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:

Three reviewers have given feedback to your manuscript. They all acknowledge the significance of the topic and value for the readership of HESS, and I agree with them. They all also give constructive comments on how the manuscript can be improved in terms of the explanation of the methods and additional analysis. You have already responded to the comments in your final comments to the discussion. If implemented as suggested, I believe they would address most of the concerns raised. Improving the methods section and adding analysis that allows evaluating the role of the rooting depth will definitely enhance the impact of the manuscript. Some of the responses to reviewer 1, were not explicitly announced to become part of the new version of the manuscript (e.g. R1C3, R1C4, R1C5). I believe your answers would provide valuable information for evaluating the method to the readership beyond the review process, and I strongly encourage you to incorporate those.

Reply: Thanks for your encouraging comments and the opportunity to revise the manuscript. We have seriously considered individual comments with the main changes in the new version include:

(1) Following R2C2, R3C3, R3C14, R3C15 and R3C17, we added a beginning paragraph in the Method section and Figure 1, which summarize our modelling approach and how the physiological and structural responses of vegetation to eCO_2 are considered in our modelling framework to detect the impact of eCO_2 on Q. The method section is also extended to include all details of our calculation procedures.

(2) Following R2C2 and R3C28, we removed the catchment scale attribution analysis (i.e., what was Fig 5 in our original submission) and along the lines their comments applied the analysis across the entire global vegetated lands with Figures 6 to 8 being new analysis augmenting the Figures 5 to 6 of our original submission. The catchments Q observations are only used to validate the modelled Q (now Figure 4 being Figure 3 in our original submission).

(3) Following R1C2, R1C3 and your suggestion, we added a paragraph discussing the potential limitations of our modelling framework (Line 418 to 453).

By implementing your and the reviewers comments our manuscript has greatly improved when compared to our original submission. We look forward to receiving any further comments from you and the reviewers; thank you.

Please see our full response below.
To help editor and reviewers, the following colour scheme is used in this response letter:

- Original reviewer comments in **black**
- Our response in **blue**
- New or revised manuscript text in **green**.

To assist with navigation and cross-referencing between reviewers we use codes for each comment. With R1C1 meaning 'Reviewer 1 Comment 1' and so on.

Anonymous Referee #1

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

R1C1: 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.

Reply: Thanks for your encouraging and constructive comments. Your individual comments are replied to below.

R1C2: 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).

Reply: We agree and realized this issue when building the BCP model in 2012. The reason that we still use Guswa-2008, instead of Guswa-2010, is that the solution of transpiration (T) in Porporato-2004 includes an incomplete gamma function with rooting depth contained in both parameters of that incomplete gamma function. This feature makes the analytical solution of dT/dZ_r extremely complex (see the equation below) and it is almost impossible to derive an explicit solution for Z_r. We believe this is the reason that Guswa did not provide an explicit solution of Z_r in his 2010 paper. The results presented in Guswa-2010 and Porporato-2014 were derived numerically but only for specific cases (e.g., with specified aridity index or T_P or dT/dZ_r).

$dT/d\gamma = -T_P * ((exp(-\gamma) * (\gamma^{(W*Z_r-2)} * (W*\gamma-1) + W*\gamma^{(W*\gamma-1)} * \ln(\gamma))) / (\Gamma(W*\gamma) - \Gamma(W*\gamma,\gamma))$

$-(\gamma^{(W*\gamma-1)} * exp(-\gamma)) / (\Gamma(W*\gamma) - \Gamma(W*\gamma,\gamma))$

$+(\gamma^{(W*\gamma-1)} * exp(-\gamma) * (expint(1 - W*\gamma,\gamma) * (W*\gamma*\gamma^{(W*\gamma-1)} + W*\gamma^{(W*\gamma)} * \ln(\gamma))$

$+\gamma^{(W*\gamma)} * (W * (hypergeom([W*\gamma, W*\gamma],[W*\gamma+1, W*\gamma+1], -\gamma) / (W^2*\gamma^2)$

$+(pi * (log(\gamma) - psi(1 - W*\gamma) + pi * cot(pi * (W*\gamma-1)))) / (\gamma^{(W*\gamma)} * sin(pi * (W*\gamma-1)) * \Gamma(1 - W*\gamma)))$

$-expint(-W*\gamma,\gamma)) - W*\Gamma(W*\gamma) * psi(W*\gamma))) / (\Gamma(W*\gamma) - \Gamma(W*\gamma,\gamma))^2)$

and $\quad \gamma = \dfrac{Z_r * \mathrm{WHC}}{\alpha}$

where WHC is soil water holding capacity and α is the mean rainfall intensity.

In the BCP model, the Guswa's (2008) model is used to estimate the effective rooting depth, which is then used to calculate the Porporato's parameter ω (the symbol γ is used in Porporato-2004). According to Guswa-2010, the Porporato's solution for transpiration will lead to a slightly deeper rooting depth than the Milly's solution for transpiration (adopted in Guswa-2008 and this study). Despite that, the responses of Z_r to changes in climate are essentially the same when the two transpiration solutions are adopted. Moreover, the responses of Z_r to changes in CO_2 in the two solutions should also be essentially the same, since the effects of CO_2 on Z_r are expressed via water use efficiency and potential transpiration in our parameterization, which are independent of Z_r parameterizations. In summary, using different transpiration solutions (Milly-1993 versus Porporato-2004) would only lead to difference in the resultant absolute magnitude of runoff (Q) but unlikely to result in differences in the response of Q to CO_2 changes in any notable way, especially when the relative magnitude is used.

In the revised manuscript, we have mentioned this point as a model limitation and discussed this limitation (Line 421 to 441). The relevant new text reads: "Finally, it is worthwhile noting there are several limitations in the developed modeling framework. First, Guswa's (2008) rooting depth model adopted herein employs an intensive root water uptake strategy, which assumes that root water uptake occurs at a potential rate (i.e., E_{P_T}) until soil moisture reaches the wilting point when transpiration is completely suppressed (Guswa, 2008). This intensive root water uptake strategy differs from the root water uptake strategy employed in Porporato et al.'s (2004) stochastic soil water balance model, which is a more conservative strategy under which root water uptake linearly decreases with the decrease of soil moisture (Porporato et al., 2004). Combining the two strategies in one modeling framework potentially leads to inconsistency in the theoretical aspect of the approach. In fact, a later study by Guswa (2010) incorporated Porporato et al.'s (2004) soil water balance model into his cost-benefit framework for rooting depth (referred to as the Guswa-2010 approach). However, the Guswa-2010 approach could not provide an explicit solution for Z_r, because the solution of transpiration in Porporato's model is an incomplete gamma function of Z_r (Guswa, 2010; Porporato et al., 2004). As a result, to allow an analytical solution to be derived we used Guswa (2008) for Z_r in our modeling framework. According to Guswa (2010), using the conservative root water uptake strategy would result in a slightly deeper Z_r compared to that if the intensive strategy were used. Despite that, the response of Z_r to changes in C_a under the two strategies should be similar, as the effects of eCO_2 on Z_r are expressed via water use efficiency and E_{P_T} in our parameterization, which are independent of Z_r parameterizations. This means that adopting different root water uptake strategies would only lead to differences in the resultant absolute magnitude of runoff (Q) but

unlikely to result in differences in the response of Q to eCO$_2$, especially when the relative magnitude is used (Figures 5d, 5e and 6a, 6b, 6e and 6f)."

