**Elusive drought: uncertainty in observed trends and short- and long-term CMIP5 projections**

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The paper deals with historic and future meteorological and soil moisture droughts on a regional and global scale, which are obtained from three observation-based gridded precipitation datasets and multi-GCM and scenario simulations. First the paper describes the used indicators (mainly the Standardized Precipitation Index, SPI, and the Soil Moisture Anomaly, SMA), and it identifies drought hot spots across the globe considering drought vulnerability and drought projections. Then past droughts are presented by: (i) testing the consistency in drought trends on a global scale among the models and against the observation-based datasets, and (ii) investigating the trends (magnitude and frequency) in meteorological drought for the identified hot spots (both in the observation-based products and GCM simulations) and soil moisture drought (only GCM simulations). Next, future meteorological and soil moisture droughts are presented for both wetting and drying hotspots in the 21$^{st}$ century. Eventually the evolution of three uncertainty sources over the 21$^{st}$ century for the hot spots is evaluated and compared with a heat index.

I believe that the quantification of different drought types (reconstruction of the past and projections) with an adequate description of current limitations, incl. a measure for uncertainty, is of crucial importance for the further development of our knowledge on hydrological extremes and to the contribution of realistic water management planning that considers potential water scarcity and drought. Hence the paper deals with relevant scientific questions within the scope of HESS. The work mainly presents an analysis of drought using comprehensive new data (AR5: 32-39 GCMs and three GHG scenarios), i.e. CMIP5 simulations. The tools are more or less conventional. Substantial conclusions are achieved (e.g. drought signal-to-noise ratios for the two different drought types using the new dataset).

I found the paper to be well-written and presented and it is potentially a very useful contribution to HESS. However, it needs additional elaboration (see major and minor points in supplement).

**Major Points:**

1. Study is on several places too qualitative, too descriptive. A quantitative analysis should support the remarks/conclusions made. Readers cannot derive this from the maps only. This applies to: (i) intercomparison of maps on economical, physical exposure, crop land distribution, and related to population density. I believe that a quantitative measure is required to support the conclusion “are very similar” (e.g. 13778, lines 23-24)., (ii) Future projections of two drought indicators in the CMIP5 ensemble ... show that some of these hot spots are consistently projected to become even drier during the 21st century... On the other hand, some of the drought hot spots of the recent past are projected to become wetter, ... (13779, lines 8-17), (iii) “This is further supported by the relatively consistent increases in the observational datasets

(Fig. 2c)" (13780, lines 22-23), and (iv) "First, the three observational datasets correlate reasonably well, although the amplitudes are less consistent". (13781, lines 24-25);

2. At other places the methodology/approaches/choices need some elaboration to be understandable or to justify. This holds for: (i) motivation why you use SPI-12 and not another time step (13777, lines 16-17), (ii) Section 2: you need to define somewhere what you mean with "drought magnitude". It is not the deficit volume, which some readers will confuse. In your study it seems to be the standard deviation relative to a threshold, also a standard deviation, e.g. -0.5 or -1.0, and (iii) motivate why RCP8.5 was used to extend the historic time series beyond 2005 (13780, lines 19-20) and not the other more modest GHG concentration scenarios;
3. Section 2.2.1: add equations for SPI-12 where your study adds to existing literature (e.g. McKee et al., 1993; Lloyd-Hughes and Saunders, 2002) on SPI calculation. Eventually, you use the annual averages of SPI12 values and occurrence frequencies of SPI12 below a threshold (captions Figs. 3 and 4). You also used "detrended annual values" (13794, caption Fig. 1). Please make clear how (reference). Equations can be rather simple, but make the paper more transparent and calculations reproducible;
4. Section 2.2.1: add reference(s) and equations how you derive the monthly SMA Monthly ("...are calculated w.r.t. the 1979–2009 monthly means and standardised by the monthly 1979–2009 standard deviations"). Eventually, you used the annual SMA averages and occurrence frequencies of SMA values below a threshold (captions Figs. 5 and 6). Equations can be rather simple, but make the paper transparent and calculations reproducible;
5. Your study compared: (i) annual averages of running SPI12 values with annual SMA averages, and (ii) occurrence frequencies of running SPI12 and monthly SMA values below a threshold. Add motivation why it is allowed to compare SPI12, which essentially averages over 12 months with monthly SMAs (e.g. Figs. 4-6 and Figs. 7 and 8). The temporal scales (memory) are different, even if you calculate as a last step the average annual SMA;
6. The IPCC-SREX report (Seneviratne et al., 2012) concludes that no clear general conclusions on drought can be drawn, a.o. because of confusion about definitions/indicators, tools used, intercomparison of different periods. This paper also is at some places too generic (it says "drought", but only addresses meteorological and soil water droughts and it does not cover hydrological drought, which is of most importance for water resources management, e.g. Stahl et al., 2010; 2012);
7. In the paper you need to make a remark that soil moisture drought indicators derived from GCM' output are not more than indicative, because of the scale (coarse grid) and conceptualization of land-surface processes. These are hard to compare against field data that have a high spatial variability;
8. I wonder how drying trends in soil moisture can be explained by increased runoff when using GCM output that shows increased precipitation or no change in precipitation (e.g. 13782, lines 16-19). Clearly, it can happen in reality and in more sophisticated hydrological models than GCMs, but the latter only have a storage-dependent runoff in their land surface scheme, which does generate lower runoff when storage (soil water) is lower. The only reason for drying trends in soil moisture in a GCM setting with increased precipitation is increased evaporation, which is correctly mentioned as one of the reasons in the paper.;
9. HESS stands for "Hydrology and Earth System Sciences". In the hydrological community we like to make a difference between real observations and observation-

