Response to Reviewers' comments

We greatly appreciate the reviewers providing valuable and constructive comments on our manuscript HESS-2019-701. We seriously considered each comment and will revised/improved the manuscript accordingly. The individual comments are replied below. In the following the reviewer comments are black font and our responses are blue and to assist with navigation we use codes, such as R1C2 (Reviewer 1 Comment 2).

Anonymous Referee #1

R1C1: General comments: This paper analyzes projected changes in PDSI. It compares PDSI estimates obtained using potential evapotranspiration with and without accounting for the response of vegetation to increasing atmospheric CO2 concentration, as well as a direct estimate based on hydrological output from CMIP5 climate models. The main point is that there is no significant global drying trend based on PDSI, and the reason this was previously suggested is that offline impact models did not account for the response of vegetation to increased CO2. As noted by the authors (page 3, lines 54–57), several recent studies have already pointed out this problem when computing ET offline.

The valid point the authors make of refuting a general rule of "warming leads to drying" should not be interpreted as there will be no drying. The authors could try to make this even clearer by further emphasizing the projected increase in land area fraction under extreme conditions of water availability as well as the uncertainties in the projections.

Overall, the manuscript is well-structured and clearly conveys its main point. Nonetheless, it would be useful to further discuss some aspects of the methodology and address potential caveats of the PDSI.

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

Specific comments

R1C2: Although PDSI has been a widely used index, it is not exempt from caveats. When analyzing projected changes in drought (water availability) it would be beneficial to also directly show the changes in relevant variables like soil moisture and precipitation minus evapotranspiration. Although results for SPEI are presented in the supplement, a summary of trends in projected soil moisture anomalies would be a suitable complement to the manuscript. Particularly, maps of the trends would provide

a more comprehensive picture as opposed to the global averages.
Reply: The maps of trends in soil moisture and precipitation minus evapotranspiration have been shown in a few previous publications (e.g., Berg et al., 2017; Greve et al., 2017; Swann et al., 2016; Yang et al., 2019); we have cited these papers and summarized/discussed their findings in the manuscript.

An important motivation of this study is actually based on these previous findings that total soil moisture does not show notable changes and precipitation minus evapotranspiration (or runoff) shows a slightly increase but estimated drought increases substantially in the coming century. The current study is designed to solve this contradiction. Several studies have pointed out the issue of ignoring the CO2 effect in offline ET (and/or runoff) estimations (as noted by the reviewer), with the findings have important implications on drought changes. This study goes one step further by directly focusing on drought, using a widely drought index – PDSI.

The spatial patterns of PDSI trend are shown in Figure 3. The global averaged PDSI series was intended to give an overall comparison between different PDSIs at the global scale (given comments by the editor and other reviewers, we will delete this global average PDSI series in the revised manuscript).

R1C3: It appears that the climatically appropriate for existing conditions (CAFEC) coefficients are estimated for the entire period 1901–2100 (if this is the case, it should be explicitly stated). This seems counterintuitive to me when analyzing projected changes. Why would it not be more meaningful to estimate the soil moisture anomalies relative to some reference conditions from the past or present, e.g. 1901–1960 as for SPEI in Fig. S4?
Reply: Both PDSI and SPEI are calculated for the entire period 1901-2100 (both indices calculate the monthly departure from climatological means, and the climatological means are computed as the mean over 1901-2100). With the calculated SPEI series, in Fig. S4, we show the long-term SPEI change relative to the 1901-1960 mean to better highlight the changes. We will make it clear in the revised manuscript.

R1C4: It would be relevant to discuss and/or provide sensitivity tests to assumptions underlying the calculation of PDSI. For example, what value was selected for the available water capacity (AWC)? Is it constant in space? Are the values model dependent?
Reply: We will explicitly state all relevant data/parameters used for PDSI calculation. The sensitivity of PDSI to AWC has been examined in a previous study (Sheffield et al., 2012), and the authors found that changes in AWC have only very minor impact

on PDSI estimates. We will discuss this point in the revised manuscript.

R1C5: It would be insightful to know more about the variability of PDSI given that all data is already available. For example, maps of change in the standard deviation of PDSI from a future period relative to present-day can be shown in the supplement.
Reply: Thanks for the suggestion. We will provide relevant results in the revised manuscript.

