Response to Reviewers' comments

We greatly appreciate the reviewers providing valuable and constructive comments on our manuscript HESS-2020-548. We seriously considered each comment and revised/improved the manuscript accordingly. The individual comments are replied below.

Editor:

(Black = original reviewer comments; blue = our response; and green = new or revised text).

I think the suggestions by Reviewer 2 is valuable, and suitable to improve the quality of the manuscript. I recommend that you consider them sincerely. As the reviewer at the same time assumes that the results would not change substantially, (1) I would leave it up to you whether you want to implement the proposed change of methods to enhance the consistency of the approach or add a section to the discussion about those potential improvements to the method. Similarly, (2) I agree with the reviewer on adding discussion on the effect of seasonality on the results.

Reply: Thank you very much for continuing to handle our manuscript and for providing overview suggestions. We have revised the manuscript following yours and the reviewers' comments. As for the two points you specifically mentioned, which we have numbered above to aid communication.

First, we choose not to re-perform all calculations using the new modelling framework and as per your advice mentioned it in the discussion and pointed it out as a potential future research direction, please see L447 to L450 in our revised manuscript. The scientific justification for this choice is stated in our reply to R1C1.

Line 447-450: "Alternatively, Rodríguez-Iturbe and Porporato (2004) incorporated the intensive root water uptake strategy into a stochastic soil water balance model and obtained a steady-state solution that has a simper form than Porporato et al.'s (2004) and also mimics the Budyko curve. This approach deserves further investigation."

Second, for the "growing season versus annual" issue, we have provided a discussion in the revised manuscript (please see Line 450-458 is our revised manuscript).

Line 450-458: "Second, if interpreted strictly from a theoretical perspective, Porporato et al's (2004) model is more suitable to estimate hydrological partitioning during growing seasons instead of over the entire year as it assumes a constant evaporative demand and precipitation regimes and does not account for snow processes. Expanding all these simplifications, acknowledging imperfect knowledge and parameterisation, would require further analyses to better understand how they might affect the results shown here. Nevertheless, the uncertainties caused by these simplifications in Porporato et al's (2004) model might be partly overcome during the empirical connection made here between the Porporato's model and the Choudhury's formulation of the Budyko curve, as evidenced by the overall good performance of the developed BCP model in capturing the observed *Q* (Figure 4)."

Anonymous Referee #1

(Black = original reviewer comments; blue = our response; and green = new or revised text).

R1C1: The authors responded to my comments in the first round of review, and amended the text accordingly, but I am not convinced their approach is theoretically sound (though it might work in practice). Mixing and matching analytical models with different assumptions and re-parameterizing an analytical solution using an empirical (albeit simpler) equation makes the overall approach of this contribution not very appealing nor elegant. I realize that formal elegance might not be the goal here, but there are objective shortcomings too. For example, approximating Porporato's analytical solution for the Budyko curve with Choudhury's empirical formulation results in two types of errors: first, the empirical formulation is not necessarily a good approximation, and second, the relationship between the 'n' parameter and physically-based parameters in Porporato's analytical solution also implies approximations and errors. I realize these errors are probably small, but still, it is not a 'clean' approach in my view.

The authors explain that "it is almost impossible to derive an explicit solution for Zr" for Guswa 2010 model, and that "Porporato's model is much more complex than the Budyko model and can only be solved numerically". However, the change in Q (Eq. 23) and the sensitivity S (Eq. 24) are calculated numerically anyway (L209), so there is no apparent disadvantage having a more complex solution for the Budyko curve. I do see the point that the approach adopted here has been also used in many previous papers, but that does not imply that the approach is set in stone - it can indeed be improved. Instead I agree there is a clear advantage in having an explicit solution for the rooting depth.

To sum up these thoughts, I would suggest trying the following:

1) Use Guswa 2008 model (as done in the original manuscript and in the current revision)

2) Use the Budyko curve obtained from the long-term mean water balance from the same.hydrologic model as in Guswa 2008. Essentially, it's a simpler version of Porporato 2004 already derived previously (Milly, 1993), and was also compared to Porporato 2004 in Rodriguez-Iturbe and Porporato (2004, Section 2.6.2 Minimalistic models of soil moisture dynamics); this would allow a consistent model approach throughout.
3) With this simpler Budyko curve solution, it would not be necessary to approximate the analytical solution with Choudhury's empirical formulation.

My impression is that this approach would make this work more theoretically sound, even though the main results might not be severely impacted.
Reply: Thanks for the constructive comment. However, using the suggested modelling framework deserves a new study itself, which is beyond the scope of this study. In addition, as commented by this reviewer (R1C2), the suggested modelling framework also suffers from the "annual versus growing season" issue, while this issue may be partly overcome in our modelling framework by empirically linking the Porporato's model (growing season) and the Choudhury's model (mean annual). This point is discussed in the revised manuscript (also see our reply to R1C2). In the revised manuscript, we have pointed out the Rodriguez-Iturbe and Porporato (2004)'s derivation as a potential future research direction (Line 447-450). Thanks for the suggestion.