R1C3: 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).

Reply: The data were obtained directly from Porporato et al. (2004) is their numerical solutions of the corresponding E/P for every 0.1 increment in E_P/P for the six ω curves. By numerically solving the Choudhury's formulation of the Budyko curve, we determined the values of the Budyko parameter (n) that correspond to the E/P values of each of the six ω curves. We then pooled all $n - \omega$ pairs and derived a simple relationship between them (Eq. 2 in the manuscript). Relevant information is now provided in the revised manuscript.

The relevant text now reads (Line 113 to 119): "A relationship between Porporato's ω parameter and Choudhury's n parameter was built following three steps. Firstly, we obtained the numerical solution of the Porporato's model of the corresponding E/P for every 0.1 increment in E_P/P for six separate ω curves. Secondly, by numerically solving the Choudhury's formulation of the Budyko curve, we determined the values of the Choudhury's parameter (n) that correspond to the E/P values of each of the six ω curves. Thirdly and finally, we pooled all $n - \omega$ pairs together and deduced the relationship between n and ω"

We agree with this reviewer from a theoretical perspective. However, from a practical perspective, Porporato's model is much more complex than the Budyko model and can only be solved numerically (for the reason stated in the reply to R1C2). With a specified model parameter, Porporato et al. (2004) proved the similarity between their solution and the Budyko's solution of mean annual water balance. Compared with Porporato et al (2004), the Budyko's formulation is much simpler, which allows an analytical attribution of Q changes. Therefore, developing relationship between the Budyko's parameter (here the Choudhury's expression of the Budyko curve) and Porporato's parameter is a simple yet effective way to solve the problem. The same approach has been adopted previously (e.g., Donohue et al., 2012, https://doi.org/10.1016/j.jhydrol.2012.02.033; Liu et al., 2016, https://doi.org/10.1016/j.jhydrol.2016.10.035; Yang et al., 2016, https://doi.org/10.1002/2016WR019392; Shen et al., 2017, https://doi.org/10.1016/j.jhydrol.2017.09.023; Zhang et al., 2018,

R1C4: 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.
Reply: In our study, the Guswa's model was indeed applied for growing season to determine the effective rooting depth. We have made this point explicit in the revised manuscript (Line 126). The growing season is determined following Zhu et al. (2016), which was explicitly stated in L133-134 of our revised submission (Line 117 in our original manuscript). The determined effective rooting depth during growing season is then used to determine the Porporato's parameter and further, the Budyko parameter. It should be noted that the effective rooting depth is essentially the maximum depth of the hydrologically active soil layer, which should remain unchanged between the growing season and the whole hydrologic year.

R1C5: 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.
Reply: We agree with this reviewer that the precipitation event frequency is important in the control of partitioning precipitation into evapotranspiration and runoff and the Porporato et al (2004)'s framework does not explicitly account for it in their model parameterization. Nevertheless, the Porporato's framework considers both the total precipitation amount and the mean event depth, which when combined provide information about event frequency. Therefore, the effect of variation in event frequency on the hydrological partitioning in the Porporato's framework and the BCP model is implicitly expressed by the variations in both total precipitation amount and mean event depth.

R1C6: 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).
Reply: Eq. (6) follows the gas-exchange theory at the leaf-level to quantify the response of WUE (W in the following equations for simplicity) to elevated CO_2, originally given by Wong et al. (1979),

$$W_{L} = \frac{A_{L}}{E_{TL}} = \frac{g_{s}(C_{a} - C_{i})}{1.6 g_{s}(v_{i} - v_{a})} = \frac{C_{a}}{1.6v}(1 - \frac{C_{i}}{C_{a}})$$

where A (g C m^{-2} s^{-1}) and E_{T} (mm s^{-1}) stand for the assimilation and transpiration rate,

respectively, and the subscript L denotes the leaf-level variables. C_a (ppm) and C_i (ppm) respectively represent the ambient and intercellular concentration of CO_2, and v_a (Pa) and v_i similarly represent ambient and intercellular concentration of water vapor while g_s (m s^{-1}) is the stomatal conductance to CO_2. The numeric factor 1.6 accounts for the greater diffusivity of water vapor relative to CO_2 in air [Wong et al., 1979]. We use v to denote the leaf-to-air water vapor pressure difference (Pa), which is approximated by the atmospheric vapor pressure deficit in subsequent analysis. The relative change in W_L is given by:

$$\frac{dW_L}{W_L} = \frac{dA_L}{A_L} - \frac{dE_{TL}}{E_{TL}} = \frac{dC_a}{C_a} - \frac{dv}{v} + \frac{d(1-\frac{C_i}{C_a})}{(1-\frac{C_i}{C_a})} \quad .$$

Observations have shown that for a given photosynthetic pathway (*i.e.*, C3 or C4 species), C_i/C_a is relatively conservative [Arens et al., 2000, Long et al., 2004, Wong et al., 1979]. The response of the term $1 - C_i/C_a$ to a change in v can be quantified by taking $1 - C_i/C_a$ as being approximately proportional to the square root of v [Donohue et al., 2013; Farquhar et al., 1993, Medlyn et al., 2011]. Therefore, Eq. (2) can be written as:

$$\frac{dW_L}{W_L} = \frac{dA_L}{A_L} - \frac{dE_{TL}}{E_{TL}} \approx \frac{dC_a}{C_a} - \frac{1}{2}\frac{dv}{v} \quad .$$

Eq. (6) in the current manuscript is essentially the same as the above equation. This equation does not require steady-state to be satisfied. However, the above equation is for leaf-level fluxes. Applying this equation at the canopy-scale implicitly assumes the same upscaling factor when converting the leaf-level assimilation and transpiration to the canopy level for a given location. This assumption is also adopted in Donohue et al. (2013, 2017). We have made this assumption explicit in the manuscript (Line xxx) and text below. It is also noted that Donohue et al. (2013, 3017) applied this equation at the same 5-year period as in the current study.

This reviewer did point out an important issue that this theory works better for undisturbed and mature vegetation but can be problematic for disturbed and immature vegetation (e.g., seedlings). However, the issue of vegetation age and disturbances is very complex and is well beyond the scope of this manuscript, noting there are no global dataset monitoring vegetation age that we could use in our modelling.

In the revised manuscript we discuss these points (Line 441-456). Relevant text reads: "The second limitation of the current study lies in the steady-state assumption of the modeling framework. More specifically, the steady-state assumption is made in (i) catchment water balance and (ii) vegetation functioning. For (i), a five-year period does not necessarily guarantee zero-storage change. Nevertheless, the imbalance in water balance calculation under a steady-state assumption at a five-year scale is generally very small (i.e., typically less than 6% of P in arid regions and less than 3%

of P in humid regions) (Han et al., 2020). For (ii), both the Guswa's model for Z_r and Donohue's model for L (see Section 2.1.5) adopted herein were developed for steady-state vegetation (i.e., mature and undisturbed vegetation). Applying these two models to immature (e.g., seedlings) and/or disturbed vegetation can be problematic because immature and/or disturbed vegetation may have very different water use and carbon allocation strategies compared to steady-state vegetation (Donohue et al., 2017; Kuczera, 1987). However, the issues of vegetation age and disturbances are extremely complex and are well beyond the scope of this manuscript. Moreover, global datasets of vegetation age and disturbances are currently lacking. In this light, our modeled response of Q to eCO_2 should be regarded as if all vegetation were mature and undisturbed. Further efforts are needed to better quantify the age and disturbances of vegetation and to better understand the water use and carbon allocation strategies through the entire vegetation life-cycle and under various types of disturbances."