R1C6: The manuscript concludes (page 11, lines 273–274) highlighting the increased spatial variability in surface hydrological conditions. In this context, it could be appropriate to also discuss local changes in temporal variability, see Kumar et al. (2013). Kumar, S., Lawrence, D. M., Dirmeyer, P. A. & Sheffield, J. Less reliable water availability in the 21st century climate projections. Earth's Futur. 2, 152–160 (2013).
Reply: Thanks for the suggestion. We will discuss this point in the revised manuscript.

R1C7: Page 6, lines 129–132: Potential for discussion. Differences between total soil-depth representation in CMIP models may lead to systematic differences in PDSI estimates from individual models.
Reply: Reply: Thanks for the suggestion. We will discuss this point in the revised manuscript. In addition, we will update our results by showing mean and range (estimates from all individual models) instead of standard deviation in the revised manuscript. This better shows the difference between individual models.

R1C8: It should be noted that the discussed response of vegetation to increasing CO2 applies to transpiration, but not to evaporation from the soil and canopy as well as snow sublimation. In this case, increasing CO2 and temperature would have a direct effect towards increasing evaporation.
Reply: We agree with this point. We focus here the question: what would be the difference if we consider or ignore the vegetation response to elevated CO2. Increased CO2 will lead to warming through enhanced radiative forcing; this effect is reflected in the used meteorological variables (e.g., air temperature) as well as in the direct hydrological outputs of CMIP5 models (e.g., total ET). All those variables have been used in the calculation of PDSIs. We find that despite the increasing temperature (and likely increasing soil evaporation and snow sublimation), PDSI_CMIP5 only shows minor changes compared with PDSI_Penman that ignores the CO2 effect on transpiration.

R1C9: Fig. 3 shows that even for direct CMIP5 output there can be a considerable

increase in the land fraction experiencing extreme drought/moist conditions. These areas could be even larger if we were to consider the full spread of the CMIP5 ensemble as opposed to plus/minus one standard deviation. Is it reasonable to consider that differences in how individual models represent the response of vegetation to increasing CO2 could explain the spread in CMIP5 projections? This may be an important discussion point for the paper.
Reply: Thanks for the suggestion. We will discuss this point in the revised manuscript. In addition, we will update our results by showing mean and range (estimates from all individual models) instead of standard deviation in the revised manuscript to better show the difference between individual models.

R1C10: What is the reason why this particular subset of 16 CMIP5 models was used and not all models that are available?
Reply: These particular 16 CMIP5 models were used because these 16 models provide all outputs we need, in particular runoff estimations.

R1C11: Trends in vegetation greening are mentioned in the abstract. The following reference about hidden vegetation browning could be helpful. Pan, N., Feng, X., Fu, B., Wang, S., Ji, F. and Pan, S.: Increasing global vegetation browning hidden in overall vegetation greening: Insights from time-varying trends, Remote Sens. Environ., 214, 59–72, doi:10.1016/J.RSE.2018.05.018, 2018.
Reply: Thanks for the suggestion and we will mention this in the revised manuscript.

Technical comments
R1C12: In Palmer (1965), equation 26 appears to use monthly recharge (R) instead of long-term average R. This might be worth double checking since it seemed to me the average is used in the provided scripts.
Reply: The equation 26 in Palmer (1965) does not use monthly R but monthly climatological R: long-term mean R for each month. We follow that.

That equation is to estimate monthly weighting factors, so each month has only one weighting factor.

R1C13: Lines 45 and 225: Inconsistency in the reference Lehner et al., 2017 or 2018? There is only one reference entry.
Reply: Apology for the typo. We will correct it in the revised manuscript.

R1C14: Page 10, lines 23: I would delete the word "also" since the effects are opposite.

Reply: Thanks for your suggestion. We will update the text following your suggestion.

R1C15: Page 11, line 273: Is Fig. 3b–f correct? Or Fig. 3b–c?
Reply: Apology for the typo. We will correct it in the revised manuscript.

R1C16: Figure 3: The selection criteria for where to have the black dots does not seem optimal. As it is now, it is showing all regions where the mean and median of PDSI have the same sign. I would suggest a different threshold for model agreement, e.g. black dots where at least 2/3 of the models agree in sign. Alternatively, it could be useful to include in the supplement maps of model agreement that are complementary to Figs. 3d–f.
Reply: There is a typo in the caption of Figure 3. The black dots actually show the same sign detected in at least 13 models (so >80%). We will correct it in the revised manuscript.

R1C17: Page 8, line 194 and 196: It should be Fig. 4.
Reply: Apology for the typo. We will correct it in the revised manuscript.

