Hydrol. Earth Syst. Sci. Discuss., 8, C1523–C1537, 2011
www.hydrol-earth-syst-sci-discuss.net/8/C1523/2011/

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.

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Received and published: 17 May 2011

We thank the reviewer for his critical comments on our work, which we will certainly take into account in our revised version. In the following reply we shall express our opinion on the reviewer′s comments.

1 - 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 rather a cursory way, and is let down by a lack of consistency and rigour.

The validation of the model is not the objective of this paper as it is stated in the sections Introduction and Methodology. We believe that the term "validation′′ used the section Discussion might have misled the reviewer and therefore a correction will be added. This paper presents the work on the development of a methodology to identify droughts in gridded time series from large scale models rather than to test large-scale models in their ability to simulate drought as mentioned by the reviewer. It is not our intention to make a formal and comprehensive validation of the WaterGAP model in its ability to simulate drought. In a next phase of the project the methodologies presented in this paper will be used to intercompare a number of large-scale models among each other and against evidence from other sources.

This paper aims at further development of methodologies describing the spatio-temporal development of hydrological drought (i.e. subsurface-runoff) on a global scale (non-contiguous and contiguous approaches). The methodologies are illustrated with the outcome from the global hydrological model WaterGAP (Water – Global Analysis and Prognosis, Alcamo et al., 2003) which has been widely tested in different researches (Döll and Lehner, 2002; Döll et al., 2003).

2 - 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.

We agree with the reviewer that it is important to state a reference to other related methods and what the key differences are. Therefore, a paragraph on the difference between the SAD curves and the CDA presented in this study will be included in the corrected version. The comments on the patterns will be addressed later in this reply (see 632 and Fig. 7, pg. 6).

3 - 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.

We believe that the proposed NCDA and CDA methods applied at a global scale to identify drought in runoff are novel in itself. As said before (item 1, pg. 1), the focus of the paper is not to prove that the results (i.e. the droughts) are convincing, the paper claims that the methodologies are convincing to be applied at global scale and they deveal important information that help on the visualization and understanding of spatial and temporal variations of droughts. However, in a next study we will explore its use in a a intercomparison of global models. We are thankful for the ideas and comments made by the reviewer and we will add some information on the global pattern time series graph (Figs. 7b and 10a) as well as on discussion of the time series results. In this sense we will add 3 regions more that represent a larger area of the globe and which will show the combined effect of different climate regions that causes the outcome from the methodology to show synchronicity between years. .

Please find bellow the answers to the specific comments:

Specific Comments

621, L2. "nil'' rainfall would sound better
Will be updated in corrected version.

621, L12. Should be "dollars'' not "dollar'' or use the symbol.
Will be updated in corrected version.

621, L23. Should not abbreviate "incl.'' – this should be including, and check elsewhere in paper. Also should be ": : :drought converts into hydrological: : :.''. Convert should be converts.
Will be updated in corrected version.

622, L1. Should be "interaction of atmosphere-land: : :''
Will be updated in corrected version.

622, L20. This paragraph states that methodologies have to be developed to inter-compare 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 ground-truth 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)

The reviewer identifies in an excellent way the whole research programme. First, methodologies need to be further developed (this paper) that describe space-time development of large-scale drought. The methodology presented helps to describe how simulated droughts evolve in time in a bounded or unbounded region. If the models capture the main spatio-temporal characteristics of large-scale droughts then these:

(i) can help to better understand drought generation as a response to climate drivers and river basin structures, incl. possible world-wide synchronicity, and (ii) can be used to assess the impact of global change on drought (21st C drought). In the revised manuscript we will put this study in the context of the whole research programme (Introduction) and will come back to it in the Discussion and Conclusion.

622, L24. ": : :the drought propagation''. The "the'' is superïňĆuous.
Will be updated in corrected version.

623, L18. Would be better to say "research studies'' rather than pluralising research.
Will be updated in corrected version.

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''.
The sentence states that visual inspection can be used to identify regions. Will be updated in corrected version.

624, L19. Should be "globe as a whole''.
Will be updated in corrected version.

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.
Will be updated in corrected version.

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.
Will be updated in corrected version.

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 time-scales?
Although it is possible to think on more fuzzy bounds in the decision boundary of the dryness of the cell, for the sake of simplicity in this approach a sharp boundary was selected. In this sense the reviewer is correct, we should use Q80 meaning the subsurface runoff value, which is exceeded 80% of the time. We will check the paper and make it consistent. The Q80 has been used in many drought studies. We motivate the choice of the Q80 on page 625, L 13-20. A drought duration of short value (3 days) was selected as the 10% of the month, on a daily scale. We will add comment to make this more clear.

626, 7. Would be better written as ": : :daily deïňĄcit (X – T), as visualised in Fig.3''.
Will be updated in corrected version.

626. L14. Why standardise by the standard deviation, as this brings variability into the question – which could have an impact?
Will be updated in corrected version.

628, L7. Maybe rephrase this section as it is a bit Europe-centric (when the paper is focused on global drought), to ïňĄrstly 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.

We will rephrase this sentence. The selected KGs are certainly not Europe-centric as mentioned by the reviewer.

628, L23. Should be "another clustering algorithm'' or "other clustering algorithms'' (or,better, a "different clustering algorithm''. And should be Severity-Area-Duration curves.
Will be updated in corrected version.

