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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. 3

We thank Reviewer Nr. 3 for explaining in detail the concerns regarding the robustness of our results as well as the suggestions proposed to increase the robustness of the analysis. We have now performed an uncertainty analysis and a statistical assessment that improve the robustness of our results. We have also addressed below each of the

Reviewers remarks, questions and suggestions.

Reviewer 1: This paper uses the Budyko framework to study the effect of changes in evaporative ratios at a number of boreal and temperate catchments in Sweden. The study looks at changes in the location of each catchment in Budyko space during two consecutive 25-year periods in the early 21st century and second half of he 20th century, and separates the changes into climatic and non-climatic effects. The significant non-climatic effect is then attributed to forest expansion. However, I have a few methodological concerns (detailed below) that leave me concerned about the robustness of the results. I also find the analysis of the results to be fairly limited – the temperate vs. boreal differences are barely discussed for example.

Response 1: We thank the reviewer for the recommendations. We address each of these concerns below. Regarding the differences between the boreal and temperate basin groups, we will take care to discuss in more detail the similarities and differences in responses in a revised manuscript.

We will also expand on the difference in the species composition between both groups, in terms of the mean biomass of each species in the initial period 1961-1986. We will include this as a Panel c in Figure 1 and discuss the main differences accordingly (oranges are deciduous species and blues are coniferous species). The corresponding change is then the one originally shown in Figure 8.

Reviewer 2: The only real result presented is a qualitative statement of relative dominance that confirms previous studies nor is the amount of variability in climatic and vegetation drivers within each biome (despite data on this clearly being used before aggregation in this study). My methodological concerns are as follows: 1) It is argued that forest inventory data cannot be used because they represent too large of an area (e.g. a county that may be larger than the watershed of study within it). In response, the authors aggregate the data even further, to cover an even larger area! How do we know that forest changes and climatic changes are consistent across all of the temperate and all of the boreal areas? The authors should assess the spatial variability of both forest inventory and rainfall data in each biome to ensure this is a reasonable approach.

Response 2: We thank the Reviewer for bringing this important issue that although inherent of the Swedish Forest Inventory in itself, must be clarified here. Let us answer the Reviewer here by first doing some clarifications on the forest data to show that we have already dealt with spatial uncertainty.

Forest data

The idea here is not to describe again the entire methodology which is available in Fridman et al. (2014), but rather to clarify the necessary aspects required to answer the Reviewer's concern.

The data of the Swedish National Forest Inventory (NFI) utilizes a stratified systematic sample based upon clustered sample plots and designed to deliver statistics at county level, mainly of forestland. The NFI was first undertaken in 1923 in the form of a belt inventory, and since 1953 has become a systematic cluster sample inventory. Since 1983, the sampling scheme also includes permanent sample plots, providing a greater precision in change estimates of forest characteristics. The NFI already accounts for changes in methodology across time. The main forest attributes used here to study forest development in Sweden are the area, standing volume and leave/needle volume of productive forest differentiated into several categories (i.e., species, diameter and age composition, forest management stage). The strata of the NFI are based on the Swedish counties; the sample plots have been distributed within each stratum. A single sample distribution is completed every five years, however as each year (representing a fifth of the sample) is evenly distributed over the country, any consecutive five year period can be used.

The sample plots are not restricted to provide only county wise estimates – every sample plot has an upscaling factor and therefore by using a GIS-layer polygon any group

of sample plots can be used to create estimates of forest attributes for such polygon. However, a larger polygon will result in more sample plots being used and a lower standard error. Many of the original 65 basins contained too few sample plots to provide meaningful estimates and therefore the basins were aggregated into larger groups. This is the reason why we decided to aggregate the 65 basins into two main basin groups in order to show the change in forest statistics within the 65 basins and reduce the sampling standard error. The sampling standard error of such calculation and spatial aggregation is already shown and quantified in Figure 8 and mentioned in its caption for both area and volume data. The sampling error for the LAIQ (from now on QLAI) estimate is the propagation of the corresponding sampling errors of forest area and volume statistics (See caption of the Figure).

We acknowledge that there will be a spatial variability within the boreal and temperate basins that is not accounted for. We originally tested with smaller basin groups but the resulting standard errors were too large. Although there is a difficult in achieving the best balance between spatial resolution and the statistical uncertainty associated with a sample based inventory, we think that the selected basin-groups used for the analysis represent the best approach to address them.