References:
Berg et al., Divergent surface and total soil moisture projections under global warming, Geophysical Research Letters, 44, 236-244, 2017.

Greve et al., Simulated changes in aridity from the last glacial maximum to 4XCO2, Environmental Research Letter, 12, 114021, 2017.

Swann et al., Plant response to increasing CO2 reduce estimates of climate impacts on drought severity, PNAS, 113, 10019-10024, 2016.

Yang et al., Hydrological implications of vegetation response to elevated CO2 in climate projections, Nature Climate Change, 9, 44-48, 2019.

To Anonymous Referee #2:

R2C1: General Comments This report is a welcome contribution to the ongoing discussion in the literature regarding how to characterize changes in drought incidence under the changing climate. The paper is a follow-up to the paper by Yang et al (2019), which presents an equation that generally captures the variation of effective stomatal resistance within CMIP5 models as a function of atmospheric carbon dioxide concentration. In this paper, that relation is used to show how a popular drought index (the PDSI) can be adapted to characterize drought in our world of greenhouse warming. A readily available and simple offline alternative to the usual PDSI (and, in particular, to the Allen et al. form of the Penman-Monteith equation) will likely be of value to climate-change impacts analysts, many of whom may not be familiar enough with the biological processes in play or have the resources to model the processes with greater fidelity. That being said, it is important to evaluate the performance of the modified index carefully and to lay out clearly the assumptions and limitations in one place.

To some extent, the literature in this area has had a certain feel of X-vs-Y to it, X being increase of drought, and Y being no change in drought to speak of. This paper moves a bit toward the middle in acknowledging increases in drought incidence, but the overall presentation still has the feel of Y. Some specific suggestions for movement toward what might be a more balanced presentation are offered below for the authors' consideration.

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

Specific Comments:

R2C2: The title "Little Change. . ." (which echoes that of Sheffield et al.) places the paper in the Y category mentioned in the General Comments above. To me, and perhaps to other readers, "little" implies something along the lines of "nothing to worry about." The authors might consider modifying the title to avoid that implication.

Reply: Thanks very much for the point. We will change the title to: Vegetation response to elevated CO_2 reduces drought increase under warming in climate projections.

To editor (Prof. Ryan Teuling): Given the suggestion from this reviewer, we have decided to change the title (shown above). As a result, to also follow your suggestion, we will remove the global mean PDSI series but focus our analyses on spatial patterns and areas/time under drought in the revised manuscript.

R2C3: The reference to “PDSI” in the title, without qualification, is potentially confusing. Other publications (as well as this one) have shown that the usual PDSI equation applied to climate-change projections do imply increased drought. Would it be appropriate to change “PDSI” to “Co2-aware PDSI” or something else that conveys that idea?
Reply: Please see our reply to R2C2.

R2C4: In general, the paper does a good job of citing the relevant literature. However, it’s not clear to me that the abstract does justice to the previous literature (including the authors’ own works) when it uses the phrase “resolve a paradox.”
Reply: We will remove the phrase “resolve the paradox” in the revised manuscript.

R2C5: It’s not immediately apparent what it means for the abstract to say that “global PDSI_CMIP5” remains generally unchanged. If this refers to the global average of the time average of the ensemble average of PDSI, then it is possible that the element of variability in space, in time, and across models could be lost in translation. It’s hard to think about “drought” without considering variability.
Reply: We will rewrite the abstract to avoid any misunderstanding/misinterpretation of such.

R2C6: The statement in the abstract that “projected increase in PDSI drought reported previously is primarily due to ignoring the vegetation response” seems somewhat overstated when I look at Figure 3, which suggests that the increase is about 50% or so due to ignoring the biological response, leaving another 50% that is not due to that.
Reply: We will rewrite the abstract to avoid any misunderstanding/misinterpretation of such.

R2C7: I did not carefully evaluate what was implied by lines 138-140: “The PDSIs were calculated using outputs of each CMIP5 model in turn, and the ensemble PDSIs (averaging PDSIs over models) were used in the following analyses,” but that passage gave me pause. Won’t averaging across models reduce both the temporal and spatial variability and thereby impact drought estimates?
Reply: We will update our results of Figure 3 to also show the range of drought/moist areas projected by all individual models. In addition, results in Figure 5 are not ensembles but the results agreed in at least 8 models.