based datasets, like the three gridded precipitation datasets (CRU, CMAP and GPCC). The paper should not avoid the term “observation”, but use instead “observation-based” to make this clear to the reader of HESS (13777, lines 2-3);

10. I do not believe that the Warm Spell Duration Index (WSDI) is really needed in the paper to pass the scientific message. The paper covers sufficient aspects to support the conclusions and recommendations. The WSDI only shows that the GCM formulation never substantially contributes to the total uncertainty, especially beyond the near future. So what? WSDI is only temperature dependent, while the SPI depends on precipitation and SMA on precipitation and the latent heat flux (not purely determined by temperature). We know that the GCMs have reasonable skills in predicting the temperature, which usually outperforms prediction of precipitation and evaporation-dependent weather variables;
11. You mention that SMA cannot be compared against observation or an observation-based product (13782, lines 27-29 and 13783, lines 1-4). However, you could at least try to compare the maps with the work from, for instance, Sheffield and Wood (2008a; 2008b, 2011), who use an independent modeling approach (offline VIC model forced with GCMs or US reanalysis data);
12. Stahl et al. (2010) (Section 3.2 in their paper) report on recent critics on the determination of magnitudes and significance of trends in hydroclimatic time series. Elaborate why your approach (13780, lines 11-15) is acceptable given the concerns about the power of various tests in the presence of auto and cross-correlation;
13. Global maps (Figs. 1 and 2), incl. the legends are too small. Furthermore the top of Figs. 4-6 (map with 12 selected hotspots) has to be given only once. In that case make it larger;
14. SMA should not be used as an indicator of agricultural drought. It is a physical indicator that should be called soil moisture drought. Agricultural drought includes more than only physical aspects. It also comprises economic factors (it is typically an impact indicator). You can say at the start and the end of the paper that SMA provides information for the assessment of agricultural drought;
15. Section 2.3: future drought projections were used to identify hot spots (Figs. 1e and 1f). Rising GHG were considered (RCP8.5) only. Why not other GHG experiments, but more importantly you could also have used past trends, which likely are more reliable or last can be compared against observations (at least for precipitation);
16. Section 4.1: I believe it is sensible to make a remark that caution is needed to do any future drought projects using GCM simulation given the rather high uncertainties, inconsistencies and low robustness in reconstructing the past (previous sections in this study);
17. The longest common period (i.e. 1979-2009) was only based upon the observation-based data. I wonder why you did not consider the GCM simulations. The historical runs with observed GHG end in 2005. Then model experiments start. Why not 1979-2005 as common period for the whole study? Then you do need to decide on a RCP.

**Minor Points:**

18. “However, high uncertainty should not be equated with low drought risk...”. (13774, lines 26-28). Who is doing this? You expect a reference, but cannot be done in the abstract. Risk = probability on the hazard (topic of the paper) x vulnerability / exposure. Uncertainty in the hazard automatically leads to uncertainty in the risk (likely with a larger spread in the risk probabilities);