R1C7: 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.
Reply: In the revised manuscript, we have re-defined some symbols and clearly defined all symbols to improve the flow of the manuscript. Specifically, the definitions of E_P, E_T, E_{P_M}, E_{P_T} are defined upon their first appearance, respectively in Line 103, Line 137, Line 192 and Line 126. Moreover, Z_r is now used for rooting depth, g_s is used for stomatal conductance, WHC is used for water holding capacity and α is used for mean rainfall depth. β is now used to represent a resource availability index.

R1C8: L26: why "implicitly" - do you mean "explicitly"?
Reply: Done. Revised as suggested (Line 26).

R1C9: L31: "the resource availability gradient" suggests that this gradient has been presented before, but it is not.
Reply: Thanks for pointing this issue out. We have changed "the resource availability gradient" to "a resource availability gradient" in the revised manuscript (Line 31).

R1C10: L50: other recent works have discussed these issues, including Fatichi et al. (2016, www.pnas.org/cgi/doi/10.1073/pnas.1605036113).
Reply: Done. The work of Fatichi et al., 2016 is cited in the revised manuscript (Line 50). Thanks.

R1C11: L61: please check spelling of BCP model author names.
Reply: Sorry for the typo. We have fixed it in the revised manuscript (Line 63).

R1C12: L67 and 69: are "model parameter" and "land surface parameter" indicating the same quantity?
Reply: Yes, they are the same parameter. We now use "model parameter" consistently throughout the revised manuscript to avoid potential misunderstandings.

R1C13: L81: this could be a good place for a summary of the research questions or aims of the work.
Reply: The aim of the work is summarized at the beginning of the last Introduction paragraph (Line 62-67). Putting it at the beginning of this last introductory paragraph instead of the end improves the flow of the manuscript.

R1C14: 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.
Reply: The Choudhury's formulation of the Budyko curve expresses actual ET as a function of P, PET and a model parameter. Then, the assumed steady-state water balance is used to calculate Q as a residual (i.e., P – actual ET = Q).

R1C5: L137: some words missing - e.g., "parameters"?
Reply: Done and thanks. Revised as suggested (Line 182).

R1C16: L139: but evaporation from the soil surface is neglected here (L91), so I am not sure I understand this statement.
Reply: We believe this reviewer misunderstood our approach. L91 (original manuscript) reads "by taking soil surface resistance equal to zero". This means that soil evaporation would occur at its potential rate.

R1C17: L147: I would define here symbols E_{P_M} and O.
Reply: Done. Revised as suggested (Line 192 to 194)

R1C18: L150: not clear how E_{P_M} differs from E_P.
Reply: In our study, E_P is calculated using the Shuttleworth-Wallace two source evapotranspiration model (described in Section 2.1.2 of the revised manuscript), where E_P is also affected by vegetation parameters (L and g_s). Since both L and g_s respond to eCO$_2$, E_P also responds to eCO$_2$. As a result, we define E_{P_M} as the component of E_P that only varies with meteorological variables (or under a constant CO$_2$). This term is defined in Line 192.

R1C19: L156: this sentence is hard to follow.
Reply: We have revised relevant sentences to make it easier to follow. Relevant text

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reads (Line 199 to 201): "The first term on the right hand of Eq. (23) represents dQ caused by P change and the second term represents dQ caused by eCO_2. The third term calculates dQ induced by changes in E_{P_M} and is calculated as $\frac{\partial Q}{\partial E_P} dE_P -$

$\frac{\partial Q}{\partial E_P} \frac{\partial E_P}{\partial C_a} dC_a$."

R1C20: L160: singular "affects".
Reply: Done. Revised as suggested (Line 204).

R1C21: Section 2.3: I would emphasize that this dataset covers experiments with artificially elevated CO2.
Reply: Done and good point. Revised as suggested. Relevant text reads: "The response of leaf-level stomatal conductance (g_s) response to eCO_2 was determined using 244 field experiments with artificially elevated CO_2 across a broad range of bioclimates (Ainsworth and Rogers, 2007)." (Line 213 to 215)

R1C22: L210: how was beta calculated?
Reply: The mean rainfall intensity was calculated following the IPCC Fifth Assessment Report (Hartmann et al., 2013), which is the ratio of annual total precipitation over the number of wet days (with daily precipitation higher than 1 mm). The information is now provided in the revised manuscript (Line 255 to 257). Relevant text reads: "The mean rainfall intensity was calculated as the ratio of annual total precipitation over the number of wet days (with daily precipitation higher than 1 mm; Hartmann et al., 2013)."

R1C23: L236: "differentially better" - meaning not clear.
Reply: Done. We have deleted "differentially" in the revised manuscript (Line 288).

R1C24: L238: these statements are qualitative and no performance measure is provided to compare the two model variants.
Reply: Done. We added the performance measures to compare the two modelling results in the revised manuscript (Line 287 to 296).

Relevant text reads: "Results show that the BCP model, when considering eCO_2, performed better in estimating Q trends than the BCP model without considering eCO_2, as evidenced by an improvement of R^2 by 0.02, a reduction of RMSE by 0.03 mm yr^{-2} and a decrease of mean bias by 0.11 mm yr^{-2}, averaged over all catchments (Figure 4d). More apparent improvements of the BCP model performance with the consideration of eCO_2 are found in regions having a relatively higher resource availability index. For β of 0.4-0.6, 0.6-0.8 and 0.8-1.0, the mean bias of simulated Q trends with eCO_2 is -0.02 mm yr^{-2}, 0.06 mm yr^{-2}, -0.36 mm yr^{-2} but increased to 0.24 mm yr^{-2}, 0.20 mm yr^{-2} and -0.53 mm yr^{-2}, respectively, when eCO_2 is not considered (Figure 4d). These results suggest that the analytical framework developed herein

captures the eCO$_2$ signal on the observed Q changes."

R1C25: L249: ". . .caused an increase of L" - in the remote sensing data or based on model predictions?
Reply: This is a modelling results, since remotely sensed trends in L is a combined results of different forcing factors. Here, the increases in L are only driven by the CO$_2$ fertilization effect. We have made it explicit that this is a modelling result in the revised manuscript (Line 307, 311, 315 and 318).

R1C26: L252: "L increase is found. . ." - in the remote sensing data or based on model predictions?
Reply: This is a modelling result. Also see our reply to R1C25.

R1C27: L265: suggested rewording ". . . shows a slight decrease in. . ."
Reply: Done. Revised as suggested (Line 323).