Precipitation

Now, regarding the spatial uncertainty of precipitation (P), as mentioned in the manuscript, we have used a widely used methodology, the Thiessen polygon method, to interpolate spatially the spatial data of the 68 precipitation stations with best availability that are located within and near the 65 basins. However, in order to address the reviewers concern, we will use two more precipitation products to calculate mean annual precipitation values for each basin, and incorporate them in an uncertainty assessments. These products are the precipitation estimate from the Climatic Research Unit-gridded P product CRU TS3.23 (Harris et al., 2014) and the mean daily P product of the Luftwebb (http://luftwebb.smhi.se/) portal of the Swedish Meteorological and Hydrological Institute (SMHI). The latter is a gridded dataset of precipitation for Sweden during the period 1961-2014 with a 4 x 4 km horizontal resolution that is based in data collected from over 87 precipitation stations around the country (Johansson, 2000; Johansson and Chen, 2003).

We have now combined the three precipitation P products with the three potential evapotranspiration (E0) products mentioned in Page 3 lines 20 and 28 (excluding the Penman-Monteith estimate after following Reviewer Nr. 1 suggestion), to obtain a total of nine possible combinations of P and E0. As such, we now have three estimates of change in the evaporative ratio ($\Delta\Psi$) and nine of each of its components ($\Delta\Psi$c and $\Delta\Psi$r). We have generated a corresponding uncertainty range (Modified Figure 6, see below) for the calculations of the arithmetic average (blue vertical range of uncertainty) and area-weighted average (red vertical range of uncertainty) for $\Delta\Psi$, $\Delta\Psi$c and $\Delta\Psi$r in each of the two basin groups; temperate and boreal. We found that regardless of the combination of P and E0, $\Delta\Psi$c is always negative and $\Delta\Psi$r is always positive, supporting our previous results and evidencing their robustness. We thank the Reviewer for suggesting this analysis. We will add this new methodology, analysis and figure in a revised manuscript.

Reviewer 3: 2) Similarly, LAI is calculated by using a constant leaf mass per area and biomass data from biome-aggregated NFI data (I think the exact treatment of the NFI data is not clearly explained in Sec. 2.4). The authors then argue that LAI and areal forest cover is constant even as biomass increases by 23%. This would imply a huge trend in stem and branch biomass without any changes in other forest properties, which seems somewhat unlikely. Have the authors checked whether there are changes in forest composition over that time? What is the uncertainty induced in the LAI calculation based on assuming constant LMA for two species, and no other species contributions, however small? Furthermore, the statement that LAI is constant on page 6 line 32, directly contradicts the statement that LAI is changing on page 7 line 9.

Response 3: We must clarify that our results do not show that the leaf area index of deciduous and temperate forest cover is constant in time. What is constant in time is the

ratio of deciduous LAI to total LAI (LAIQ), used here as a proxy for forest composition (See section 2.4). We will change the name of this variable to QLAI throughout a revised manuscript to avoid this confusion, and reword on page 6 line 32. 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 due to a large propagated sampling standard error (Figs. 8 (new updated version below) and Fig. 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.

Reviewer 4: 3) Even for a catchment with unchanging vegetation conditions, there can be quite a lot of scatter on where a specific catchment's point falls relative to a theoretical Budyko curve due to interannual variability and imperfects in the Budyko framework. While a 50-year average may reduce noise to some degree, the entire climatic vs. non-climatic calculation is potentially highly sensitive to the exact value of n used. Some bootstrapping and uncertainty propagation for n would be helpful for demonstrating that the results are robust.

Response 4: Thanks for this reminder. The uncertainty of n in Budyko type equations should be taken into account in order to obtain a proper range of uncertainty of the annual climatic estimate of the evaporative ratio (Greve et al., 2015) and its corresponding changes in the components ïĄĎΨc and ïĄĎΨr. Based on the nine possible outcomes of $\Delta\Psi$r that are now available after following the uncertainty analysis described in Response 2 to this reviewer, we now have also nine possible n values for each basin and many more for each basin group. This, because we use the mean estimates of P and E0 to calculate an n value for each basin. The uncertainty ranges of ïĄĎΨ,ïĂăïĄĎΨc, ïĄĎΨr shown in the modified Figure 6 of Response 2 include the minimum and maximum area-weighted and arithmetic means obtained from propagating such uncertainties into the calculation of these components. The results show again the robustness of our results regarding the estimates of P, E0 and n. Again, these results prove the robustness of our results and we thank the reviewer for addressing this concern.

Reviewer 5: 4) As both the introduction and discussion mention, changes in the fraction of precipitation falling as snow could have a significant effect on the evaporative ratio (Berghuijs et al., 2014) that is not captured in the present analysis. A study of similar effects in China studying the effects of such a change is dismissed for making unrealistic assumptions, but that does not mean that the change itself could not be a factor here. The authors should at a minimum check if there are trends in the fraction of precipitation falling as snowfall. This is particularly troubling since Figure 5 shows a significant change in the seasonal cycle of rainfall in temperate areas.

