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Hydrology and Earth System Sciences Discussions

Interactive comment on "On the spatio-temporal analysis of hydrological droughts from global hydrological models" *by* G. A. Corzo Perez et al.

Anonymous Referee #3

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General

This paper develops new methodologies for analysing the spatio-temporal characteristics of droughts. The paper is very ambitious in attempting to develop such methods for application on a global scale and, technically, this is an impressive project, involving some demanding computational work in order to analyse such a large dataset. It also has timeliness and novelty; Global Hydrological Models have rarely been tested for their ability to simulate runoff extremes, and global-scale analyses of drought have generally focused on other hydrological parameters such as soil moisture.

I feel that the paper has potential for publication in HESS, but at present there are a number of problems in interpretation, particularly in relation to the results of the global analyses. The authors attempt to "validate" the model outputs, but I feel this is done in

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rather a cursory way, and is let down by a lack of consistency and rigour. There is also some questionable treatment of the results from the CDA and NCDA outputs at a global scale; there are some patterns which do make sense, and demand interpretation and explanation, but this is not really offered. It is also not that clear what the key benefits of the CDA approach are compared to the abundance of existing approaches such as Severity-Area-Duration methods, so the authors should put their method in context in their discussion and conclusion.

Overall, this paper has some novel approaches to offer, but has some major scientific weaknesses which need resolution - the authors have come up with a methodology, but I do not feel they have yet proved its worth with convincing results. That said, the paper is potentially an important contribution and is worthy of HESS, so I recommend publication subject to significant revisions being made. In particular, the authors must add more clarity to the methodology and justification for some decisions. They must also attempt to explain certain features of the results which seem questionable at present; especially the remarkable synchronicity in the global extent of drought between all years, and the unusual temporal patterns which appear anomalous. These must be addressed before the paper could be published, as such patterns could also point to problems in the analytical methods, which could undermine the results.

Specific Comments

621, L2. “nil” rainfall would sound better

621, L12. Should be “dollars” not “dollar” or use the symbol.

621, L23. Should not abbreviate “incl.” – this should be including, and check elsewehere in paper. Also should be “. . .drought converts into hydrological. . ..”. Convert should be converts.

622, L1. Should be “interaction of atmosphere-land. . .”

622, L20. This para states that methodologies have to be developed to inter-compare

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global drought amongst models. But why? The authors might want to expand on this, as it seems a key aim, but it is rather lost here; a lot of effort goes into this sort of analyses, but what is its impact? Is the overall aim of this paper to come up with a method for intercomparison (like WaterMIP)? If the models are to be compared, what is the groundtruth which they will be compared against (this is tricky for space-time drought evolution, but the authors should comment!)? The authors may also wish to reflect on what the other potential benefits are of this work (i.e. improve our understanding of how hydrological droughts evolve at large scales; examine synchronicity, etc)

622, L24. "…the drought propagation". The "the" is superfluous.

623, L18. Would be better to say "research studies" rather than pluralising research.

624,L5 "A region can be built up….." This whole sentence does not really make sense and I am not clear what the authors mean here by "visual inspection of the overall information of a pre-defined region".

624, L19. Should be "globe as a whole".

624, l25. "…helps with the overall representation of the overall drought region". What region? This is ambiguous – please rephrase. Perhaps "overall areal extent of drought" or similar.

625. The methodology could be clearer, and in places not justified well. This is all fairly pivotal, as it will affect the number and duration of events, and therefore all the results – I wonder whether issues with the threshold may be behind some of the unusual patterns in Fig 7 which are discussed later.

The authors use the term 80th percentile, but this is surely not the case? This would be high flows. I assume they mean the % exceedance, i.e. the runoff value which is exceeded 80% of the time (or Q80 to use the common hydrological nomenclature). This needs to be clarified for consistency with other studies.

The authors should give more justification for their decisions here. Why is the Q80 used as opposed to, say Q90? Were others considered? Is this really a “drought” – in many areas this would not be especially severe. Of course it is still useful to highlight low runoff, but the authors should point out why this was chosen and whether alternatives were considered.

More importantly, the authors need to explain their decision re: ephemeral runoff. It would seem odd to only exclude those cases where runoff >80% of the time is zero. This would still mean that many cells will have zero runoff for up to 79% of the time, which means the cells are still zero-dominated and Q80 from these cells would be zero, which would influence results.

Why is the minimum 3-day independence criteria applied? This still seems like a very short drought event, why not longer as most droughts are on >monthly timescales?

626, 7. Would be better written as “. . .daily deficit (X – T), as visualised in Fig.3”.

