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Hydrology and Earth System Sciences
Discussions

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.

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Response to Reviewer Nr. 2

We thank Reviewer Nr. 2 for highlighting the importance of our study, for appreciating our appreciating our "convincing arguments" and for proposing valuable suggestions to improve the manuscript. We have addressed below each of the Reviewers remarks, questions and suggestions.

Reviewer 1: This manuscript uses climatic (temperature and precipitation), vegetation (forest expansion in Sweden), and runoff time series from 65 unregulated Swedish basins over 1961-2012 to investigate changes in the precipitation partitioning into evapotranspiration (ET) and runoff. The authors are specifically interested in seeing if increase in forest biomass that occurred in the past decades would combine with two competing physiological phenomena to either increase or decrease ET beyond the extent dictated by climate (represented by the aridity index): (1) decrease plant stomatal conductance in response to increase in CO2 (water saving responses), resulting in a decrease in ET, or (2) CO2-induced increase in plant growth and leaf area, resulting in a increase in ET. The contribution of this manuscript is thus organized into two main components: that of analysis of change, and of attribution of this change to forest properties (total area, volume, and proportion of deciduous species to total LAI). In my opinion, the authors have made a convincing argument for residual changes in basin-level ET that goes the extent dictated by climate. They have postulated that, because the observed ET has increased despite a decrease in aridity index (when Budyko's curve, under stationary conditions, would suggest otherwise based solely on climatic effects), there must exist some non-climate related mechanisms that offset this increase.

Response 1: We thank the Reviewer for the good summary of our work and for appreciating our "convincing arguments" and analyses.

Reviewer 2: To make this point, however, I think that Figure 4 is redundant with Figure 6. Figure 4's use of "wind roses" does not add additional support for the authors' main point. While they claim that these wind roses are "a simple way to synthetize general tendencies of movement," the general direction of these movements are well summarized by the histograms presented in Figure 6, so to me these are two different graphical representation for very similar sets of information. In addition, "spectra of movements in Budyko space," used repeatedly in Section 3.1, need to be rephrased. Since "spectra" has a very specific meaning in time series analysis, I would suggest the authors avoid this term in reference to movement in the Budyko coordinates.

Response 2: Thank you for both suggestions. First, we will remove any reference to "spectra" and refer to the wind-rose type diagrams as "roses" instead. We hope that this change will satisfy the reviewer. The reviewer is right in the fact that the roses (Fig. 4) and the boxplots (Fig. 6) show similar data. However, we still consider both are important for the red line of the manuscript and we think that the problem was the fact that we did not justify enough the simultaneous use of both figures. We will expand on the usefulness of each figure when analyzing the results of each figure in a revised manuscript, as follows:

In summary, the roses (Figure 4) show the direction and magnitude of movement in Budyko space from the first period to the second period for each basin as typical wind roses show wind direction and wind speed. As such, this representation enables the simultaneous study of changes in both evaporative ratio and aridity index and their distribution among individual basins. This cannot be done with the boxplots of Figure 6. By following the analysis work of Jaramillo and Destouni (2014), these roses give an opportunity to estimate the percentage of basins in each basin group for which a change in potential evapotranspiration and precipitation is not enough to explain the observed change in the evaporative ratio (approximately 60% in the case of both our basin groups). We consider this an important result, and have included it in the abstract (Line 23-25 Page 1) that could not be obtained from the boxplots of Figure 6. The roses also allow help synthetize movement in Budyko space for large number of basins since such movements are small and difficult to identify at the complete scale of the Budyko space plot (see Reponse 9 to Reviewer 1).

On the other hand, the box plots of Figure 6 present the characteristics of each basin-group distribution (quantiles, outliers, and median) of the total, residual, and climatic components of the evaporative ratio as well as their arithmetic and area-weighted means for each group. This would be a difficult task with the roses. To address the reviewers concern, we have also used Figure 6 to include an uncertainty analysis (Please

see figure below and Response 2 of Reviewer 3 for more information on the uncertainty analysis) requested by reviewer 3, differentiating it even more from Figure 4. We hope that the Reviewer considers that this explanation supports the use of both figures.

Reviewer 3: I think also that the weakness of the manuscript as it stands lies in formulating the argument for the second point, e.g., in attributing the observed increase in ET to a specific, hypothesized mechanism. In Figure 8, boreal and temperature forests showed opposite changes in this deciduous proportion, though how this might contribute to the overall increase in ET in both forest types is not discussed.

Response 3: Thanks for this valuable suggestion. Our intention was not to attribute the increase in the residual of the evaporative ratio to a single mechanism but rather to assess which forest attribute may best explain that increase. We will revise the title and abstract of the manuscript accordingly. We will reaffirm in a revised manuscript that the resulting change in the residual component of the evaporative ratio is related to changes in "forest structure", and that in turn the forest structure attribute that best explained the observed changes in the residual component of change in the evaporative ratio was forest biomass, followed by forest cover.