R1C28: L348: I am not sure how results here can guide climate model development.
Reply: Originally, our idea, which we agree we did not articulate well, was that the response of LAI to eCO$_2$ along the resource availability gradient reported in herein (also observed by FACE) may provide some useful guidance to the development of ecosystem model, as current ecosystem models perform poorly in representing this response. In addition, the current ecosystem models do not (at least explicitly) consider the response of plant roots to changes in CO$_2$, and this response should be considered in future ecosystem model development. However, as this discussion is not concrete in the revised manuscript we deleted relevant statements about future model development thus avoiding any potential misunderstandings.

R1C29: Figure 3: please check units of RMSE and mean bias in panel (b).
Reply: Sorry for the typo. We fixed it in the revised manuscript (see Figure 4b in the revised manuscript).

R1C30: Figure 4: are the shown changes in L modelled or measured from remote sensing? Note that "but for each" in the caption is repeated.
Reply: These are modelled results. We have made it explicit in the revised captions of Figure 5. In addition, errors in the caption are fixed.

R1C31: 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.
Reply: Errors in the caption are corrected. This reviewer was correct that the variance is indeed very large. In the revised figure, we show one standard deviation among grid-boxes using figures instead of error bars. This helps to better compare the difference in means (showing standard deviation as error bars would make the scale

of Y-axis too large to compare the difference in means).

Thanks very much for your questions and comments that helped us improve our manuscript.

Anonymous Referee #2
(Black = original reviewer comments; blue = our response; and green = new or revised text).

R2C1: The manuscript by Yang et al. aims at quantifying the impact of physiological and structural vegetation adaptations induced by elevated atmospheric CO2 concentration (eCO2) on mean annual runoff (Q). The vegetation-mediated eCO2 effect on Q is complex and involved several processes with sometimes opposite effects. Also, the link of below-ground processes to eCO2 is still not entirely clear. For these reasons, the effect of eCO2 on Q is a source of uncertainty in simulation models. This paper uses an attribution framework, based on the previously applied BCP model, to quantify the net vegetation-mediated eCO2 effect on Q. This is a highly topical subject, the choice of methods seems appropriate and the inclusion of a link to below-ground processes constitutes a substantial novelty, which makes this manuscript of interest to HESS. However, my concerns relate to the presentation of the material: I find the manuscript difficult to follow and think that its value could be greatly increased by improving the description of methods. I therefore recommend a minor revision before the paper gets published.
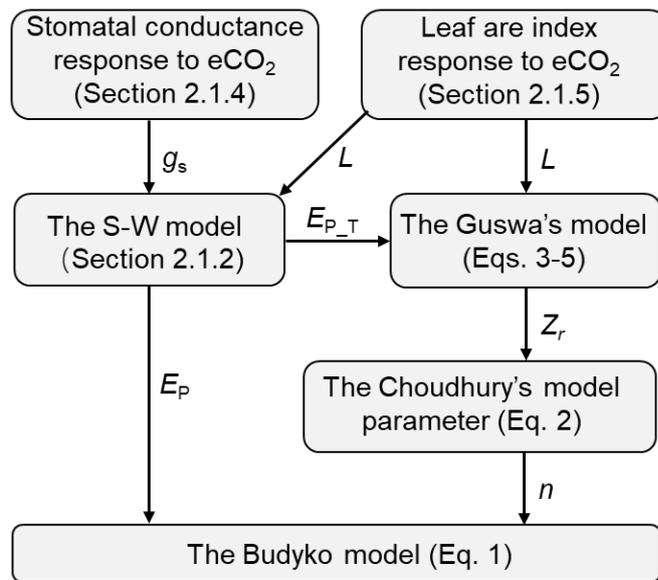Reply: Thanks for your favorable evaluation of our study. We respond to / address your individual comments below.

R2C2: I find the presentation of the methods somewhat unclear and found it difficult to understand how the different methodical steps are linked together, particularly Sections 2.3 and 2.4. Are the responses of stomatal closure and L to eCO2 integrated in the BCP model? If so, please make the links explicit. If not, please clarify how these different steps work together in the attribution framework. Also, it seems to me that the step of extending the analysis from the study catchments (l. 196 states that the analysis is limited to those) to a global raster map (e.g. Fig. 7) is not described in sufficient detail in the Methods.
Reply: Thanks for your suggestion. Following your comments, we have added a beginning paragraph in the Method section and Figure 1, which summarized the links between different parts of the BCP model and how the stomatal and structural responses (new section 2.1.4 and 2.1.5) are incorporated into the BCP model to determine the eCO$_2$ effect on Q.

Relevant text (Line 87-99) reads: "The Budyko-Choudhury-Porporato (BCP) model was adopted here to simulate Q and to attribute changes in Q (Yang et al., 2016b; Donohue et al., 2012). Briefly, the BCP model uses the Choudhury's (1999) formulation of the Budyko curve to estimate Q (Eq. 1 below), in which the model parameter is estimated based on the relationship between the Choudhury's model parameter and the Porporato's model parameter (Eq. 2 below). The required rooting depth (Z_r) in estimating the Porporato's parameter is calculated using the Guswa's (2008) rooting depth model (Eqs. 3-5 below). To quantify the response of Q to eCO$_2$ via vegetation feedbacks, the stomatal response of vegetation to eCO$_2$ is determined

by upscaling the observed response at the site level to the biome level (Section 2.1.4) and the Leaf area index (L) response to eCO_2 is quantified based on the response of *WUE* to eCO_2 adjusted by the local resource availability following Donohue et al. (2017) (Section 2.1.5). The effects of eCO_2 on both stomatal and L also affect rooting depth in Guswa's (2008) model. A flowchart of our modeling approach is summarized in Figure 1 and detailed calculation procedures are described in Sections 2.1.1 to 2.1.5."

Figure 1 Flowchart of using the BCP model to detect the eCO_2 impact on Q. The terminologies used are explained in the following text (section 2.1.1 through 2.1.5).

In addition, we removed the catchment scale analysis and presented the modelling results across the entire global vegetated lands in the revised manuscript (Figures 5-9). The catchment observations of Q were only used for model validation in this new version of the manuscript (Figure 4). We revised relevant text to accommodate this change (e.g., Line 67).

R2C3: In the presentation of the results, it is not immediately clear if the Q-eCO2 response refers to the net effect of increased CO2 concentration on Q (through all the known effects on e.g. meteorological forcing, plant physiological and structural adaptations to CO2 and climate etc – this seems to be the case in the first paragraph of Section 3.3 and Fig. 5), or the net effect of eCO2-induced plant physiological and structural adaptations (this seems to be the case in the second paragraph of Section 3.3 and Fig. 6). Then again, in the first paragraph (l. 270 ff.) the authors discuss the relative importance of physiological and structural effects of eCO2 on vegetation before the corresponding evidence has been presented.
Reply: The reported effect does not include the CO_2 effect on meteorological forcing

but only for the CO_2 effect on Q via vegetation feedbacks (plant physiological and structural responses). We have now made this explicit in the revised manuscript (Line 29, line 67, line 327 and line 373).

