

***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.**

Fernando Jaramillo et al.

fernando.jaramillo@natgeo.su.se

Received and published: 8 August 2017

Response to Reviewer Nr. 1

We thank Reviewer Nr. 1 for highlighting the importance of our study and for proposing valuable suggestions to improve the manuscript. We have addressed below each of the Reviewers remarks, questions and suggestions.

Anonymous Referee #1,

Reviewer 1: Overview. This manuscript addresses causes of water balance changes in Swedish forests during the period 1961 to 2012. Water balance changes were encoded in estimates of the evaporative fraction, i.e., annual actual evapotranspiration over precipitation (E/P). The estimated changes in E/P were explained by climatic changes due to precipitation and potential evapotranspiration and by ecosystem changes due to standing forest biomass. The authors conclude that E/P increased 1961-2012 and attribute this increase to increased forest cover, despite a concurrent increase in precipitation. (i.e., decrease in the aridity index). The authors anticipated a reduction in E/P due to CO₂ fertilization and interpret observed increased E/P as evidence that increased forest area overcompensated for any CO₂ fertilization effect. The authors address a question of broad interest with relevance to water and carbon budgets from watershed to global scales. As noted by the authors, recent high profile papers have addressed the CO₂ fertilization effect on the water balance directly (Betts et al. 2007) and in conjunction with climate, land use, and leaf area (Piao et al. 2007). As another example, Swann et al. (2016) and Milly and Dunne 2016) both suggest CO₂ fertilization-induced decreases in ET may partially mitigate currently projected changes in continental drying and drought severity. The current paper follows in the footsteps of Piao et al. (2007) by addressing the effect of reforestation on basinscale evapotranspiration in the context of simultaneous CO₂ and associated climatic changes. In general, the hypothesis and analysis were well thought out and executed.

Response 1: Thank you for this contextualization of our work and for appreciating its contributions.

Reviewer 2: Broadly, I think the clarity of the paper could be improved. And, more specifically, I have some difficulty understanding the analysis linking evapotranspiration changes to forest expansion rather than CO₂ fertilization. “Possible physiological water saving response to rising CO₂” The authors state in their title that the ET increase from forest expansion overrides ET decrease from rising CO₂. The authors

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then present evidence that (1) the aridity index decreased over time due to an increase in precipitation; (2) this decrease in aridity index lead to a decrease in E/P, as expected from the Budyko curve; and (3) there was an overall increase in E/P. This increase in E/P was then attributed to changes in forest standing biomass. There was no evidence or estimate of the CO₂ fertilization effect and, therefore, I find it difficult to conclude that this effect was present and indeed over-compensated. To address this issue, I would suggest the authors reduce the focus on CO₂ fertilization in the title and abstract.

Response 2: Thank you for this valuable remark. It is true that we do not provide direct evidence or quantification of an effect of a stomatal CO₂ response on the evaporative index. What we do show is that such effect is either inexistent or weak, and considerable smaller than that of increasing forest biomass. We will reformulate both the title and abstract to acknowledge this. The overall conclusion of our study is not that there is a weak stomatal water-saving response to increasing CO₂ but rather that increasing forest biomass due to forest management is an important driver of evapotranspiration change in this region. The reformulated title reads like this: “Increasing biomass drives evapotranspiration change in Swedish forests: Interpretation of movement in Budyko space”

Reviewer 3: My intuition is that the CO₂ fertilization effect was relatively weak in this ecosystem. The atmospheric CO₂ concentration increased approximately 85 ppm (315 to 400 ppm) over the study period, 1961-2012. In a meta-analysis of the FACE experiments (Ainsworth and Rogers 2007), trees showed one of the lowest responses of stomatal conductance to elevated CO₂ (20% decrease). In these experiments, CO₂ was increased from 366 ppm to 567 ppm, an increase 2.35 times that experienced from 1961 to 2012. As a first guess, one might expect less than 10% decrease in stomatal conductance whereas the authors show that forest standing biomass increased 25% and 55% in boreal and temperate watersheds (Figure 8). Further, in several places, the authors make the same argument based on results of species-specific responses. See page 2, lines 9-20 and page 7, lines 14-23. To paraphrase, the watersheds studied

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are dominated by coniferous species and CO₂ water saving response has not been observed in these species. In conclusion, there is little evidence to expect a CO₂ water saving response in the studied watersheds. Therefore, I do not think it is appropriate to set up the paper with this hypothesis that is later not supported with the data. On the other hand, I do think it is appropriate to address this weak CO₂ effect as one reason why an increase in E/P was observed, as the authors have done on page 7.

