Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-548-RC1, 2021

Hydrology and Earth System Sciences
Discussions
Open Access

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 #1

Received and published: 5 January 2021

This manuscript studies the effect of elevated atmospheric CO2 on runoff at the catchment scale. The approach is based on a combination of models linking elevated CO2 to plant water demand (mediated by leaf area and stomatal conductance changes) and supply (depending on soil water access via changes in rooting depth). The approach is to my knowledge novel (despite building on several previous models and data analyses) and results are interesting. The topic is certainly suitable for HESS. However, I have some concerns regarding the theoretical setup of this work, specifically how different models have been linked and the consistency of underlying modelling assumptions.

Other comments are also listed below.

Main concerns

- Consistency across stochastic soil water balance models. The model by Guswa (2008) assumes that actual evapotranspiration (ET) is fixed and equal to potential ET (PET) as soil moisture varies between the wilting point and saturation. In contrast, the model by Porporato et al. (2004) assumes that actual ET increases from 0 at the wilting point to PET at saturation. These two models are therefore based on different assumptions regarding the relation between actual ET and soil moisture, which in turn affect the long-term mean soil moisture and actual ET values. As a result, the ET/precipitation vs. PET/precipitation relations (i.e., relations in the Budyko space) will differ between these models. To develop a self-consistent theoretical approach to study elevated CO2 effects on runoff, a single stochastic soil moisture model should be selected and used throughout. For example, see how the model by Porporato et al. (2004) can be integrated into Guswa's framework for rooting depth (Guswa, 2010, doi:10.1029/2010WR009122).

- Budyko curve parameterization. The authors use results from Porporato et al. (2004) to link the exponent n in Eq. (1) to rooting depth, water holding capacity, and mean precipitation event depth. This approach is based on analysis of "data from Porporato et al. (2004)" (L103), though it is important to emphasize that in that paper there are no data (except for net primary productivity), so the regression reported in Eq. (2) is obtained by fitting results from the analytical model in Porporato et al. (2004). This step is quite unnecessary, since the results are already in a close-form solution, which can be used directly without any fitting. In other words, Porporato et al. (2004) already provides a fully parameterized Budyko curve, which should be used for consistency with the other parts of the model instead of Eq. (1).

- Model interpretation at annual time scale. The models by both Porporato et al. (2004) and Guswa (2008) have been developed for growing season conditions, assuming no seasonality in precipitation and potential evapotranspiration. In this contribution, these models are interpreted as representative of the whole hydrologic year and used to partition variability in annual runoff. I wonder if and how the original model assumptions and the current model interpretation can be reconciled.

- Role of precipitation event frequency. Eq. (2) neglects the effect of precipitation event frequency on the shape of the Budyko curves from Porporato et al. (2004) framework. The variations in frequency across climates can be more pronounced than variations in mean event depth.

- Interpretation of results from Donohue et al. (2013). Eq. (6) presents an iterative scheme to estimate changes in WUE through time, but in the original articles by Donohue et al. (2013, 2017) steady state models are developed, without an explicit dynamic component. The time scales to achieve steady state are probably in the order of decades (necessary for vegetation change), not years as indicated in Eq. (6).

Other comments

Notation: several symbols are defined differently from the publications they are taken from, creating some confusion. For example, mean rainfall depth is denoted by alpha (not beta) in Porporato et al. (2004); rooting depth is denoted by Z_r (not Z_e) in Guswa (2008); symbol beta is used in Guswa (2008) as well, but has a different meaning; many symbols are used to define evapotranspiration and potential evapotranspiration, and not all are clearly defined (E_{P_T}, E_T, E_{P_M}, E_P); stomatal conductance is generally denoted by g_s, not C_s; symbol theta is used for volumetric soil moisture (not water holding capacity). To summarize, for readers familiar with the literature, reading this manuscript can be difficult because of the different meaning of commonly-used symbols.

L26: why “implicitly” - do you mean “explicitly”? L31: “the resource availability gradient” suggests that this gradient has been presented before, but it is not. L50: other recent works have discussed these issues, including Fatichi et al. (2016, www.pnas.org/cgi/doi/10.1073/pnas.1605036113). L61: please check spelling of BCP model author names. L67 and 69: are "model parameter" and "land surface parameter" indicating the same quantity? L81: this could be a good place for a summary of the research questions or aims of the work. L96-97: just a comment - typically, ET is estimated from precipitation and runoff, since ET is the most difficult term in the catchment water balance to estimate; here the water balance is used to estimate Q, assuming the ET is known. L137: some words missing - e.g., "parameters"? L139: but evaporation from the soil surface is neglected here (L91), so I am not sure I understand this statement. L147: I would define here symbols E_{P_M} and O. L150: not clear how E_{P_M} differs from E_P. L156: this sentence is hard to follow. L160: singular "affects". Section 2.3: I would emphasize that this dataset covers experiments with artificially elevated CO2. L210: how was beta calculated? L236: "differentially better" - meaning not clear. L238: these statements are qualitative and no performance measure is provided to compare the two model variants. L249: ". . .caused an increase of L" - in the remote sensing data or based on model predictions? L252: "L increase is found. . ." - in the remote sensing data or based on model predictions? L265: suggested rewording ". . . shows a slight decrease in. . ." L348: I am not sure how results here can guide climate model development. Figure 3: please check units of RMSE and mean bias in panel (b). Figure 4: are the shown changes in L modelled or measured from remote sensing? Note that "but for each" in the caption is repeated. Figure 6: I suspect L587-590 are not meant to be in the caption (they seem not relevant). I would also show error bars consistent with other plots - here they represent 1/10 of standard deviation, indicating that in fact the variance is extremely large.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2020-548, 2020.

Printer-friendly version

Discussion paper