R1C2: Regarding my comment on seasonality, the authors commented that "The determined effective rooting depth during growing season is then used to determine the Porporato's parameter and further, the Budyko parameter". I understand that the rooting depth is calculated using growing season data, but the analytical solution of the Budyko curve by Porporato 2004 is then applied to determine annual discharge. Porporato's model is not suitable to calculate annual averages if interpreted strictly - it assumes constant evaporative demand and precipitation regimes, and does not account for snow accumulation. To be strict, it should be applied to a-seasonal climates only. This requires some clarifications and discussions.
Reply: Done. This point is discussed in the revised manuscript. Relevant text reads (Line 450-458): "Second, if interpreted strictly from a theoretical perspective, Porporato et al's (2004) model is more suitable to estimate hydrological partitioning during growing seasons instead of over the entire year as it assumes a constant evaporative demand and precipitation regimes and does not account for snow processes. Expanding all these simplifications, acknowledging imperfect knowledge and parameterisation, would require further analyses to better understand how they

might affect the results shown here. Nevertheless, the uncertainties caused by these simplifications in Porporato et al's (2004) model might be partly overcome during the empirical connection made here between the Porporato's model and the Choudhury's formulation of the Budyko curve, as evidenced by the overall good performance of the developed BCP model in capturing the observed Q (Figure 4)."

Minor comments

R1C3: L82: I would mention that by "resource" it is actually meant water, since as explained elsewhere light- and nutrient-limited environments have high values of resource availability according to Fig. 2.

Reply: We have indicated that resource availability is typically low in dry (and/or cold) environments and increases as the climate becomes more humid in the manuscript (Line 82-84)

R1C4: L95: acronym WUE is not defined.

Reply. Done and thanks. Revised as suggested (Line 95).

R1C5: L193: "factors… encode" (plural)

Reply. Done. Revised as suggested (Line 195).

R1C6: L205: "factors driving…"

Reply. Done. Revised as suggested (Line 208).

R1C7: L357: "factors… dominate changes…"

Reply. Done. Revised as suggested (Line 362).

Anonymous Referee #2

(Black = original reviewer comments; blue = our response; and green = new or revised text).

R2C1: The manuscript is much improved for clarity and the flowchart describing the methods is particularly useful. I recommend the manuscript for publication following a few minor revisions, detailed below.

Reply: Thanks for your encouraging comments. Your suggestions are replied to below and the manuscript has been revised accordingly.

R2C2: L26: Some models, including Earth System Models, used in past studies do increase root biomass in response to elevated CO2. However, none of them adjust rooting depth to my knowledge. The sentence would be more accurate stating that rooting depth is not taken into account, rather than CO2 effects on below-ground vegetation (which encompasses things like root biomass).

Reply: Done. Revised as suggested. Relevant text now reads (Line 26-27): "In addition, none of the existing studies explicitly account for eCO_2-induced changes to plant rooting depth."

R2C3: L172: Revise heading

Reply: Done. The heading is corrected (Line 173).

R2C4: L308: The text should reflect the fact that the uncertainty encompasses zero, i.e. no significant effect is detected

Reply: In most of the world, WUE (and thus LAI) increases with CO_2 unless there is an even larger increase in vapor pressure deficit. To avoid potential misunderstanding, we revised the relevant text and now also provide the 5% and 95% percentiles (Line 311 & 319).

R2C5: L319: experimental observations -> experiments

Reply. Done. Revised as suggested (Line 322).

R2C6: L356: to dominant -> to dominate

Reply. Done and thanks. Revised as suggested (Line 362).

R2C7: L357: are extensively examined -> have been extensively examined

Reply. Done. Revised as suggested (Line 363).

R2C8: L378: perhaps worth pointing out that the small CO2 effect is partly due to the two opposing processes (structural vs physiological)

Reply: Done. Revised as suggested. Relevant text reads (Line 384-385): “partly due to the two opposing water effects between the structural and physiological responses to eCO_2”

R2C9: L415: Would point out that many climate models only simulated physiological, and not structural changes up until recently. Hence they will inevitably simulate a net increase in Q due to CO2. In CMIP5 about half of the models used fixed LAI (and some models continue to do so in CMIP6)
Reply: Done. This point is added in the revised manuscript. Relevant text reads (Line 423-424): “Nevertheless, this may partly be because only some climate models consider the physiological effect while ignoring structural responses of vegetation to eCO_2.”