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Hydrology and Earth System Sciences Discussions

Interactive comment on "Contributions to uncertainty in projections of future drought under climate change scenarios" *by* I. H. Taylor et al.

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General comments

This is a generally well-written paper on a relevant topic, based on innovative Earth system modelling simulations. I have however three general comments detailed below that need to be addressed.

Physical and socio-economic droughts

There is a recurrent ambiguity/confusion between physical droughts – some of which are actually studied in this paper – and socio-economic droughts, mainly in the introduction (P12616, L11-12, P12616, L24-25, P12617, L3-6) and discussion (P12633, L7, P12636 L4-6, P12637, L13-14). See all specific comments below. This distinction is crucial, as physical droughts are hazards, and socio-economic droughts are the results of hazards and vulnerability, which is specific to each anthropogenic hydrosystem (be it a hydropower plant or a crop field). As clearly defined by Wilhite and Glantz (1985), socio-economic droughts "incorporate features of meteorological, agricultural and hydrological droughts". It should therefore be made clear that this study is only about (some types of) physical droughts. Additionally, any conclusion about socio-economic droughts cannot really be reached at the global scale unless anthropogenic hydrosystems are explicitly modelled into the Earth System.

Definition of uncertainties

Although you appropriately distinguish between actual uncertainties (ensemble members and emissions/concentration scenario) and choices (drought index and drought threshold) P12618 L9-14, you consider in this study the above choices as uncertainties. This is closely linked to the issue raised by Justin Sheffield. First, different drought indices represent different things, and the choice of a specific index is dictated by its vulnerability to a specific type of drought. For example, a high-altitude hydropower reservoir or a rainfed rice crop is mainly dependent on meteorological drought, a wheat crop on agricultural (edaphic) drought (which combines effects of precipitation and evaporation), and a run-of-river hydropower plant or a drinking water scheme on hydrological drought. Then, the choice of a drought threshold also depends on the hydrosystem studied, and more specifically on its degree of vulnerability against water deficits. For example, the cost effectiveness of a hydropower scheme may be positive when streamflow is above a x

Consequently, I would strongly recommend not to include drought index and drought threshold as actual uncertainties. This has no implication on the calculations performed in the paper except for the analysis of contribution from the uncertainty sources (P12625 L25 onwards and P12631 L7 onwards), which should be removed from the manuscript as they are not relevant. This has however large implications on the way to present the objectives of the paper and the results obtained. For example, the current title (and abstract) would no longer holds when drought index and drought threshold are not considered as uncertainties. I would suggest to present this paper in line with something like "What are the impacts of mitigation on future droughts?". The objective of such a paper would then be to compare drought changes between A1B and RCP2.6, across a range of drought indices and drought thresholds. This would be in my opinion much more policy relevant.

Choice of drought indices

You chose here to compute three different indices, one meteorological drought index (the standard WMO-adopted SPI) and two agricultural drought indices. I could not clearly see what the objective is here behind this choice (except only by reusing indices computed by Burke and Brown, 2008). A possible objective could be to compare different agricultural drought indices, like SMA and PDSI. Another possible (and much more attractive) objective could be to characterize droughts along the land surface hydrological cycle, by using indices of all three types of physical droughts. One potentially highly interesting addition to this study would be to compute a runoff-based index in order to see how changes in climate variables impact propagate along the land surface hydrological cycle (see Vidal et al., 2010). If runoff data is available from the ESE, I would strongly suggest to compute a Standardised Runoff Index, as suggested also by

Justin Sheffield.

Moreover, you consider only the 12-month time scale in this study (at least for SPI and presumably SMA), although the response of drought to climate changes has been proved to be highly sensitive to the time scale considered (see e.g. Vidal et al., 2012). If feasible (it would not take that much time now the calculation procedures are implemented), I would recommend to compute SPI and SMA at other time scales (for example 3 months, in order to assess the differences between short and long droughts). If not, I would strongly recommend to clearly acknowledge this limitation of this study.

Specific comments

P12616, L11-12: This sentence is a bit ambiguous, given that (1) you consider only physical droughts in your study and (2) the analysis is restricted to indices of meteorological and agricultural droughts.

P12616, L20-23: Please give a more precise reference than the IPCC Summary for Policymakers.

P12616, L24-25: "This suggests a particular vulnerability in these regions to any projected increases in drought occurrence". You're writing here about vulnerability, so to socio-economic droughts, whereas the previous and following statements about the occurrence of intense and multi-annual droughts, and the extent of drought-affected areas, refer presumably only to physical droughts. Please make it clear, by also giving some more precise references than an IPCC chapter.

P12617, L3-6: "Drought is not solely affected by climatic drivers, non-climatic drivers such as population changes, land use and water management have a large influence on water availability and hence drought (Kundzewicz et al., 2007)" Once again, you refer here to socio-economic droughts, whereas surrounding general statements are about physical droughts.

