

Interactive comment on “The effect of northern forest expansion on evapotranspiration overrides that of a possible physiological water saving response to rising CO₂: Interpretations of movement in Budyko Space” by Fernando Jaramillo et al.

Anonymous Referee #2

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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

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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.

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 synthesize 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.

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 temperate 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.

In addition, the relationship between forest attributes and $\Delta\Psi_r$ is described in Section

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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 $\Delta\Psi_r$ and each of the forest attributes) in this section to more quantitatively describe these relationships. I also remain unconvinced of the authors’ use of the cumulative $\Delta\Psi_r$ in comparison to the forest attributes (Figure 9), and the application and choice of a 5-year moving window for $\Delta\Psi_r$. Both of these usages require further justification.

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

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