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 ShefïňĄeld et al. (2009) used 500,000 km2 for their global analysis. The authors should comment on this decision in the context of previous work.
The use of a low spatial threshold of two cells will help identify different small patterns in regions where it might not be common to have a drought. This measure was useful for the KG regions where it is possible to have smaller areas. A comment will be added in corrected version.

630, L10. Should be "extent'' of lakes.
Will be updated in corrected version.

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-ïňĆow 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.

A comment on the subsurface runoff will be added.

631, L10. Simplify this ïňĄgure, e.g. 146.7 million km2
Will be updated in corrected version.

631, L17. The spatial variation in total number of events is difïňĄcult 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?
A comment on this will be added to the corrected version. The scales of the colour coded figure will be verified and some discussion on the relation to the WaterGAP total runoff will be added.

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 ïňĆows'' (or any ïňĆows) 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. The dry regions will be denoted in the corrected version as well as including a logarithmic scale on the colour bar, which will also help to show up other more wetter regions. 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.
Will be updated in corrected version.

Fig 6d - the text says 1963, the map 1976. L23 – l27 just seems to state the obvious fact that an individual time-slice is different from the mean values, and is superfluous.
Will be updated in corrected version.

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.

As it is mentioned before we will change the scale of the color coded map to address the comment. Clearly, there is through the drought definition a negative correlation between Nd and ADDc. An update will be added to the corresponding lines.

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 roughs 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.

This comment has already be touched upon answer 1 on the top of this reply. An analysis of the PDA from several KG regions (not in the paper) showed that KG regions with a clear seasonality (high flow variability in the transition period from one season to the others).likely cause this phenomenon. For example, the Dfb climate has a high PDA around day 90 when the cold season with snow enters the warm summer. The Dfc climate has two PDA peaks around days 90 and 120, which reflect the transition from

the cold winter with snow into the cool summer. The Aw climate has a less pronounced higher PDA around day 150, which represents the change from the dry to the wet season. On the contrary some climates have no clear PDA peaks (e.g Af and Cfb). The analysis of five selected KG regions show that the composite (weighted) PDA shows PDA peaks in the period between day 50 and 150, which confirms the vertical patterns in Figs. 7 and 10. A graphical representation of the dominant regions that cause the pattern will be added as well as a paragraph that discusses the seasonality found in the different KG regions.

633, L18. This should presumably be 1976 not 1967.
Will be updated in corrected version.

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.
Number of clusters is a first measure to use with the CDA, and for us recommended since it will help on the identification of the distributed events of a certain size. So the process done here for the Max size of cluster can be also used to explore ranges of events. However, it is not the purpose of the paper to thoroughly explore all possible regions of the solution space. Aside of this it is important to highlight that the duration of a spatial event depends on physical processes also operating in neighbouring regions. In general partial events are not explicitly linked and therefore temporal links between the geolocation and merging of spatial drought events requires to be analysed. This tracking technique is not part of this paper and will be presented later in the context of a following pattern tracking study.

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.
The number of clusters present in the map can be represented by small regions that in fact do not have really impact the overall analysis of the NCDA. Therefore both figures do represent different aspects of drought events (i.e. Areas and number of spatial events).

634, L5. I am not really sure this helps sell the benefit of the CDA – the patterns are similar to the time-slice 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 not think the CDA adds much to the NCDA for this global scale analysis. There does not seem to be a consideration of both area and duration together, except Fig 10b (see below), unlike in Severity-Area-Duration methodologies.
The CDA is a method that at global scale allows for an explicit analysis of the areas of drought events and locations (as done in Fig. 10). This is possible by making ranges of spatial drought events and making statistics of the locations. In this paper we only looked at one example, i.e. analysis of the maximum but as well it can be done for other ranges of drought areas. The benefits of these relate to the fact that similar patterns of extreme (maximum) drought can be identified in time and their synchronicity in time can be studied. The analysis of the different time series of different ranges can help to investigate the temporal evolution of synchronicity along different regions in the world.

634, L11. "in terms of it'' Should be "in terms of its''
Will be updated in corrected version.

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.
Answered on comment above for 632, and Fig 7 (see pg. 6).

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.
Will be updated in corrected version.

634, L22. 1976 is highlighted as exceptional for Europe, but it is less than half of that for 1990, so perhaps that should be noted (see comment on discussion)
Will be updated in corrected version.

634, L25. Should be "A unique''.
Will be updated in corrected version

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.
The word validation is misleading in the context and will be removed, as discussed on the item 1 we will update the text (see item 1, pg. 1).

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.
An extended comment on the ENSO 1992 and an update of the paragraph will be included in the corrected version, although the paper will avoid the impression that is

meant to be a formal validation.

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 previous work.

The scale of figure Fig. 10a does not represent a time line (i.e temporal evolution) and only the number of days the largest cluster was present in that region. Therefore the summer or winter period are not discussed. The fact that the size of the largest cluster remained in Europe is a measure of the event. A comment on this will be added to the text.

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.

An update on the paragraph will be done and the references will be added, although the paper will avoid the impression that is meant to be a formal validation (See answer item 1, pg. 1).

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).

An update on the paragraph will be done and the references will be added, although the paper will avoid the impression that is meant to be a formal validation.

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:

An update on the Conclusions will be done that considers the valuable comments of the reviewers and the associated improvements of the revised manuscript.

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.

An updated version of the different reasons why the correlation of CDA and NCDA are mentioned.

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

Will be updated in corrected version.

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.

This will be more clear with the update of the previous comments

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

This will be more clear with the update of the previous comments

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

Will be updated in corrected version.

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