P12617, L14-19: This not all entirely true. Indeed, as shown by Hawkins and Sutton (2009, 2011), these results depend hugely on two things: (1) the variable considered, temperature or precipitation, and you should therefore refer here also to the 2011 paper on precipitation (I see you mention it later in the discussion), and (2) the region considered, and more precisely its size and location on the earth. For example, internal variability is not the predominant source of uncertainty when looking at the global mean surface temperature (Hawkins and Sutton, 2009). Conversely, emissions scenarios account for virtually nothing even in the second part of the 21st century when looking at the European mean precipitation (Hawkins and Sutton, 2011). All these differences should be made explicit in this paragraph, and conclusions should support the choice of the specific uncertainties considered in this study.

P12617, L28 onwards: You consider one meteorological drought index and two agricultural drought indices. Please justify it (see my general comment 3).

P12618, L9-14: You appropriately mention here that any drought assessment depend on (1) uncertainties and (2) choices. Drought index and drought threshold are choices indeed, but they cannot be considered as uncertainties. See my general comment 2.

P12619, L14 onwards: I don't understand why you consider two scenarios from different families, given that ESE simulations have presumably been done also with other RCPs. Could you justify it?

P12619, L23 onwards: You describe here the ESE as incorporating many parameter uncertainties (with interactions). Why not studying the impact of these uncertainties on drought results? I see in the Lambert et al. (2012) paper that you have meta-parameters (carbon cycle and atmosphere). You could for example perform an analysis of variance (maybe after a variance reconstruction, see e.g. Vidal et al., 2008) with these effects, similarly to what you have done in Sect. 4.3. This way, you could assess the relative importance of carbon and atmosphere parameters on drought changes. I

do not suggest to do it necessarily for the present study, but I see it as a nice follow-up.

P12620, L15: The appropriate reference for the adoption of SPI by WMO is Hayes et al. (2011)

P12620, L21: Please justify the use of SPI12 (12 month accumulation).

P12621, L3-4: unclear. Please also give some details about your SPI calculation method (parametric or not parametric fitting, extrapolation, etc.)

P12621, L16 onwards: Please give some more details about the calculation of the SMA. More specifically, what is the time scale used? 12 months as in Burke and Brown (2008)?. Is a monthly climatology used?

P12622, L26-28: I agree on using Penman-Monteith instead of Thornwaite. But please give some more details about the calculation of potential evaporation, as this variable is hugely sensitive to formulation choices (time step, etc.) and to the quality of input variables (see e.g. Kay et al., 2008).

P12622, L27-28: "this is more suitable for application to climate change scenarios". Could you develop further this statement? (see previous comment)

P12624 onwards: The idea of using an exemplar member is quite interesting. However, basing its selection on an average of responses over different countries make it simply non informative at the regional scale where drought assessments are useful (and potentially used). This limitation should definitely be emphasized in the text. Moreover, in order for you not to redo this selection, I would strongly recommend to show the distributions of responses for each variable and country in order to see how the exemplar member behaves regionally compared to the overall ensemble.

P12626, L1-4: These are not all actual uncertainties. See my previous comments.

P12624, L22 onwards: It should be emphasized here that the time spent in drought (that you choose to study here) is the most basic drought characteristics. However, the impact of a drought is hugely dependent on its spatio-temporal characteristics, i.e. how it develops in time and space and its seasonality (see Vidal et al., 2010).

Fig.3: It would be nice to be able to identify the exemplar member in the final 30-yr average values at the right of each plot.

P12627, L15 onwards: You should specify here (and recall it in the captions) the chosen threshold (10

P12631 L7 onwards: This analysis is in my opinion not relevant. See my general comment.

P12632 L5-8: Presented like this, it is obvious, isn't it?

P12632, L12-14: It is by definition strictly equal (apart from SPI fitting errors) to consider SPI or precipitation percentile thresholds.

P12633, L7 onwards: Any particular drought impact assessment will make use of a specific index and threshold. So the recommendation would be here to adequately choose them to represent the vulnerability of the studied hydrosystem.

P12633 L25: Please give some references for the MOSES2 land surface scheme

P12635 L15: "It would be useful to understand the climatic processes leading to these projected changes to understand more fully how mitigation actions may influence drought occurrence regionally". Well, yes! Any physical insight would be welcome here.

P12636 L4-6: Once again, the choice of drought threshold should be conditioned by the specific hydrosystem under study. Moreover, there are inevitably "threshold effects" when considering socio-economic droughts that are related to thresholds in physical droughts.

Sec. 5.5: This section is in my opinion not relevant, as the analysis mixes actual uncertainties and choices.

P12637, L13-14: "This has important implications for drought management planning in the future." Well, not necessarily, it really depends on which areas are affected. Non vulnerable areas (inhabitated areas for example, but not only) may have large increases in drought without any socio-economic effects. Conversely, an increase in drought in areas with important crop production would have dramatic consequences.

P12637, L23-24: already written above

P12637, L26-29: unclear

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