R2C8: lines 208-210. The criterion for substantial increase in drought appears to based on the change in the average value of PDSI rather than the change in the

exceedance of a threshold. Is that a good measure?
Reply: The focus here was the increase in drought (decrease in PDSI by definition). A place changes from extreme moist to mild moist is also considered as an indication of potential drought increase. The same analysis has been applied in a few previous studies (e.g., Liu et al., 2018) so we use the same approach to be able to compare with results from others.
Changes of PDSI exceeding a certain threshold indicate changes area under drought, and this results in given in Figure 3.

R2C9: lines 210-212. It was stated earlier that ensemble averages were used for the analysis. It's quite possible I've read through the paper too quickly; the authors might consider taking precautions to avoid letting the casual reader get confused.
Reply: This is not the ensemble result; instead, it is the result that shared by at least 8 models.

R2C10: lines 233-235. I am confused by "yet on the other hand" (which by the way sounds redundant in itself) combined with "also," since both effects are working in the same direction. The "also" seems out of place here. I get that "also" here was meant in the sense of "and here's another thing it does," but the current sentence structure doesn't work for me.
Reply: We will restructure the sentence to avoid any misunderstandings.

R2C11: line 242-247. I think this is another place where the authors could relax away from the "Y" position mentioned in the general comments. It seems to me that the dryness near the surface might be important for wildfire risk and perhaps for various biological processes that take place close to the surface. This idea might even be allowed to bubble up into the abstract.
Reply: Thanks for your suggestion. We will extend the discussion on the possible implications of surface soil moisture change and will consider to also mention that in the abstract.

R2C12: Figures 1 and 2. The creation of these figure to convey what's going on is appreciated. Figure 2 takes a while to understand. It might help if the four black arrows and the plus signs were removed. It also might help if there were another column for how Ep is computed.
Reply: We will remove the four black arrows and the plus signs in the revised manuscript. The second column (under Meteorological Inputs) shows how E_P was computed for each PDSI.

R2C13: Figure 3. A map of trend in PDSI doesn't seem as useful as a map of trend in exceedance of some substantial value of PDSI.
Reply: The map of PDSI trend gives a general information on how PDSI changes. We also show trend of area under drought (PDSI exceeds a certain threshold). Following your suggestion, we will add a map showing time (i.e., the number of months) in each year with a PDSI value exceeds a certain threshold.

R2C14: lines 435-436. "where the same sign of the PDSI trend is identified in at least 8 out of the 16 CMIP5 models.." Taken alone, without additional explanation in the caption, it seems like this would be true everywhere.
Reply: There is a typo in the caption of Figure 3. The black dots actually show the same sign detected in at least 13 models (so >80%). We will correct it in the revised manuscript.

R2C15: Figure 4. This figure averages out a lot of information. Do the benefits of its inclusion outweigh the possible misunderstanding that it might generate? See also related comment above regarding "global PDSI_CMIP5" in abstract.
Reply: As also suggested by the editor, we will remove this figure in the revised manuscript.

R2C16: Figure 5. As mentioned elsewhere, change in (expected value of) PDSI might not be the best metric for change in drought. Change in exceedance of thresholds might be better. I wouldn't be surprised if these were quite parallel, but to leave that taken for granted could weaken the overall impact of the paper.
Reply: Please see our reply to R2C8.
In addition, we understand this reviewer's concern of using changes in PDSI as the measure. However, using changes in exceedance of thresholds may also incur other issues. For example, if we chose PDSI = -1 as the threshold for drought, then locations having a PDSI = -2 in the baseline period and a PDIS = -1.5 in the future period (or PDSI = -1.5 in the baseline period and PDSI = -1.51 in the future period) will be identified as places with a substantial drought increase.

Technical Corrections
R2C17: line 93. Delete "and"
Reply: Will revise in the manuscript.

R2C18: line 233. Change "increase" to "increases"
Reply: Will revise in the manuscript.

R2C19: line 270-271. Consider changing “due to the ignorance of” to “due to ignoring.”
Reply: Will revise in the manuscript.

R2C20: line 285. Change semicolon to period.
Reply: Will revise in the manuscript.

R2C21: line 433. Change “e-f” to “d-f”
Reply: Sorry for the typo. Will revise in the manuscript.

R2C22: line 287. Add period.
Reply: Will revise in the manuscript.