626. L14. Why standardise by the standard deviation, as this brings variability into the question – which could have an impact?

628, L7. Maybe rephrase this section as it is a bit Europe-centric (when the paper is focused on global drougtht), to firstly say where the KG type applies (mainly Europe, eastern USA, Argentina) and then mention the fact it can provide a general climate reference for Europe.

628, L23. Should be “another clustering algorithm” or “other clustering algorithms” (or, better, a “different clustering algorithm”. And should be Severity-Area-Duration curves.

629, L18. Could the authors comment on why this threshold was chosen, as just 2 cells would seem to be quite small for a predominantly global analysis, and will result in many small-area droughts. Andreadis et al (2005) used 25,000 for the USA analysis and Sheffield et al. (2009) used 500,000 km2 for their global analysis. The authors should comment on this decision in the context of previous work.

630, L10. Should be “extent” of lakes.

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630, L15. Why do the authors use sub-surface runoff? There should be some discussion as to why this decision was made. As droughts are slowly evolving, it would seem defensible to focus on the slower-flow component. But on a global scale, might this introduce biases, as some regions will have a greater sub-surface component than others? Could the authors perhaps comment on previous validation studies of WATERGAP, which may help convince the reader as to the utility of its runoff outputs for representing surface hydrology.

631, L10. Simplify this figure, e.g. 146.7 million km2

631, L17. The spatial variation in total number of events is difficult to see with these scales of Fig 6a. It is mostly low (<50) and then three or four times this in a few areas (Amazonia, W. Africa, SE Asia). The authors do not offer any explanation of this. The Total number of events seems correlated with total runoff from WaterGAP (see Doll et al. 2003, Fig 5). This is worth commenting on; total number of droughts highest where runoff is highest – presumably because runoff variability is high and the threshold is crossed more frequently?

Fig 6b is completely dominated by the high durations in N. Africa, so it is impossible to see the variation elsewhere. And to what extent can these parts of N. Africa really be said to have “prolonged duration of low flows” (or any flows) when runoff is <10 mm Yr (see Doll 2003). Surely this is not “droughts” being captured – even given the relative threshold method used in this study – this is just an area of very low runoff and low water availability. Comment on this is essential as this could all affect the global results.

Fig 6c is referred to as showing “mixed” spotted pattern, but it does show some regionality (Sahel, Midwest North America, etc). The authors should comment on this.

Fig 6d - the text says 1963, the map 1976.

L23 – l27 just seems to state the obvious fact that an individual timeslice is different from the mean values, and is superfluous.

Much more could be made of this section. The authors need to rethink these maps and this whole section – they say there is no relation between NC and ADDC, but this is probably because one cannot see the variability as it is dominated by north Africa (ADD) and the high runoff areas (NC). The authors should consider what this means for a global analysis – from these maps I would think any consideration of the total Area in Drought globally (the basis of the rest of this work) will be heavily affected by this variability.

632, and Fig 7. The authors do not really comment on WHY the pattern of % area in drought is so similar between all years, other than a reference to the fact there is no variation from day 230 to day 360 (L20). But surely these results are important – this synchronicity between years is astonishing and I cannot see why this would occur, given that droughts occur at various times through the year. If this is a real result, that global droughts show this synchronicity between years, it is of great interest and importance. However: why, in all years, does the % go up between day 50 and day 150? Moreover, why are there consistent and pronounced peaks and troughs at exactly the same time during this period, in all years? There are three very obvious bands of high area in drought in Fig 7b. What mechanism is responsible for this on a global scale? And why the comparative stability later in the year, which is commented on, but no real suggestion is given. These results demand explanation and the authors must comment on these features and attempt to explain them in physically plausible terms, as it could easily be that these point to some error or artefact of the analysis and, if so, this may undermine the results. The curves in Fig 8 for the two KG climate regions look realistic, with variations through years & between years. This suggests to me that the global curves presented in Fig 7 are dominated by another part of the globe, perhaps one where there is a problem with the thresholds which causes these peaks in the spring.

633, L18. This should presumably be 1976 not 1967.

633, L21. Why just the number of clusters? Surely the size is also important? The number of clusters will depend heavily on the size, and the same overall number could be made up of lots of very small events or lots of large ones, the latter of which would clearly be more important.

Figure 9 (CDA) is purported to show similar patterns to the NCDA. It is true that many of the notable years are picked up in both, but there are also profound differences. The patterns throughout the year in the NCDA are completely different – can the authors comment on the lack of congruency between the overall area in drought and the number of drought clusters on the sub-annual timescale? I think the sub-annual variations in Fig 9b look more plausible than those in Fig 7.