Response 5: We agree on the importance of accounting for changes in the fraction of precipitation falling as snow (fs) (Berghuijs et al., 2014). That is why we had calculated these changes from the period 1961-1986 to the period 1987-2012 (Lines 17-18 Page 8 “Our calculations show that the fraction of precipitation falling a snow decreased from the period 1961-1986 to the period 1987-2012 from 0.20 to 0.14 in temperate basins and from 0.45 to 0.43 in boreal basins”. Mean annual fs was calculated for each basin based on the collected daily P and T data; we assumed that precipitation in days with mean temperatures below 1 Íę C falls as snow and above 1 Íę C as rain, following Berghuijs et al. (2014).

To further address the concern of the reviewer, we have now performed an additional statistical analysis that calculates the coefficient of determination (R2) of the linear regression between all annual values of Ψr= $\Psi - \Psi$c and fs in the two basin groups. We found that forest biomass explains more variance of Ψr than fs in both biomes. We found that R2 for the linear regression between fs and Ψr is not significantly different from zero (p>0.05) for either the boreal or the temperate boreal group. In more detail, fs can explain more of the variance of Ψr in boreal basins than in temperate basins. One explanation for this may be that the possible decrease in spring snow in boreal basins is associated to the observed increase in the length of the growing season due to increasing temperatures (Hasper et al., 2016). A longer growing season may result in more annual transpiration, under constant annual precipitation conditions. These results also make our previous findings more robust and thank this the Reviewer for this other suggestion idea. We will include this new analysis, table and discussion in a revised manuscript.

Reviewer 6: There are several areas in which the presentation of this paper could be significantly improved 1) The specific Ep dataset used in Figures 3-6 is never stated.

Response 6: The data sets of P, T, Tmin and Tmax used to calculate E0 and the four models/products used for the calculation were mentioned and explained in Lines 20 to 29 of Page 3. The Penman-Monteith model will be removed from a revised manuscript to follow Reviewer Nr. 1’s suggestion.

Reviewer 7: 2) I find Figure 4 quite hard to follow. Why are the colors not the same across the 4 sub-plots? This would be easier to read. If the colors represent the radius of each paddle, why are different paddles reaching the same radius colored different (e.g. 4a). Also, how is the r chosen for each paddle, given that it presumably represents multiple catchments?

Reponse 7: 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, in the same way that a typical wind rose shows wind direction and wind speed. The colored roses (green for boreal and purple for temperate basins) show movements in Budyko space as calculated from observations (where ET=P-R; Eqs. 1 and 2). The grey roses show instead movements due to climate only as calculated with the Chodbhury equation (Eq. 4). Each paddle in a rose groups all movements occurring in a range of directions in Budyko space (θ) of 15 degrees. This value was chosen arbitrarily, to provide the sufficient detail of directions. As example (see below), 37% of all boreal basins (green rose) have moved in the range of directions (270°<θ<295°, θ starts from the upper vertical and clockwise). Now, the intensity of the color describes the range of magnitudes (r: dimensionless) of those movements in Budyko space. As example, of those boreal basins moving in the range of directions previously described (37% of the boreal basins), 14% have moved with magnitudes between 0 and 0.05 (light green), 14% with magnitudes between 0,05 and 0,10 (medium green) and 9% with magnitudes between 0.10 and 0.30 (dark green). On the contrary, no basin has moved in this range of directions ($270° < \theta < 295°$) when using the Chodbhury Equation (grey rose). We hope this additional explanation makes the use of the roses easier to understand. We will include this example explanation in the text of a revised manuscript for better understanding of these figures and improve the caption of the figure.

Reviewer 8: 3) Figure 6 suggests differences in the climatic vs non-climatic effects magnitudes between boreal and template. Possible reasons for these differences should be mentioned in the Discussion section, since this is one of the main ways in which your analysis allows detailed study. For example, are there differences in composition.

Response 8: Thanks. As mentioned in Response 1, we will expand on the discussion about the possible differences between the boreal and temperate basin groups. However, even after rechecking all our forest data, our results show no statistically significant change in forest composition (i.e., the ratio of deciduous leaf area index to total leaf area index, called now QLAI) in either of the two basin groups (Fig. 8 and 9).

Reviewer 9: 4) Can the authors comment on whether possible changes in air quality may play a role?

Response 9: Yes, since air pollution such as ground-level ozone and acid rain affects the canopy of the forest and the LAI, evapotranspiration and the evaporative ratio may be lower than under unaffected conditions (i.e., a negative change). Such is the case of the study by Renner et al. (2013) mentioned in Line 33 of Page 2. However, since the residual of change in the evaporative ratio is in our study positive, we can assume that the effect of air quality in this case is not as important as that of increasing biomass.

Reviewer 10: Other minor comments: Page 2, line 40: Typo – formal?

Response 10: We will remove the word “former”

Reviewer 11: Page 3, line 15: Would be helpful to explain 1986 is the midpoint of your data period

Response 11: We will mention that the two 26-year sub-periods are equal in length.

