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Hydrology and Earth System Sciences Discussions

Interactive comment on "Elusive drought: uncertainty in observed trends and short- and long-term CMIP5 projections" *by* B. Orlowsky and S. I. Seneviratne

J. Hannaford (Referee)

jaha@ceh.ac.uk

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This is an interesting, generally well-written and highly topical article which manages to cram a huge amount of different analyses into a relatively short space. Drought hotspots, historical changes, future changes and uncertainty analysis – it's all here! And all carried out at a global scale, primarily using CMIP5 model runs with some observational precipitation data.

In fact, there is so much here that it at times I felt like I was reading an amalgamation of several different papers. Each of these topics has indeed taken up whole papers in

their own right. On reading the paper I felt at times that I was losing the narrative thread – the big idea bringing this all together. My main overarching criticism would be that the paper tries to do too much, and as a result some of the sections are passed over rather quickly, without much detail. As a result some of the points made seem rather vague, sweeping statements which not always well supported by the analysis (see specific comments). The paper would benefit from more careful interpretation and more detail in some sections, with more discussion of wider context and previous work.

And also my recommendation would be to remember that narrative thread (neatly summed up in the "elusive drought" title) running through the paper. On revising, the authors could spend more time early in the paper introducing their rationale for covering all these different areas, and also add some more "signposting" to keep the reader on track through the paper.

Overall, this is a worthy addition to the literature on the evidence for historic and future drought changes, and the conclusion that drought change is hard to detect and highly uncertain in the latest projections is an important addition to ongoing debates on drought under anthropogenic climate change – particularly given other recent analyses which have highlighted caution in the assumption of a straightforward, widespread increase in drought severity in warming world.

I recommend publication in HESS following (generally moderate) modifications as follows:

1. Introduction. A bit short – could be more explicit about the rationale of the study. Para 2 (L23 onwards) sketches the outline of the paper but it isn't clear what the justification of doing this is, especially as other studies have covered these areas individually. Why is there a need for this compared to previous work of Dai, Sheffield etc on past and future drought and other workers looking at sources of uncertainty (Burke & Brown, 2008).

13774, L15. Should "until" the end of the 21st Century be before?

L17. “Unsignificant” - should be insignificant but non-significant would be more appropriate in the statistical sense.

13775, L25 (and elsewhere) exposition should be exposure. L27 – is the abbreviation w.r.t. acceptable for HESS house style?

13776 L19. I am not sure what r1i1p1 means and this may confuse readers – could this be explained briefly?

Sect 2.2.1. This paper uses multiple indicators which is good to see. The paper uses SPI12 – this is a fairly long averaging long period and some of the areas are likely sensitive to shorter duration, intense droughts (e.g. SPI3, 6). The authors would likely get different results for shorter durations, which may be more important in many regions. This could have a bearing on the conclusion of “no increase in drought”, as short intense droughts may have increased and SPI12 wouldn't capture that. This is an important point (one of the key benefits of the SPI is the different averaging periods) so is worthy of some comment here and during the interpretation later.

L13778, Sect. 2.3. The selection of hot-spots is not consistent – it seems to be rather subjective and based on different approaches (a priori definition based on exposure, plus potential future change – the latter is what this paper later studies, so this is rather circular). The logic which means the Amazon is incorporated (based on future change) would imply southern parts of South America should be included? What about Middle East, e.g. w of Caspian sea? Perhaps worth a line or two of commentary to highlight these other areas not included in the current analysis (which projections suggest may face big changes).

L13779, L4 – would probably be safer to omit “basically India” as this region incorporates a number of other very large, very populous countries which probably don't sit well with being labelled as basically India.

L13780, L13. This interpretation of significance is indeed not that stringent, but given

the endless debates in the literature about significance in the context of serial, cross-correlation and long-term persistence, this is a tricky area. On one level I agree it is not overly important given the current purpose, but then also I imagine the autocorrelation of annual drought statistics could be extremely high given the likely prevalence of multi-year drought events – and autocorrelation tends to increase the probability of detecting a trend when none is present. Worth some commentary here on the justification for the adopted approach, alongside references to the trend literature.

Sect. 3.1. (and similar 3.2). Given the current interest in the debate on whether global drought is increasing (this section already references both Dai, Sheffield 2012 papers) this section would perhaps warrant another short paragraph discussing how the results compare with other regional- to global-scale studies in more detail. E.g. comparison with work which finds significant (and attributable?) drying in the Med (e.g. Hoerling et al. 2012, Stahl et al. 2012).

13781, L25. Not clear that there are “no trends in this period” – depends what is meant by no trends. Clearly there are no compelling strong trends and the variability on inter-annual to interdecadal scales is more important. But it looks like there is a downward trend in the MED post-1980. Upwards trends in observations in CNA, decrease in EAS, SHE, etc. I agree with the conclusion that there is little evidence of the global increases reported elsewhere (Dai et al.), but to say there are “no trends” is rather sweeping; worth adding a paragraph which explains the patterns in these plots in a bit more detail.

