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Discussions

Interactive comment on "Little change in Palmer Drought Severity Index across global land under warming in climate projections" *by* Yuting Yang et al.

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Reply to interactive comment on "Little change in Palmer Drought Severity Index across global land under warming in climate projections" by Conrad Jackisch.

Comment (1): Yang, Zhang and co-workers have presented an application of their CO2-dependent modification of the Penman-Monteith equation to estimate ETP (Yang et al., 2019) for 16 CMIP5 climate projections (monthly until 2100). Based on the Palmer Drought Severity Index (PDSI) they work out the argument that inconsistencies arise, when only the hydrological variables are considered and adversary effects of higher CO2 on stomatal conductance is neglected. They show that using the variables

of the climate model predictions directly or their modified Penman-Monteith approach leads to consistent projections of no increase of global drought under climate change, which is very different compared to PDSI calculations neglecting the effect of CO2 on transpiration. The presented study is highly relevant and covers one important aspect of current ecohydrological sciences under climate change. The manuscript is concise and well structured. It is transparently reporting the methods and results including the used matlab scripts. Since I have done a similar study recently (calculating the PDSI based on different climate model projections), I got interested in the study. Despite all merits for the study and without claiming to have a practical solution to the problem, I have some concerns about the fundamental assumptions of the used approach.

Reply: Thanks for your encouraging comments and our reply to your specific comments are given below.

Comment (2): Only CO2-effects reducing transpiration considered. The authors consider two mechanisms: 1) that elevated atmospheric CO2 directly reduces stomatal opening and 2) that higher CO2 concentrations rises air temperature and leads to increased vapour pressure deficit and thus again stomatal closure. Hence both assumptions imply a reduced transpiration. Thus, the finding of their model might not be a result of competing mechanisms but of the assumptions and problem framing. As much the authors argue for a more broad conscious about CO2-effects, they neglect that stomatal conductance is not uniquely coupled with photosynthesis but also with cooling and other physiological processes. If plants could only reduce stomatal conductance, leaf temperatures would likely increase above operable levels. Urban et al. (2017) have shown such effects of stomatal opening under leaf temperature increase for cooling. They base their findings on measurements under controlled conditions separated from the effect of vapour pressure deficit on poplar and pine trees. Their results suggest that under stress photosynthesis and stomatal conductance become decoupled and thus transpiration could still increase with higher CO2 and temperature.

Reply: We absolutely agree with the additional mechanisms you mentioned. All these effects and potentially others (which you did not mention in the above comment) are considered in our calculation of PDSI_CMIP5, since we use direct outputs of CMIP5 models to drive the PDSI calculation (so a fully coupled approach), if those mechanisms were also considered in the underlying CMIP5 models. The use of a CO2-corrected Penman-Monteith model and the corresponding PDSI (i.e., PDSI_PM[CO2]) is to demonstrate that previous overestimations of drought changes using an offline approach (i.e., PDSI_PM-RC) is primarily caused by ignoring the vegetation response to elevated CO2, as demonstrated by a very close agreement between PDSI_PM[CO2] and PDSI_CMIP5 but a much more stronger drought increase by PDSI_PM-RC. It should be noted that there are still differences between PDSI_PM[CO2] and PDSI_CMIP5, suggesting that only accounting for the CO2 effect as we conducted here is not able to fully recover the coupled-processes in CMIP5 models. Nevertheless, the method we used to correct CO2 effect in the calculation of potential evapotranspiration in the Penman-Monteith model still provides an effective and simple (yet imperfect) way for offline assessment of hydrological changes in climate model projections.

Comment (3): Penman-Monteith Equation. Moreover, the Penman-Monteith Equation (which is fundamental to the study) has been criticised for limited capabilities to cover the actually claimed functionality (eg. Schymanski and Or, 2017) and to be consistent within the energy balance (eg. Kleidon and Renner, 2018). While from a practical point of view there is good reason to base studies on this equation, this cannot replace empirical evidence and/or detailed discussion of the implicit assumptions. Hence, the claim of the authors to be more correct with their "modified" model version without proper analysis appears a little weak.

Reply: We agree with this reviewer on the fundamental assumptions and limits of the Penman-Monteith model. However, the focus of this study is to demonstrate that previous estimates of PDSI using an offline approach without considering the CO2 effects deviates a lot from the underlying PDSI that can be calculated directly using CMIP5

model outputs of precipitation, evaporation, runoff and soil moisture. In that context, discussion and analysis of the limitations of the Penman-Monteith model are well beyond the scope of this study.

The claim of our “modified” model is more correct is well demonstrated by a close agreement between PDSI_PM[CO2] and PDSI_CMIP5, as well as in Yang et al. (2019, NCC, https://doi.org/10.1038/s41558-018-0361-0). In Yang et al. (2019), surface resistance is derived by inverting the Penman-Monteith model using direct outputs of actual evapotranspiration and other relevant meteorological variables, and they find that surface resistance (in the CMIP5 models) is very closely related with CO2. Yang et al. (2019) also acknowledged that the corrections they made is only direct relevant for analyzing CMIP5 model outputs (so is relevant in the current study), whether and to what extend the proposed correction could represent the real world remains an open question.

In short, a formal (and appropriate) assessment of PDSI changes in CMIP5 model should be based on PDSI_CMIP5 (as we highlighted in the manuscript, see Figure 5), and the correction of Penman-Monteith model by considering CO2 is one way that this can be done although we recommend the approach in PDSI_CMIP5.

Comment (4): Palmer Drought Severity Index. The PDSI calculates a very simple water balance – in the presented case with monthly time step. This implies a further hypothesis, which is about water availability to be evenly distributed over a month plus full water redistribution into the rhizosphere. Because water availability is another important control of stomatal conductance, the approach using PDSI on monthly data might overestimate water availability which would be in line with the reported findings?

Reply: The monthly time step is used because it is the most commonly time step used for PDSI calculation in a great many existing studies, and for long-term drought changes in other drought indices as well (e.g., SPEI).

Comment (5): Conclusion. There are many more aspects, which have to be and have been considered to predict responses of vegetation to elevated atmospheric CO2 concentrations and temperature (which I have no doubt that the authors are aware of and partly participated in). Despite the freedom of the study to focus on one aspect alone, I find it difficult to allow for the main conclusion of the study based on the given situation of i) a model which cannot account for trade-offs between different plausible effects, ii) very large scale and high level of aggregation, and iii) many implicit assumptions which have not been addressed. I find it very helpful that the authors point out difficulties and traps of climate model output interpretations with respect to drought stress based on the PDSI and offline applications. In this respect, the manuscript makes a point, which is worth to be worked out. However, I do not see that the findings really refute the common "warming leads to drying" perception. Maybe a more detailed analysis and discussion of the Penman-Monteith model and measures to evaluate drought/wetness could be a way to substantiate the manuscript?

Despite all critics, I thank the authors for their work and the transparent presentation of their study. I think this is a good example how the open standards lead to higher quality and progress in our sciences.

Reply: We well understand this reviewer's points and concern, and we readily acknowledge that CMIP5 models may not be complete. However, we again note that the PDSI_CMIP5 accounted for all the coupled processes that have been considered in the CMIP5 models. We also acknowledge that there may be large uncertainties in the CMIP5 models, but that is not an issue this study deals with. Here we are interested in obtaining an accurate representation of the model outputs. That is also the reason that our title includes a statement of ". . .. in climate projections". The conclusion of "warming does not lead to drying" is based on CMIP5 model projections: under widespread and persistent climate warming, some places show a drying trend and others show a wetting trend with little average changes on a global scale.

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