R2C4: The authors conclude by stating that the analyses provide insightful guidance for the development of climate models. It would be helpful to describe how exactly the findings from this analysis can be used in climate model development. In general, if this is where the value of the paper lies, it would greatly benefit from connecting the different steps (methods and discussion) to the current state of research in climate and earth system modeling (including the significance of these feedbacks and their uncertainty for earth system modeling, e.g. Hickler et al. 2015 https://doi.org/10.1007/s40725-015-0014-8, Li et al. 2019 https://doi.org/10.5194/bg-15-6909-2018) . For example, how does the CO2 fertilization effect calculated in this paper compare to results obtained in modeling studies? How is the link between Ca and below-ground vegetation dynamics currently represented in models, and how might they benefit from the advances in this study?

Reply: Originally, our idea, which we agree we did not articulate well, was that the response of LAI to eCO_2 along the resource availability gradient reported in herein (also observed by FACE) may provide some useful guidance to the development of ecosystem model, as current ecosystem models perform poorly in representing this response. In addition, the current ecosystem models do not (at least explicitly) consider the response of plant roots to changes in CO_2, and this response should be considered in future ecosystem model development. However, as this discussion is not concrete in the revised manuscript we deleted relevant statements about future model development thus avoiding any potential misunderstandings.

R2C5: l. 111: Please indicate the values for root respiration and the Q10 parameters.

Reply: In this study, we use the standard $Q10$ theory to calculate root respiration. The parameters used in the $Q10$ equation are taken from the MODIS NPP algorithm. We have added relevant information in the revised manuscript (Line 126-129).

Relevant text reads: "γ_r is the root respiration rate (g C g^{-1} roots day^{-1}), which is quantified using the standard Q_{10} theory (Lloyd and Taylor, 1994; Ryan, 1991) with a fixed Q_{10} coefficient of 2.0 (Zhao et al., 2011). The base respiration rate at 20 °C for each biome type is determined following Heinsch (2003)."

R2C6: l. 142 ff: This is not necessarily the case. In the Guswa model, the relation of optimal rooting depth to P/EP is nonlinear and non-monotonic, with the greatest optimal depth calculated in conditions where water supply and demand are approximately equal.

Reply: Our apologies for not expressing this well in our original submission; the statement here discusses the potential CO_2 fertilization effect on rooting depth and not how rooting depth would respond to climate variations. We have made this point clear in the revised manuscript (Line 185).

R2C7: Eq. 15: please define beta.

Reply: As suggested by Reviewer #1 (i.e., R1C7), we used symbol α to represent mean rainfall intensity, which is first introduced in line 111-112 and used in Eq. 2.

R2C8: l.161: what exactly does "residual" mean in this context?

Reply: Residual here refers to total dQ minus P-induced dQ plus E_{P_M}-induced dQ plus eCO_2-induced dQ. In our parameterization, this other effect may include dQ caused by changes in rainfall intensity and/or climate-driven (other than eCO_2-induced) vegetation changes. We have made this explicit in the revised manuscript (Line 205-206).

R2C9: Eq. 16: What are the units of S_Q_to_eCO2?

Reply: Apologies for our oversight; the units of $S_{Q\_to\_CO2}$ in Eq. 16 is mm yr^{-1} ppm^{-1}. This information is added in the revised manuscript (Line 207).

R2C10: l. 250: This average value by itself is not very informative, I suggest characterizing the distribution (mode(s) and range) in more detail (including a discussion of Fig. 4 b).

Reply: We have provided mean plus one standard deviation in the revised manuscript (Line 302, 308, 316).

The discussion of Figure 4b is provided in the discussion section (Line 388-403). Relevant text reads: "The positive response of Q to eCO_2 in high β catchments (primarily located in tropical rainforests; Figure 6a) implies a dominant effect of eCO_2-induced partial stomatal closure over increases in L and Z_r on E in these environments (Figure 6). This is reasonable, as both theoretical predictions and *in-situ* observations have consistently reported a negligible response of L to eCO_2 in humid and closed-canopy environments (Donohue et al., 2017; Yang et al., 2016a; Norby and Zak, 2011; Körner and Arnone, 1992). In such environments, water is generally abundant with light and/or nutrient availability being the most limiting resources for vegetation growth (Nemani et al., 2003; Yang et al., 2015), and vegetation have evolved to efficiently capture light by maximizing their above-ground structure (i.e., L). As a result, in these high L regions, vegetation have already absorbed most of the incident light and any extra leaves would not materially increase the light absorption (Yang et al., 2016a). By contrast, in dry regions, eCO_2-induced increase in vegetation water use efficiency (so less transpiration for the same amount of carbon assimilation at the leaf-level) would lead to an increase in L that is directly proportional to an increase in water use efficiency which would increase canopy-level carbon fixation (Figure 5b). This finding is consistent with satellite observations (Donohue et al., 2013) and *in-situ* FACE experiments (Norby and Zak, 2011)."

R2C11: l. 257 "has resulted": I suggest making it clearer that this statement describes simulation results, rather than observations (as I understand it).

Reply: Done. We have made it explicit that this is a modeling result (Line 307, 311,

315 and 318).

R2C12: l.288: did you mean "other factors including"?
Reply: Done. Revised as suggested (Line 355).

R2C13: l. 320: which mechanism?
Reply: We replaced "the Q-eCO$_2$ response mechanism" with "the Q-eCO$_2$ response pattern" in the revised manuscript (Line 386). The pattern is described in the previous sentence (Line 384-385), being "a significant positive trend ($p<0.01$) in the Q-eCO$_2$ response along the resource availability gradient".

R2C14: l.337: I am not sure if the word "exaggerate" corresponds to the idea expressed by the authors. Maybe "exacerbate"?
Reply: Done. Revised as suggested, thanks, please see Line 408).

R2C15: l. 340 "This suggests that the structural response. . ." This causal link is not immediately clear to me, please clarify
Reply: The structural response of vegetation to eCO$_2$ decreases with the increase of leaf area index, so with the increases of leaf area index in future climate scenarios (due to higher precipitation and CO$_2$) this means that the response of vegetation structure (e.g., leaf area index) to elevated CO$_2$ will eventually decrease. This point is made more clearly in the revised manuscript (Line 411-414). Relevant text reads: "As the vegetation structural responses to eCO$_2$ decreases with the increase of L, the predicted future L increases suggest that the structural response of vegetation to eCO$_2$ may eventually decrease and the physiological effect of vegetation to eCO$_2$ may become increasingly dominant in the overall response of vegetation water use to eCO$_2$."

R2C16: Fig. 4 a,b,d,e: To avoid any confusion I think it is important to make clear that the data shown are the results of simulations, and not (as I understand it) based on observations.
Reply: Done. Revised as suggested (see new caption of Figure 5, which was Figure 4 in our original submission).

R2C17: Fig. 5: What exactly is meant by "Q change induced by eCO2" (see my comment in the 3rd paragraph)?
Reply: The eCO$_2$ effects examined here are only via vegetation feedbacks (i.e., it does not include the eCO$_2$ impacts on meteorological forcing variables). Please also see our reply to R2C3.