References:
Liu et al., Global drought and severe drought-affected population in 1.5oC and 2oC warmer world, Earth System Dynamics, 9, 267-283, 2018

To Reviewer #3:

R3C1: This very innovative and important study shows that when the familiar Palmer Drought Severity Index (PDSI) is computed directly from global Earth System Model output of precipitation, evaporation, runoff and soil moisture storage (rather than box-modeling all those quantities from an offline-computed potential evaporation of questionable accuracy as is traditionally done), the dire projections of ubiquitous future global drought from those traditional studies vanish. Instead, the PDSI projections become both wetting and drying depending on region, consistent with the *direct* simulations of runoff, deep-layer soil moisture, etc. by the ESMs but not with the traditional PDSI studies.

This is a key methodological advance and shows that the PDSI index itself is not flawed under climate change, rather its known problems stem from the traditional potential evaporation input which is inaccurate, leading to inaccurate inferred water flux changes. The inaccuracy of the traditional potential-evaporation input is because leading-order biological effects of changing CO2 and vapor pressure are taken into account in the ESMs but not in the potential-evaporation calculation, as the authors show well here.

I recommend only minor revisions before publication, since I was anonymous Reviewer #2 on the earlier version of this study that was originally submitted to Environ. Res. Lett., and I already had my concerns largely addressed during that review process at that journal. My strongly recommended minor revisions are listed below.

Reply: Thanks for your favorable evaluation of our study. Your individual comments are replied below.

R3C2: 30: This kind of parenthetical remark/qualification is appropriate for the body text, but I don't think is needed for the Abstract - it makes the Abstract too complicated and clunky. At least, that is how I read it. So I think you should either remove or greatly shorten this remark. You can put something like this in the body text instead.

Reply: We will remove this in the revised manuscript.

R3C3: 54-57, 94-96, 122, 226, 271, 418: Should also mention the impact of increasing/elevated vapor pressure deficit, as you do in the Abstract. The direct effect of CO2 is only part of the story, as you explain well at 235-237 but the text does not reflect here at all.

Reply: Will do in the revised manuscript.

R3C4: 88-89: It should be clarified here that this corresponds to the center stream in Figure 1, parallel to how you point out the right stream and left stream later in the paragraph.
Reply: Will do in the revised manuscript.

R3C5: 117-120: Similarly, this should mention that it is the right stream in Fig 1.
Reply: Will do in the revised manuscript.

R3C6: 120-123: Similarly, this should mention that it is the left stream in Fig 1.
Reply: Will do in the revised manuscript.

R3C7: 126-135: Similarly, this should mention that it is the center stream in Fig 1.
Reply: Will do in the revised manuscript.

R3C8: 178: Should be 3d, not 3e.
Reply: Sorry for the typo. We will correct it in the revised manuscript.

R3C9: 238: As stated in previous review for Environ. Res. Lett., "our results" on this line will be read by most readers as meaning "the current study" (even though that's not what you actually mean.) Since it's actually Yang et al (2019) that showed this key fact (not the current study), this needs to be rephrased to make that clear. It's largely the same authors, but different study, and the distinction is important.
Reply: We will rephrase this sentence in the revised manuscript.

R3C10: Fig 2: Caption should point out which rows respectively correspond to the left, right and center streams of Fig 1.
Reply: Good suggestion; we will update the caption of Figure 2 in the revised manuscript.

R3C11: Fig 3d-f: Stippling when >50% of models agree on sign of change is trivial - this will almost always be true (unless exactly 8 models have increase and exactly 8 have decrease.) Rather, you should stipple when, say, >67% or >80% of models agree on sign of change. This better filters out changes that are just noise.

It is true that I suggested 50% threshold in previous review, but that was for models with dPDSI < -1, not for basic sign of change! 50% makes sense if the criterion is dPDSI < -1 because that's not likely to occur by chance. But it doesn't make sense for dPDSI < 0 or dPDSI > 0, since that occurs most of the time by chance (unless *exactly* 8 models happen to have a decrease!)

Reply: We are sorry about this but there is a typo in the caption of Figure 3. The black dots actually show the same sign detected in at least 13 models (so >80%). We will correct it in the revised manuscript.

R3C12: Supp Fig S1: This is greatly appreciated, but I think it would have an even greater impact if you reversed the color scale in panels b and c (i.e. make negative green/blue, and positive yellow/brown.) This is because in this context we are thinking of E as a loss term in the water budget, and so increasing trend of E -> more drying. (I know that in other contexts/purposes more E -> wetter, but here the purpose is clearly to indicate that panel c is not as "drying" as panel b, so the colors should intuitively reflect that!)

Reply: Thanks for the suggestion. We will recolor the curves in the revised manuscript.