Reviewer 12: Page 7, line 16: This is not really a conflict with global studies. Even if global average trends are a certain way, showing that a specific location doesn’t follow them is not a contradiction but indeed just a sign of spatial variability – CO2 effects can still dominate elsewhere and therefore for the global average cycle. However, see also Swann et al, PNAS 2016 for additional discussion on this topic.

Response 12: Thanks. We will include the reference mentioned by the reviewer and remove the mention of contradiction with other global studies.

Reviewer 13: Page 7, line 33: That “most of [drainage] was implemented before the present study period” conflicts with you statement that there is a peak in forest drainage implementation in the late 1970’s and 80’s (line 31)

Response 13: Thanks. We will rewrite to avoid this contradiction. Most of the drainage occurred before our study period, but there was also a later smaller peak within our period, in the late 1970’s and 80’s.

Reviewer 14: Page 8, line 7-8: This sentence (“The fact that the upward: :”) is quite hard to follow.

Response 14: We will reword this sentence for more comprehension. Thanks again.

References

Berghuijs, W. R., Woods, R. A. and Hrachowitz, M.: A precipitation shift from snow towards rain leads to a decrease in streamflow, Nat. Clim. Change, 4(7), 583–586, doi:10.1038/nclimate2246, 2014. Fridman, J., Holm, S., Nilsson, M., Nilsson, P., Ringvall, A. and Ståhl, G.: Adapting National Forest Inventories to changing requirements – the case of the Swedish National Forest Inventory at the turn of the 20th century, Silva Fenn., 48(3), doi:10.14214/sf.1095, 2014. Greve, P., Gudmundsson, L., Orlowsky, B. and Seneviratne, S. I.: Introducing a probabilistic Budyko framework, Geophys. Res. Lett., 42(7), 2015GL063449, doi:10.1002/2015GL063449, 2015. Hasper, T. B., Wallin, G., Lamba, S., Hall, M., Jaramillo, F., Laudon, H., Linder, S., Medhurst, J. L., Räntfors, M., Sigurdsson, B. D. and Uddling, J.: Water use by Swedish boreal forests in a changing climate, Funct. Ecol., 30(5), 690–699, doi:10.1111/1365-2435.12546, 2016. Johansson, B.: Areal Precipitation and Temperature in the Swedish Mountains, Hydrol. Res., 31(3), 207–228, 2000. Johansson, B. and Chen, D.: The influence of wind and topography on precipitation distribution in Sweden: statistical analysis and modelling, Int. J. Climatol., 23(12), 1523–1535, doi:10.1002/joc.951, 2003.

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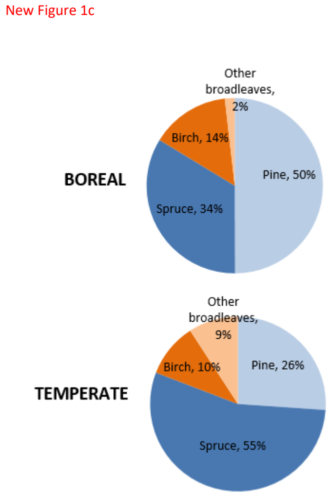

Fig. 1.

Figure 6-Modified

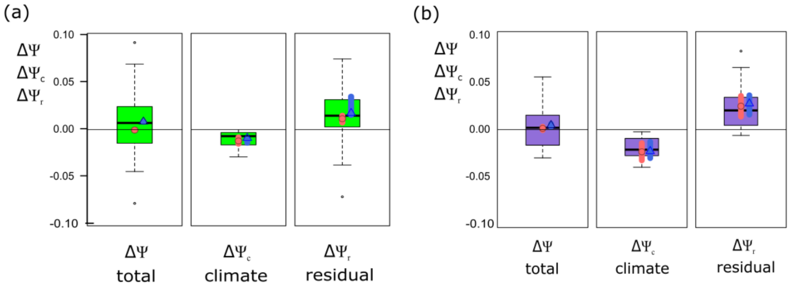

Fig. 2.

Corrected Figure 8

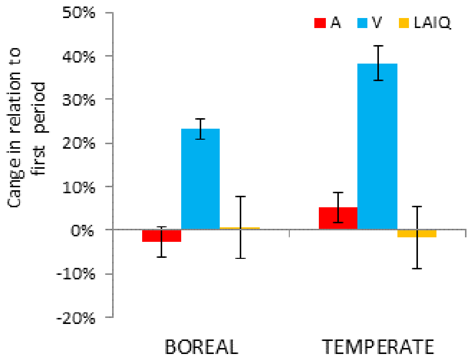

Fig. 3.

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-value	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-value	0.026*	0.048*	0.168	0.612

Fig. 4.