R2C18: Fig. 6: The size of the error bars representing 1/10 suggests a great variability of these quantities among the different catchments. Consider using an alternative visualization method (e.g. boxplots or kernel density plots).
Reply: This reviewer was correct that the variance is indeed very large. In the revised

figures (now Figure 7f and Figure 8), we show one standard deviation among grid-cells using figures instead of error bars. This helps to better compare the difference in means (showing standard deviation as error bars would make the scale of Y-axis too large to compare the difference in means). Please see our response to R1C31.

R2C19: Fig 6: some sentence of the caption refer to elements that I cannot see (viewing the PDF in Chrome on Windows): values in parenthesis; vertical grey dashed line.
Reply: Our apologies, there were some mistakes in the caption of our original Figure 6. We have corrected these mistakes in the revised manuscript (please see Figure 8 of our revised manuscript).

Thanks very much for your review which helped us improve our manuscript.

Anonymous Referee #3
(Black = original reviewer comments; blue = our response; and green = new or revised text).

R3C1: 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.
Reply: Thanks for your encouraging and constructive comments; replies to your individual comments are provided below.

R3C2: 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 authors 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.
Reply: This is a very good suggestion; thanks. However, in our framework, the impacts of eCO_2 on vegetation structures (both LAI and rooting depth) are calculated simultaneously, making it impossible to separately calculate the LAI-induced runoff changes and rooting depth-induced runoff changes. Specifically, eCO_2-induced rooting depth change is parameterized through changes in WUE and potential transpiration, and the eCO_2-induced potential transpiration change is caused by eCO_2-induced changes in stomatal conductance and LAI. Hence, our framework does not allow a separate evaluation of LAI and rooting depth effects.
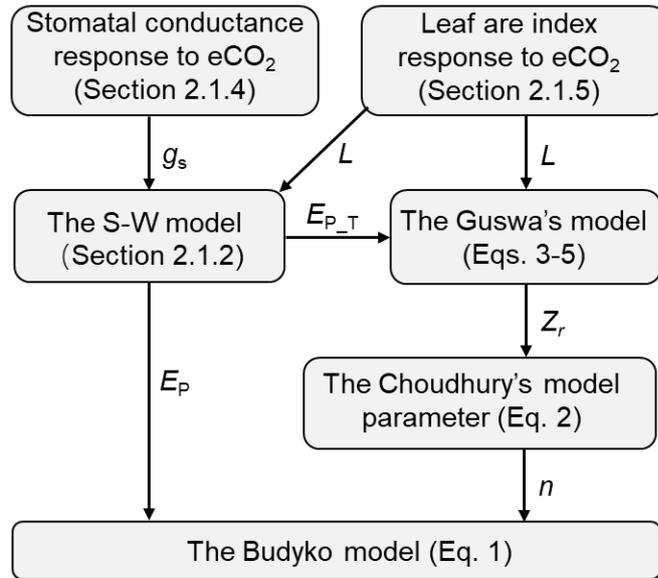
While not exactly your suggestion (which focused on above-ground and below-ground separation of Q impact), following your very good suggestion, we performed new analysis to examine how the physiological and structural responses of vegetation to eCO_2 affect runoff separately (Figure 6 and Line 326-342). Relevant text reads: "Over 1982-2010, C_a increased by ~12.1%. For the same period, the BCP model detected a very small reduction in Q of ~1.7% (or 2.2 mm yr^{-1}) induced by eCO_2 via vegetation feedbacks across the entire global vegetated lands (Figures 6b and 7d). This 1.7% reduction in Q, under the context of 12.1% increases in C_a, demonstrates a muted response of Q to eCO_2. In addition, the overall negative effect of eCO_2 on Q suggests that the structural forcing of eCO_2 on vegetation water consumption (both above- and below-ground) outweighs the physiological effect of eCO_2 driving leaf-

level water saving. Across the global vegetated lands and for the same period, the physiological response of vegetation to eCO$_2$ has led to an increased Q by 0.7% (or 0.9 mm yr^{-1}), with the simulated Q increases being increasingly larger as β increases (Figure 6d). By contrast, the structural response of vegetation to eCO$_2$ has resulted in an overall Q reduction by 2.4% (or 3.1 mm yr^{-1}), with the decreases in Q being increasingly smaller as β increases (Figure 6e). These two opposite responses of vegetation water use to eCO$_2$ along the resource availability gradient have led to a significant positive trend ($p<0.01$) in the Q-eCO$_2$ response along the resource availability gradient, from a negative response in low β landscapes to a positive response in high β landscapes (Figure 6b). Nevertheless, an exception in found extreme arid zones (i.e., when $\beta<0.1$; Figure 6b). This is because in extremely dry areas, the availability of water defines the outcome and the sensitivity of Q to any changes in land surface properties is very small (Donohue et al., 2013; Roderick et al., 2014)."

R3C3: 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.

Reply: Done. Following your suggestion, we have added a beginning paragraph in the Method section and Figure 1, which summarized the links between different parts of the BCP model and how the stomatal and structural responses (respectively new sections 2.1.4 and 2.1.5) are incorporated into the BCP model to determine the net eCO$_2$ effect on Q.

Relevant text (Line 87-99) reads: "The Budyko-Choudhury-Porporato (BCP) model was adopted here to simulate Q and to attribute changes in Q (Yang et al., 2016b; Donohue et al., 2012). Briefly, the BCP model uses the Choudhury's (1999) formulation of the Budyko curve to estimate Q (Eq. 1 below), in which the model parameter is estimated based on the relationship between the Choudhury's model parameter and the Porporato's model parameter (Eq. 2 below). The required rooting depth (Z_r) in estimating the Porporato's parameter is calculated using the Guswa's (2008) rooting depth model (Eqs. 3-5 below). To quantify the response of Q to eCO$_2$ via vegetation feedbacks, the stomatal response of vegetation to eCO$_2$ is determined by upscaling the observed response at the site level to the biome level (Section 2.1.4) and the Leaf area index (L) response to eCO$_2$ is quantified based on the response of WUE to eCO$_2$ adjusted by the local resource availability following Donohue et al. (2017) (Section 2.1.5). The effects of eCO$_2$ on both stomatal and L also affect rooting depth in Guswa's (2008) model. A flowchart of our modeling approach is summarized in Figure 1 and detailed calculation procedures are described in Sections 2.1.1 to 2.1.5."

Figure 1 Flowchart of using the BCP model to detect the eCO$_2$ impact on Q. The terminologies used are explained in the following text (section 2.1.1 through 2.1.5).

R3C4: Title: The study period doesn't cover the last three decades.
Reply: To be explicit regarding the study period we have changed the title to be "Low and contrasting impacts of vegetation CO$_2$ fertilization on global terrestrial runoff over 1982-2010: Accounting for above- and below-ground vegetation-CO$_2$ effects"

R3C5: L23-26: This sentence could be written more clearly.
Reply: Done. Revised as suggested, the relevant text reads (Line 23-26): "This is partly due to eCO$_2$-induced changes in vegetation water use being opposing at the leaf-scale (i.e., water-saving caused by partially stomatal closure) and the canopy-scale (i.e., water-consuming induced by foliage cover increase), leading to highly debated conclusions among existing studies."