Response 3: We also thank the reviewer for this valuable comment and appreciate the calculations done to support it. We agree that many studies have shown that common gymnosperm species (e.g. conifers) in Northern Eurasian forests exhibit small stomatal responses to elevated CO₂, as we also acknowledge on lines 15 to 20 of page 2. As mentioned in Response #2 above, we will remove the reference to CO₂-induced stomatal water-savings from the title and reformulate the abstract to address this concern of the reviewer. Nevertheless, we think that the possible stomatal water-saving response to increasing CO₂ should still be partially included in the abstract, introduction and main thread of the manuscript due to the following:

1. Although gymnosperms often exhibit small or inexistent reductions in stomatal conductance in response to elevated CO₂, this is not always the case. For example, a Finnish field experiment showed that elevated CO₂ decreased stomatal conductance in Scots pine (Kellomäki and Wang, 1996), as mentioned on lines 19 to 20 in Page 2. It is therefore possible a priori that conifers in our ecosystems exhibit significant CO₂-induced stomatal closure responses. In addition, the most common deciduous tree species, Silver birch, exhibited marked elevated CO₂-induced reductions in stomatal conductance in another field experiment (Rey and Jarvis, 1998), as mentioned in line 22 Page 7. Deciduous forest biomass accounted for up to 20% and 18% of total forest biomass in the temperate and boreal basin-groups, respectively, and their stomatal response to rising CO₂ is thus potentially important for ET trends in our ecosystems.

2. Even though there are experimental indications that Northern conifers may exhibit weak responses of stomatal conductance to elevated CO₂, vegetation models predict

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equal CO₂-induced reductions in stomatal conductance in conifers and gymnosperms. The reason for this is that the coupled stomatal–photosynthesis model used in vegetation and ecosystem models (Ball et al., 1987; Leuning, 1995; Medlyn et al., 2011) have a general formulation of the stomatal CO₂ response for all species. It is therefore highly relevant for our study to assess the likelihood for substantial CO₂-induced stomatal water-saving responses for our region (as for example predicted by Luo et al. (2008) or Betts et al. (2007), or if this response is much less important for trends in evapotranspiration than changes in the landscape (e.g. increases in forest biomass).

3. The analysis of long-term monitoring data in the present study tests if responses observed in manipulative field experiments and predicted by ecosystem models agree with field observations in Swedish forests over the past 50 years. Field experiments suffer from being conducted at limited temporal and spatial scales; most tree experiments investigating effects of rising CO₂ on stomatal conductance and transpiration in Northern forest trees are only a couple of years long and include a low number of trees. Models heavily rely on knowledge of mechanisms and processes gained in experiments and thus gain from being thoroughly evaluated for ecological realism using monitoring data. We therefore think that our methodology is a valuable tool for evaluating large-scale and long-term applicability of both experimental responses and modelling predictions. By showing that changes in forest biomass are more important than possible (if existing) stomatal closure responses to rising atmospheric CO₂ in controlling ET/P change over the past 50 years, our study highlights the importance for models to accurately capture changes in the landscape when projecting hydroclimatic change of this region. In addition, it shows that predictions of large-scale CO₂-induced stomatal water-saving responses are unlikely to hold for our type of ecosystem. We will clarify these three points in the Introduction and Discussion of a revised manuscript. We will also add in the Introduction the additional references suggested by the reviewer (Milly and Dunne, 2016; Swann et al., 2016) and one additional study (Prudhomme et al., 2014) to highlight the importance of understanding the existence of the water saving response in Northern latitudes due to its implications for future estimates of evapotran-

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spiration and drought.

Reviewer 4: Specific comments Hydroclimatic Data: 1) The temporal scales of the data are inconsistent. For the Penman-Monteith model, the long-term mean annual geostrophic wind at 1000 meters above sea level is used. Given fine-scale variability in windspeed and its local, leaf-scale effect on transpiration, this approach is not warranted. This is especially true for comparison with the Langbein and Hargreaves models, which use daily temperature as model input. My suggestion is to remove the Penman-Monteith analysis and use the 3 other sources for PET.

Response 4: Thanks for this suggestion. We will remove the Penman-Monteith estimate of potential evapotranspiration from the analysis and methodology, still leaving the other remaining three methods of calculation.

Reviewer 5: 2) The discussion in the first paragraph surrounding equation (1) is disorganized. My suggestion is to place the description of the P data before equation (1). Then follow with the sentence, “We used annual P and R data to calculate : : :” after you’ve described what the annual P and R data are.