19. "... in particular for several hot spot regions that are consistently projected to be more strongly affected by drought in future CMIP5 simulations (e.g. the Mediterranean, Central America/Mexico, the Amazon, North-East Brazil and South Africa)." (13775, lines 12-14). You expect a reference at the end of this sentence, or is it "(IPCC, 2012; Seneviratne et al.,2012)"?
20. The Introduction is too general about drought (13775, lines 1-27)). I believe that you have to state clearly that the paper is limited to meteorological and soil moisture droughts and that you do not cover hydrological drought (see Stahl et al., 2010; 2012), which could have derived from the runoff simulated by the GCMs. See point 6.;
21. "In this study we address past and projected future changes in droughts from a variety of perspectives." (13775, lines 23-24). Should be ".. changes in meteorological and soil moisture droughts". Do not confuse the readers;
22. Section 2.1.1 CMIP5. Not all HESS readers are familiar with "RCP" (Representative Concentration Pathways), the resolution (Table 1), or T42 grid (13779, line 12). I suggest to add between brackets (approximate degrees, e.g. $2.8^0$, $1.1^0$);
23. In Table 1 (reference to it, 13776, lines 21-22) acronyms occur that are not explained yet (e.g. WSDI). CDD (Consecutive Dry Days is mentioned there) – not mentioned in the text. Relict of earlier draft?
24. Elaborate why it is permitted to intercompare trends in SPI12 (Fig. 2) with zonal mean precipitation anomalies (other indicator) over different periods (13780, lines 26-29, 13781, lines 1-3). See point 6;
25. "Using that range as an estimate of internal climate variability (which seems reasonable, since ...." (13782, lines 2-3). I believe it more appropriate to derive the internal climate variability from the SPI12 range using the observation-based datasets (each hot spot area consists of many grids that can be used for the daily temporal distribution) rather than from models that have limitations;
26. "The few spots of systematic SMA decreases in Fig. 2 are consistent with the drying regions identified in Fig. 6". Is it also the other way around?
27. "Only for the Mediterranean the GCM simulations seem to indicate increased drought,..." (13782, lines 9-10) add "Only for the Mediterranean the GCM simulations seem to indicate increased drought <u>frequency</u>,..." and "...and SAF panels in Fig. 6 for increasing drought)" (13782, line 15) add "...and SAF panels in Fig. 6 for increasing drought <u>occurrence</u>)". Only " drought" is not precise (see point 6). Check other general phrasing in the paper;
28. "....as Fig. 8b, d shows by the number of months per year in which 25 the SPI12 and SMA drop below −1,..." (13783, lines 24-26). Motivate why you swap from -0.5 (Figs. 4-6) to -1.0. Is there a reference; like the mild drought characterized by -0.5 (Lloyd-Hughes and Saunders, 2002)?
29. Conclusions: "However, our analysis of the 12-month Standardised Precipitation Index (SPI12) indicates that the recent droughts are not exceptional in a climatological sense but are consistent with the range of internal climate variability estimated from the CMIP5 ensemble of GCM simulations" (13786, lines 9-13). Add "...indicates that the recent <u>meteorological and soil moisture</u> droughts are not exceptional...". Only " drought" is not precise (see point 6);
30. Conclusions: "Large internal variability and general uncertainty is also found..." (13786, line 25). Be a bit more precise on "general uncertainty" . Is it uncertainties due to GCM formulation and GHG concentration scenarios?

31. Conclusions: “Extreme drying scenarios are therefore about as likely as significantly reduced drought risk”. These cannot be compared directly (see point 18);
32. Acknowledgements: “We acknowledge partial funding from the EU 7th framework program through the DROUGHT-RSPI and EMBRACE projects” (13788, lines 15-16). Add grant number (mandatory, see annexes to EU Grant Agreement);
33. Fig. 2 (13795). Add legend/units under horizontal bar (is it % change?). What is the meaning of the colour of the symbols (o, + and -) in Fig. 2c?
34. Fig. 3 (13796): “.....projections for the three GHG concentrations scenarios RCP2.6, RCP4.5 and RCP8.5 are combined”. How, simply averaged?
35. Figs. 4 and 6: (13797 13799): the period 1950-1960 is left out. Need to mention that you used a 10-yr forward moving average (Section 3.2);
36. Fig. 7: “Wetting regions” (13800), add “Wetting regions in the 21st century” (makes numbers below x-axis better understandable). “Standardised Precipitation Indices (SPI12) change in “SPI12” You do not need to spell out, not done in previous graphs. “...in regions where SPI12 increases.”. Median / average SPI12?
37. Fig. 8: see point 36;
38. Fig. 9 (13802): Add y-axis with legend (“fractional uncertainty”?). “...from three different GHG concentrations scenarios (colours blue, green, red)”. Add RCPs, like in Figs. 7 and 8.

**References:**


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