and conclusions and will satisfy the reviewers and editor. We also hope that after including all these updates into a revised manuscript as well as the other minor points on visualization, format and sentence structure, the Reviewers and the Editor consider that our manuscript is worth publication in HESS.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2017-347, 2017.

Printer-friendly version

Discussion paper