Response 5: Thanks for the suggestion. We will do as suggested by the Reviewer. Budyko Framework:

Reviewer 6: 1) I am not familiar with the Psi notation for evaporative index and typically see it written as E/P or something similar. It may help the reader to be consistent with notation from your references.

Response 6: We will change the notation as suggested by the Reviewer, so that notation is consistent with former references.

Reviewer 7: Linking the residual effect Delta Psi_r to forest change: 1) I don’t understand equation 8. It is described as a five-year moving average, but there is only a j and j+1 term – does this mean only the current and previous year are used in the calculation?

Response 7: Sorry for the misunderstanding. The 5-year moving window was applied after (not before) the resulting annual time series of the residual effect on the evaporative ratio is obtained from Eq. 8. We added this five-year window to the annual results of the residual of the evaporative ratio in Figure 8 to help visualizing the evaporative ratio trends. Furthermore, we have noticed that Eq. 8 was misleading and confusing, as noticed also by Reviewer 2. The “cumulative of the residual change of the evaporative ratio [$\Delta\Psi_r$]” was definitely an unnecessarily complicated way to express such variable. It should have been described as “the residual (Ψ_r) obtained when subtracting for each year the climatic estimate of the evaporative ratio Ψ_c (Eq. 4) from the estimate calculated through the water budget equation Ψ (Eq. 1 and 2)”, i.e. $\Psi_r = \Psi - \Psi_c$. 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 the use of the 5-year moving window. We have here performed a statistical analysis on the annual data and show that our results are still robust regardless of the use of this 5-year moving window. Please see Response 4 for to Reviewer 2.

Reviewer 8: Figure 2, Can you re-orient the arrow from t1 to t2 to be consistent with your result of decreasing aridity index? That would make the figure easier to read.

Response 8: We have accordingly modified Figure 2, please see below. We will include it as such in a revised manuscript.

Reviewer 9. Figure 3: Can you include a separate Budyko plot for the early and late periods? That would give the reader a general, more intuitive sense of how the watersheds moved in the Budyko space.

Response 9: We will include these figures in the Supplementary information. Please see them below. We thank the Reviewer for this suggestion since with these figures show why movements in Budyko space for a large set of basins are difficult to differentiate without the use of wind-rose type plots of Fig. 4.

Reviewer 10, Figure 6: What is the y-axis label in this figure? Same for Figure 7.

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Response 10: The y-axis is the change in any of the evaporative ratio index and its components, described under each box plot distribution. We will add this accordingly.

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Figure 2 modified

(a)

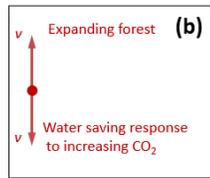
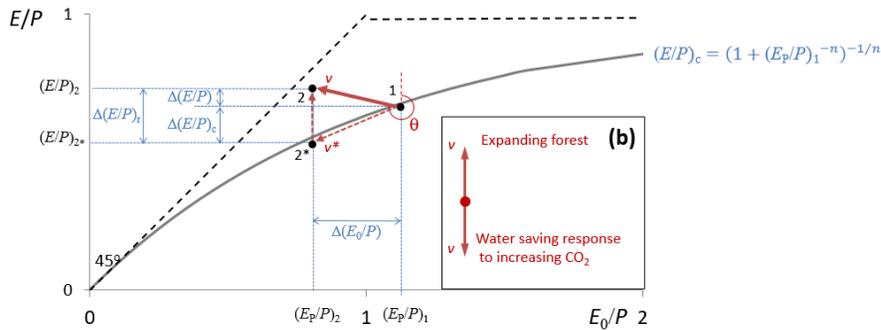


Fig. 1.

Supplementary Figure for Budyko space- both periods

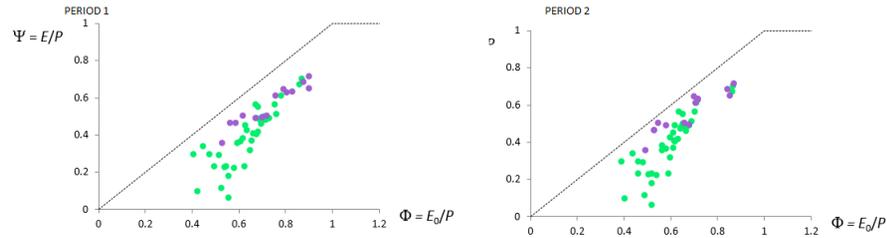


Fig. 2.