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

Conrad Jackisch

c.jackisch@tu-braunschweig.de

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Yang, Zhang and co-workers have presented an application of their CO_2-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 CO_2 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 CO_2 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.

1 Only CO_2-effects reducing transpiration considered

The authors consider two mechanisms: 1) that elevated atmospheric CO_2 directly reduces stomatal opening and 2) that higher CO_2 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 CO_2-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 CO_2 and temperature.

2 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.

3 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?

4 Conclusion

There are many more aspects, which have to be and have been considered to predict responses of vegetation to elevated atmospheric CO_2 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

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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.

All the best.

Conrad

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