R3C6: L30-34: This sentence should also be broken up into two for clarity.
Reply: Done. Revised as suggested, the relevant text reads (Line 30-34): "Globally, we detect a very small decrease of Q induced by eCO$_2$ during 1982-2010 (-1.7%). Locally, we find a positive trend ($p<0.01$) in the Q-eCO$_2$ response along a resource availability (β) gradient. Specifically, the Q-eCO$_2$ response is found to be negative (i.e., eCO$_2$ reduces Q) in low β regions (typically dry and/or cold) and gradually changes to a positive response (i.e., eCO$_2$ increases Q) in high β areas (typically warm and humid)."

R3C7: L34: highlights -> highlight
Reply: Done. Revised as suggested (Line 35).

R3C8: L38: Suggest replacing "becoming" with "and representing"

Reply: Done. Revised as suggested (Line 38).

R3C9: L44: I would suggest Donohue is not an appropriate reference here, it is not a leafscale study.
Reply: Done. The reference "Donohue et al., 2013" is removed here.

R3C10: 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 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.
Reply: Done. Revised as suggested (Line 51-53). Relevant text reads: "Moreover, observational and evaluation studies for eCO_2 effects remain limited, particularly at regional to global scales."

Trancoso et al. 2017 is now cited in the revised manuscript.

R3C11: L53, L265: please fix grammar.
Reply: Done. Increases -> increasing (Line 55).

R3C12: L61: should be Budyko-Choudhury-Porporato
Reply: Done. Revised as suggested (Line 63).

R3C13: 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?
Reply: Milly and Dunne (2016) also used the two-source Shuttleworth-Wallace model to examine the effect of eCO_2 on vegetation stomatal conductance and the consequent impact on potential evapotranspiration (the actual evapotranspiration when water is not limiting) (their Figure 2 and Figure 3). The calculation of PET using the two-source Shuttleworth-Wallace model in our study follows Milly and Dunne (2016; https://doi.org/10.1038/nclimate3046). Yang et al. (2019; https://doi.org/10.1038/s41558-018-0361-0) further demonstrated that why Milly and Dunne found energy-only potential evapotranspiration works well is because that the warming-induced PET increases are almost entirely offset by the eCO_2-induced PET decreases. By using the two-source Shuttleworth-Wallace model, we are able to explicitly consider the impacts of leaf area index and conductance changes on PET.

We agree that the climate models do not simulate PET. We revised this to be "evapotranspiration estimates under non-water-limited conditions" in the revised manuscript (Line 170-171).

R3C14: L103: Could you be more specific here? Taking what data?
Reply: The data obtained from the authors of that paper is their numerical solutions of the corresponding E/P for every 0.1 increment in E_P/P for six separate ω curves. By numerically solving the Choudhury's formulation of the Budyko curve, we determined the values of the Budyko parameter (n) that correspond to the E/P values of each of the six ω curves. We then pooled all $n - \omega$ pairs together to derived a simple relationship between them (Eq. 2 in the manuscript).

Relevant text now reads (Line 113-119): "A relationship between Porporato's ω parameter and Choudhury's n parameter was built following three steps. Firstly, we obtained the numerical solution of the Porporato's model of the corresponding E/P for every 0.1 increment in E_P/P for six separate ω curves. Secondly, by numerically solving the Choudhury's formulation of the Budyko curve, we determined the values of the Choudhury's parameter (n) that correspond to the E/P values of each of the six ω curves. Thirdly and finally, we pooled all $n - \omega$ pairs together and deduced the relationship between n and ω ($R^2=0.96$, $p<0.001$; Supplementary Figure S1):"

R3C15: L111: Not clear to me how potential transpiration is determined?
Reply: The details of E_P (and its two components, potential evaporation and potential transpiration) calculation using the Shuttleworth-Wallace model are now provided in more detail in Section 2.1.2.

Section 2.1.2 reads: "

2.1.2 The Shuttleworth-Wallace model

The Shuttleworth-Wallace two-source evapotranspiration model (the S-W model) was used to estimate E_P and its two components (potential evaporation, E_{P_S} and potential transpiration, E_{P_T}) (Shuttleworth and Wallace, 1985). The S-W model estimates evapotranspiration as:

$$\lambda E_P = \lambda E_{P\_T} + \lambda E_{P\_S} = C_T PM_T + C_S PM_S \tag{7}$$

$$PM_T = \frac{\Delta A + (\rho c_p v - \Delta r_a^c A_s)/(r_a^a + r_a^c)}{\Delta + \gamma[1 + r_s^c/(r_a^a + r_a^c)]} \tag{8}$$

$$PM_S = \frac{\Delta A + [\rho c_p v - \Delta r_a^s (A - A_s)]/(r_a^a + r_a^s)}{\Delta + \gamma[1 + r_s^s/(r_a^a + r_s^c)]} \tag{9}$$

$$C_{\mathrm{T}} = [1 + R_{\mathrm{c}} R_{\mathrm{a}} / R_{\mathrm{s}} (R_{\mathrm{c}} + R_{\mathrm{a}})]^{-1} \qquad (10)$$

$$C_{\mathrm{S}} = [1 + R_{\mathrm{s}} R_{\mathrm{a}} / R_{\mathrm{c}} (R_{\mathrm{s}} + R_{\mathrm{a}})]^{-1} \qquad (11)$$

$$R_{\mathrm{a}} = (\Delta + \gamma) r_{\mathrm{a}}^{\mathrm{a}} \qquad (12)$$

$$R_{\mathrm{s}} = (\Delta + \gamma) r_{\mathrm{a}}^{\mathrm{s}} + \gamma r_{\mathrm{s}}^{\mathrm{s}} \qquad (13)$$

$$R_{\mathrm{c}} = (\Delta + \gamma) r_{\mathrm{a}}^{\mathrm{c}} + \gamma r_{\mathrm{s}}^{\mathrm{c}} \qquad (14)$$

where λ is the latent heat for vaporization (MJ kg^{-1}), Δ is the gradient of the saturation vapor pressure with respect to temperature (kPa K^{-1}), ρ is the air density (kg m^{-3}), c_{P} is the specific heat of air at constant pressure (MJ kg^{-1} K^{-1}), γ is the psychrometric constant (kPa K^{-1}). $r_{\mathrm{a}}^{\mathrm{a}}$, $r_{\mathrm{a}}^{\mathrm{c}}$ and $r_{\mathrm{a}}^{\mathrm{s}}$ are the aerodynamic resistance (s m^{-1}) to heat and vapor transfer between the canopy-air space and the atmosphere, between the leaf and the canopy-air space, and between the soil surface and the canopy-air space, respectively. These three aerodynamic resistance terms are estimated following Sánchez et al. (2008). $r_{\mathrm{s}}^{\mathrm{s}}$ and $r_{\mathrm{s}}^{\mathrm{c}}$ are soil surface resistance and stomatal resistance (the reciprocal of stomatal conductance), respectively. To estimate E_{P} using the S-W model, $r_{\mathrm{s}}^{\mathrm{s}}$ is set to zero and $r_{\mathrm{s}}^{\mathrm{c}}$ is set to its non-water stressed value (Milly and Dunne, 2016). The non-water stressed values of $r_{\mathrm{s}}^{\mathrm{c}}$ for each biome type are provided in Mu et al. (2007). A is the available energy (equals to net radiation minus ground heat flux, W m^{-2}) and A_{s} is the available energy at the soil surface, which is estimated as a function of L following Beer's law (Campbell and Norman, 1998; Yang and Shang, 2013). As a result, $A - A_{\mathrm{s}}$ is the available energy absorbed by the plant canopy. The impacts of eCO$_2$ on E_{P} and its two components are obtained by allowing L and $r_{\mathrm{s}}^{\mathrm{c}}$ to vary with C_{a}. Recently, Milly and Dunne (2016) showed that the S-W model could most satisfactorily reproduce evapotranspiration estimates under non-water-limited conditions from climate models under eCO$_2$."

