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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.

**Anonymous Referee #3**

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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, 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). The only real result presented is a qualitative statement of relative dominance that confirms previous studies.

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

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.

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 really helpful for demonstrating the results are robust.

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.

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.

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?

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.

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

Other minor comments:



Page 2, line 40: Typo – formal?

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

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.

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)

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

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