634, L5. I am not really sure this helps sell the benefit of the CDA – the patterns are similar to the timeslice in Fig 6d. Similarly, 634, L10 onwards. What real extra benefit is there in knowing whether droughts are contiguous, when dealing with a global scale? I can see the benefits when comparing with the Koppen-Geiger classification with fixed boundaries, but I don't think the CDA adds much to the NCDA for this global scale analysis. There doesn't seem to be a consideration of both area and duration together, except Fig 10b (see below), unlike in Severity-Area-Duration methodologies.

634, L11. "in terms of it" Should be "in terms of its"

634, L11 and Fig 10a. As with Fig 7, why is there such a strong seasonal pattern in the results, with the bands occurring in spring? Presumably the mechanism is the same, but in this plot the dichotomy between high values in the spring and low variation through the year is even more apparent than in Fig 7.

634, L16 and Fig 10b. This graph is very interesting, but why does the Fig 10b not go up to year 2000? The text refers to 1999 for Asia being over 300 days but it is not on the graph. The graph must be corrected to be consistent with the text.

634, L22. 1976 is highlighted as exceptional for Europe, but it is less than half of that

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for 1990, so perhaps that should be noted (see comment on discussion)

634, L25. Should be “A unique”.

635. I am not sure that this really constitutes a “validation” as it is done selectively rather than systematically. As the comparison is rather weak, the authors should stick to general points, and should probably tone down their conclusions that the outputs show agreement with historical events – it would be fairer to say that there are some parallels, but that more work is required to provide confidence in the outputs through comparison with existing data/knowledge.

635, L9 – 14. The association of the ENSO related droughts of 1997 – 1998 with the results is reasonably convincing for tropical rainforest Af climates (Fig 8b). But the authors could make more of this – the two references they cite are rather localised and obscure for a well-known event. They could do better by making more detailed comparisons of the event from their method with other studies. L14 suggests the 97/98 ENSO droughts can also be seen in the CDA, but this isn’t clear; I don’t think the period shows up particularly well in Fig 10a, and it is not shown in Fig 10b (see above). The authors don’t mention that 1992 (major drought) was also a major ENSO year. More detail on regional ENSO patterns in these years would be useful.

635, L15. The authors claim the 1976 drought matches previous studies. I think this is true for the NCDA, where Fig 6b looks pretty convincing in showing the evolution of the 1976 drought as previously documented. But for the CDA, it isn’t clear. The authors note the MSDE is not as high as would be expected for 1976, and I would agree, as 1976 occurred throughout the summer and in Fig 10a only occurs in spring (although this Fig is global rather than just Europe anyway). More of an issue is the fact that the length of the maximum spatial extent in Europe for 1976 is much lower than 1990. Previous work would suggest 1990 was not as spatially coherent as 1976, and was in fact an amalgalm of shorter, less severe drought phases (Zaidman et al. 2001), so this may undermine the claim that the "European" drought characteristics matches

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previous work.

The authors highlight 1991/1992 was a drought over Iberia, but it is not clear why they single out these years – this was not one of the major years in the CFb climate (Fig 8b) nor from the CDA (Fig 10d). The authors could be better highlighting 1989 and 1990 which stand out in these figures and were well known drought years in Europe (Zaidman et al. 2001; Stahl, 2001).

The authors should also comment on the difficulty of really making comparisons with work from Europe, when this Climate type also encompasses most of Argentina, eastern USA etc.

635, L24 onwards. This seems rather a piecemeal selection of well-known droughts from the USA and Africa. But I don't think the authors have convincingly shown links between their results and well-known events, and this all seems rather hasty and careless (1981 is mentioned as a year of low crop yields, but 1984 – 1985 is more associated with drought and famine in East Africa, and does show up nicely in the results of this study). These global comparisons could all be strengthened with some more care and detail and reference to the wide literature on the Sahel droughts and those from previous global analysis (there e.g the Sheffield et al papers).

Conclusions

I would suggest that the conclusion should be rewritten in light of the additional improvements required given the comments above, but some specific comments at present include:

636, L. 27. The claim of a positive correlation between CDA and NCDA seems a bit strong given the lack of congruency between the results. The authors should discuss this in more detail.

637, L1. This mentions a 1993 drought for Europe, which hasn't been discussed (see previous section).

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637, L7. As discussed above, this conclusion that droughts occur in April – May appears highly questionable, and has not been explained, so I do not think this conclusion is justified.

637, L10. I don't really feel that the maximum spatial extent for 1976 does really agree well (see above comments).

637, L15. The plural of index should be "indices".

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 619, 2011.