Changes in the composition of the forest (another forest structure attribute) may also have an effect in the evaporative ratio of these basin groups, however, our results showed no statistically significant change in forest composition (i.e., the ratio of deciduous leaf area index to total leaf area index, called originally LAIQ and from now on QLAI) in either of the two basin groups (Fig. 8 and 9). However, we will specify this in a clearer way, to say that the uncertainty of the experiment did not allow the detection of changes in forest composition. We also found a small mistake in the calculation of forest composition change in Figure 8 that was not showing this result in a clear way, so we have updated Figure 8 accordingly. See updated Figure below.

Reviewer 4: In addition, the relationship between forest attributes and ïĄĎΨr is described in Section 3.2 using very vague terms like "in agreement with" and "followed that of." I would suggest applying more statistical analysis (and plot out the correlation between ïĄĎΨr and each of the forest attributes) in this section to more quantitatively describe these relationships.

Response 4: Thanks for the suggestion. A statistical analysis would greatly improve the robustness of the conclusions of the manuscript. To further address the concern of the Reviewer, we have added the calculation of the coefficient of determination (R2) of the linear regression between all obtained annual values of the residual component of the evaporative ratio (Ψr= Ψ – Ψc) and the annual values of the three mentioned attributes of forest structure for the temperate and boreal basin groups. We found that forest biomass (V) explain most of the variance of Ψr; the R2 for the relationship between forest biomass and Ψr is significantly different from zero ($p<0.05$) for both the boreal and temperate groups. In turn, R2 for the relationship between forest cover and Ψr is only significantly different from zero ($p<0.05$) in the temperate group. Forest composition does not have any significant relationship with Ψr in any of the two basin groups. We will present these results as Table 1 in a revised manuscript (see below).

Reviewer 5: I also remain unconvinced of the authors' use of the cumulative [Ψr] in comparison to the forest attributes (Figure 9), and the application and choice of a 5-year moving window for [Ψr]. Both of these usages require further justification.

Response 5: Thanks for expressing these concerns. As expressed in our Response 7 to Reviewer Nr. 1, we are sorry to say that the expression "the cumulative of the residual change of the evaporative ratio [$\Delta\Psi$r]" was definitely an unnecessarily complicated way to define such variable. We had decided to define it as such in order to continue with the notation of change. However, it should have been described as "the residual Ψr obtained when subtracting for each year the climatic estimate of the evaporative ratio Ψc (Eq. 4) from the estimate based calculated from the water budget equation Ψ (Eq. 1 and 2)", i.e. Ψr=Ψ-Ψc. In a revised manuscript, we will remove Eq. 8, clarify this instead and do the corresponding changes in the caption of Figure 8.

We will also remove any use of the five-year moving window in a new manuscript since the new statistical analysis suggested by the reviewer and now applied on the annual results of Ψr (without the moving window) our robust enough (See Response 4 to this Reviewer).

Reviewer 6: If the authors can address these concerns, this paper will make a good contribution to the study of water partitioning at high latitudes.

Response 6: Thank you!

References

Jaramillo, F. and Destouni, G.: Developing water change spectra and distinguishing change drivers worldwide, Geophys. Res. Lett., 41(23), 8377–8386, doi:10.1002/2014GL061848, 2014.

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Figure 6-Modified

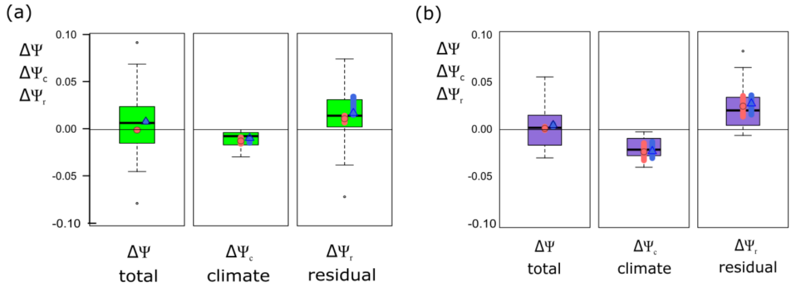

Fig. 1.

Corrected Figure 8

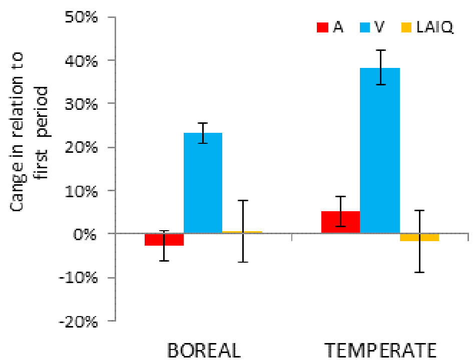

Fig. 2.

New Table 1-

BOREAL

	biomass	forest cover	forest composition	fraction falling as snow
	V	*A*	Q_{LAI}	f_s
intercept	-0.09	0.44	-0.10	0.11
slope	0.00	-0.78	0.89	-0.15
Adjusted R^2	0.07	0.02	-0.02	0.02
p	0.028*	0.168	0.629	0.187

TEMPERATE

	biomass	forest cover	forest composition	fraction falling as snow
	V	*A*	Q_{LAI}	f_s
intercept	-0.06	-0.25	-0.16	0.02
slope	0.00	0.41	1.09	-0.05
Adjusted R^2	0.08	0.06	0.03	-0.01
p	0.026*	0.048*	0.168	0.612

Fig. 3.