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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Low and contrasting impacts of vegetation CO_2 fertilization on terrestrial runoff over the past three decades: Accounting for above- and below-ground vegetation-CO_2 effects" *by* Yuting Yang et al.

Anonymous Referee #3

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The authors explore past runoff trends over undisturbed catchments and globally. Using an analytical framework, they attribute runoff trends to climate and vegetation influences along a resource availability index. The impact of CO2-induced vegetation changes on runoff has remained highly uncertain and as such, this study is a valuable contribution to the literature and well suited to HESS.

Whilst I find this study interesting, it is a shame it does not go further in quantifying the CO2-induced vegetation changes on Q. In particular, the authors mention the inclusion of CO2-induced rooting depth changes as a key novel aspect. However, whilst the au-

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thors quantify in detail the influence of CO2 on the individual above- and below-ground vegetation processes, it is not shown how these in turn affect Q. For Q, only the bulk CO2 response is presented if my reading of the results is correct. A number of studies already exist on the bulk CO2 responses and/or separating the effects of stomatal closure and LAI on Q (although I appreciate a new modelling framework is introduced here). Here it would have been interesting to know how Ze specifically changes Q. I think the results suggest the influence of rooting depth changes are minimal but this is glossed over in the discussion.

I would also hope more clarity on how parameter n is determined. The current explanation is not sufficient, including what data were used. The methods should also be revised for clarity, reading the results it becomes unclear what was quantified using the analytical framework vs other methods (e.g. stomatal closure and L responses). Perhaps a summary of the steps at the start of Methods would help the reader.

Specific comments:

Title: The study period doesn't cover the last three decades.

L23-26: This sentence could be written more clearly.

L30-34: This sentence should also be broken up into two for clarity.

L34: highlights -> highlight

L38: Suggest replacing "becoming" with "and representing"

L44: I would suggest Donohue is not an appropriate reference here, it is not a leaf-scale study.

L50: I think the authors need to unpack this sentence a little. Many of these studies look at the net response on Q so I'm not sure what the authors mean by "different aspects"? I would argue the main reason for the discrepancies across studies is due to the different processes and assumptions included in the models. Also Ukkola et al. is

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not a modelling study but based on observations (similarly Trancoso et al. 2017 which should also be cited for an observational analysis). Some model evaluation has also been conducted specifically for CO2 impacts (e.g. Ukkola et al. 2016, Environmental Research Letters for a DGVM and multiple FACE papers), here the evaluation seems to be limited to the overall Q trends which is not new. A more accurate statement would be that observational and evaluation studies for CO2 effects remain limited, particularly at the regional to global scale.

L53, L265: please fix grammar.

L61: should be Budyko-Choudhury-Porporato

L94: I think Milly and Dunne actually found that the energy-only PET best produced non-water-stressed ET from climate models (their Figure 3, associated text and conclusions). Also climate models do not simulate potential evapotranspiration so perhaps best to avoid that terminology here?

L103: Could you be more specific here? Taking what data?

L111: Not clear to me how potential transpiration is determined?

L119: I don't see where the Earth System Models are described? Also why were ESMs used rather than something more observationally constrained? Given such a short time period is taken and coupled models have their own interannual variability, taking a mean across models over such a short time period is likely to be spurious. Why wasn't observationally-driven products used, e.g. GLEAM or the TRENDY ensemble? These are of course also models but at least driven by observed meteorology.

L138-139: Why do these quantities impact Ep? Most PET estimators are mainly atmosphere-driven so if this is not the case with Shuttleworth and Wallace, more details on its calculation need to be provided for clarity.

Equation 12: should the notation be f() instead of g()?

L218: Which years were used?

L220: You should provide the name of the dataset (i.e. ISLSCP etc.)

Figure 1: White regions in the map that do not match the colour scale (e.g. Greenland). Should say if/why these were masked out

L232, L234: missing full stop

L243: Please avoid using brackets like this, it is very hard to read. Suggest: with the largest Cs reduction found in C4 crops and lowest in shrubs.

L249: How was the Ca effect on L estimated? I'm assuming using equation 18 but it has two factors influencing L (Ca and v)

Figure 5: Would be useful to see the spatial distribution of catchment trends. Suggest adding a map of the catchments eCO2-induced trends as an additional panel

Figure 6: Last panel please adjust scale to show full error bars. Also please check caption, from L587 it mentions numbers that I don't see presented in the figure

L279: I'm confused why this result differs from the number on L269 and how the changes in Q described here differ from the previous paragraph?

L286: I'm also confused that you have suddenly moved to global results (Fig 7). In the methods, you state that the analysis is restricted to the ∼2000 catchments (L195). The text doesn't also make this transition obvious.

L292: Given alpha is determined from LAI, surely low-alpha regions can be either dry or cold?

L348: How exactly can this framework guide model development? Firstly, the results from this study are very much in line with existing studies so no particularly novel insights are revealed. And secondly, how is this framework to help climate model development exactly? And finally, this is ultimately simply another model result. Overall

this feels like a bit of a throw-away statement to try and boost the value of paper

L351: Are all the datasets publicly available?

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