R3C16: 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

Reply: We are sorry that we missed the description of the ecosystem models used in this study. We've added relevant descriptions in the revised manuscript (Line 253-263). Relevant text reads: "To obtain a spatial pattern of *WUE*, global monthly GPP and E_{T} estimates over 1982-1985 were obtained from 8 ecosystem models from MsTMIP (Huntzinger et al., 2013), including: (i) CLM (Mao et al., 2012); (ii) CLM4-VIC (Li et al., 2011); (iii) ISAM (Jain et al., 1996); (iv) TRIPLEX (Peng et al., 2002); (v) LPJ-wsl (Sitch et al., 2003); (vi) ORCHIDEE-LSCE (Krinner et al., 2005); (vii)

SiBCASA (Schaefer et al., 2008); and (viii) VISIT (Ito, 2010)."

MsTMIP is a model comparison program, which is similar to TRENDY. All models participated in MsTMIP are forced by observed meteorological variables. Noting that the same modelling outputs of these ecosystem models contributed to both MsTMIP and TRENDY."

R3C17: 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.
Reply: The details of E_P calculation using the Shuttleworth-Wallace model are provided in Section 2.1.2 (please see our reply to R3C15). In the Shuttleworth-Wallace model, PET is also a function of vegetation parameters (leaf area index and stomatal conductance). Using the Shuttleworth-Wallace model allows to quantify the effect of eCO$_2$ on PET via vegetation feedbacks. If PET was an 'open water PET' then we would not be able to quantify the effect of eCO$_2$ on E_P via vegetation feedbacks.

R3C18: Equation 12: should the notation be f() instead of g()?
Reply: Using f() and g() indicate that the two functional relationships are different. That is, they are not the same function, hence it is appropriate scientific protocol to use different letters to denote these relationships.

R3C19: L218: Which years were used?
Reply: The year 2001 was used. This is now made clear in the revised manuscript (Line 268).

R3C20: L220: You should provide the name of the dataset (i.e. ISLSCP etc.)
Reply: Done. Revised as suggested the relevant text reads (Line 269-272): "The global C4 vegetation fraction was obtained from the International Satellite Land Surface Climatology Project (ISLSCP) Initiative II C4 vegetation percentage dataset (Still et al., 2009; http://webmap.ornl.gov/ogcdown/dataset.jsp?ds_id=932)."

R3C21: 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

Reply: Done. We have revised the caption of Figure 2 (original Figure 1) as: "**Figure 2** Spatial distributions of (a) resource availability index categories and (b) climate aridity zones over global vegetated lands for 1982-2010. For the land surface blank areas are non-vegetated regions. Respectively there are 2536, 8194, 10316, 12930 and 9093 0.5° × 0.5° resolution grid-cells in the 0.0-0.2, 0.2-0.4, 0.4-0.6, 0.6-0.8 and 0.8-1.0 resource availability index categories."

R3C22: L232, L234: missing full stop

Reply: Done. Revised as suggested.

R3C23: 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.
Reply: Done. Revised as suggested and following R1C7, we now used "g_s" to represent stomatal conductance. Revised text reads (Line 300-301): "All those field experiments report a reduction of g_s in response to eCO_2, with the largest g_s reduction found in C4 crops and lowest in shrubs for the same level of eCO_2."

R3C24: 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)
Reply: This reviewer was correct that both C_a and v are considered in our estimation of how L respond to changes in C_a. The reason that L increases with elevated CO_2 is because that WUE increases with elevated CO_2. However, the response of WUE to elevated CO_2 is also mediated by changes in v. In our modelling framework, we firstly estimate how WUE changes and then how L respond to changes in WUE. In the revised manuscript, we have stated that "The response of L to eCO_2 was predicted based on the response of WUE to eCO_2 adjusted by the local resource availability." (Line 220-221).

R3C25: 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
Reply: In the revised manuscript, we removed all catchments analyses (other than the catchment validation that was originally Figure 3 and is now Figure 4) and applied our attribution framework to the entire global vegetated lands (also see our reply to R3C28). The spatial distribution of eCO_2-induced Q changes is now shown in Figure 6a (in relative units) and 7d (in absolute units).

R3C26: 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
Reply: Errors in the caption are corrected. In the revised figure, we show one standard deviation among grid-boxes using figures instead of error bars. This helps to better compare the difference in means (showing standard deviation as error bars would make the scale of Y-axis too large to compare the difference in means).

R3C27: 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?
Reply: Oops! Sorry for the typo. The change should be -2.3 mm yr^{-1}.
In the revised manuscript, since we conducted our analysis across the entire vegetated lands, this value changes to -2.2 mm yr^{-1} (Line 327).

R3C28: 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.

Reply: In the revised manuscript, we conducted our analysis across the entire global vegetated lands, and only used the Q data from the 2,268 catchments to validate the modelled Q. The issue of transition from catchment to the entire globe is not relevant anymore. We revised relevant text to accommodate this change (e.g., Line 67).

R3C29: L292: Given alpha is determined from LAI, surely low-alpha regions can be either dry or cold?
Reply: Yes, this reviewer was correct. We have made it clear in the revised manuscript (Line 82-84). Relevant text reads: "Resource availability is typically low in dry (and/or cold) environments and increases as the climate becomes more humid"

R3C30: 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.
Reply: Originally, our idea, which we agree we did not articulate well, was that the response of LAI to eCO$_2$ along the resource availability gradient reported in herein (also observed by FACE) may provide some useful guidance to the development of ecosystem model, as current ecosystem models perform poorly in representing this response. In addition, the current ecosystem models do not (at least explicitly) consider the response of plant roots to changes in CO$_2$, and this response should be considered in future ecosystem model development. However, as this discussion is not concrete in the revised manuscript we deleted relevant statements about future model development thus avoiding any potential misunderstandings.

We do not agree with this reviewer on the assessment of "the results from this study are very much in line with existing studies so no particularly novel insights are revealed." The current study firstly shows the opposite overall effect of eCO$_2$ on Q via vegetation feedback between low resource availability regions (or low LAI regions, typically dry and/or cold) and high resource availability regions (or high LAI regions, typically warm and wet). Importantly our manuscript is the first study to explicitly acknowledge that eCO$_2$ change impacts below-ground vegetation components and uses this with above-ground impacts to assess Q characteristics.

R3C31: L351: Are all the datasets publicly available?
Reply: Yes, all datasets are publicly available. All data sources are provided in the Data section of the main text.

Thanks for your careful review of our manuscript, addressing your comments has resulted in an improved manuscript.