13782, L5. To say the observed droughts are not exceptional is again rather sweeping. They might rarely be out of the bounds of the CMIP5 ensemble, but that places a lot of emphasis on how well that ensemble represents natural variability. Observed major droughts (by definition) were exceptional as they were in the extreme ranges of historic variability – and that is what is traditionally used in many practical applications, e.g. water supply systems, and much societal planning. It may be safer to just explicitly stick to saying these events were within the range of GCM variability rather to say they weren't exceptional.

Plus – this section states there is no GHG forcing in the GCM ensemble – I don't follow this, from section 2.1.1. I thought it was with historical forcing (and thus both GHG and variability)? Please clarify.

L13782, L10 – not sure it is only the MED showing an increase – what about NEB? Check the accuracy of this and other statements in this section.

13782, L18 – (and on 13783, L16). Theoretically, the increase in soil moisture drought given limited precip change could be runoff or evapotranspiration. But if the GCMs and observations suggest no (or limited) precip decrease, it is unlikely that there has been a major increase in GCM simulated runoff unless there have been changes in land use, stomatal closure etc – all possible, but are these modelled in the GCMs? Plus the evidence for any global increase in runoff from observations is rather equivocal to say the least, and it is certainly dubious to imply there have been runoff increases in the areas where precip/soil moisture has decreased (again, published evidence suggests runoff has decreased in hotspots like the MED, Stahl et al. 2012,). The soil moisture increases surely reflect increased evapotranspiration, primarily as a result of increased temperatures, in the GCMs? Worth adding some discussion on this.

13782, L29. Fair enough that the soil moisture data wasn't used, given the lack of published studies. However, in my view it is critical to re-emphasise, here and elsewhere in the paper, that the soil moisture trends presented are solely modelled, and that places a real constraint on how reliable these findings are. The lack of observations (not in this study, just generally) is a real obstacle to our knowledge of real changes in soil moisture (especially given recent debates about weakness of PDSI as a proxy).

Sect 4.1 I think this is a good example of where this paper just doesn't go into enough detail, as this is a very short section given its aspiration to look at vital results, the future changes. Should briefly compare with other work: e.g. agreement in increases in the same regions, MED, SAF, CAM with Taylor et al. 2012; what about other papers cited

in SREX?

Sect 4.2. This is an interesting analysis to apply to the data, which really helps put the previous sections in context and show just how elusive drought can be. The final summing up (p.13786, L2 onwards) is nicely put. But otherwise I am missing a commentary on the wider significance of these findings. The spirit of the Hawkins and Sutton paper is very much in trying to suggest ways forward in narrowing uncertainty. Without repeating that, are there lessons from this new work, for drought science specifically, going forward?

This section should consider the work of Burke and Brown (2008) and Taylor et al. 2012 which has also addressed this question using a different methodology. Note also that Taylor et al. find the indicator to be the greatest source of uncertainty (they compare SPI, PDSI, SMA) when doing future projections. This is worth commenting on, although see the review comments on the HESS-D paper by the two reviewers. I am inclined to agree that the indicator isn't really a source of uncertainty per se but the authors could still perhaps comment on the limitation imposed by their chosen two indicators and how others may lead to a different picture. The authors could also comment on the other findings of these papers which had a similar aim re: partitioning uncertainty.

Surely the reason GCM uncertainty isn't important for the heatwave is they all predict temperature increases with good agreement, but that drought response differs hugely across the GCM formulations because of their various different process formulations. This seems fairly trivial but if it is included the reasons for these differences should be made absolutely clear.

Conclusions

The conclusion questioning value of SPI is rather weak and nothing new – justifiably, numerous studies over many decades have cautioned against using rainfall alone rather than evapotranspiration. But the authors must add commentary on the alternatives, which also have major issues – the limitations of PDSI are clear (Sheffield et al. 2012 and predecessors), and what do we really know about soil moisture given aforementioned lack of observations? With soil moisture we rely on models and as this study shows, there is a wide range in their simulations of soil moisture. And this section again suggests the role of runoff/evapotranspiration but the paper has not addressed hydrological drought at all.

There is a danger that the overall conclusion is that we just don't know enough about drought and detectability is low – so what is the implication, apart from admitting our lack of knowledge? The last few paras could be viewed as negative although does conclude by highlighting that changes in drought risk can be important even though uncertainty is high. This all hinges on the issue of detectability in the face of high variability. Numerous studies have addressed the question of statistical detectability, i.e. whether changes are detectable before a certain time (Wilby 2006, Hawkins & Sutton, 2012). But arguably the important thing is whether changes cross certain practical thresholds (practical vs. statistical significance, see Wilby 2006). Rather than "will changes be outside wide range of GCM variability" the question becomes "what is the likelihood of droughts of a given severity occurring under climate change?" Much is made here of the fact that recent events are within variability, but in reality that doesn't stop us using these and other historical events as a source of information for planning.

Given that we don't know how climate change will affect drought, due to how elusive it is shown to be here, what are the implications for researchers going forward, and policymakers? I appreciate it is hard to be specific in a global scale paper, and this is getting beyond scope, but any general thoughts the authors have on this might make a good ending.

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