

# ***Interactive comment on “Unravelling the impacts of precipitation, temperature and land-cover change for extreme drought over the North American High Plains” by Annette Hein et al.***

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Note to the editor and authors: As part of an introductory course to the Master programme Earth & Environment at Wageningen University, students get the assignment to review a scientific paper. Since several years, students have been reviewing papers that are in open online discussion for HESS or BGS, and they have been asked to submit their reports to the discussion in order to help the review process. While these reports are written in the form of official (invited) reviews, they were not requested for by the editor, and we leave it up to the editor and authors to use these reports to their advantage. While several students were often asked to review the same paper, this

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was not done with the aim to provide the authors with much extra work. We hope that these reports will positively contribute to the scientific discussion and to the quality of papers published in HESS. This report/review was supervised by dr. Ryan Teuling (teacher within the ITEE course at Wageningen University and also associated editor with HESS).

Dear editors of Hydrology and Earth System Sciences,

In this paper, Hein et al. use the coupled model ParFlow-CLM to describe which of land use changes, temperature changes, and precipitation changes representative of extreme drought have the largest impact on drought severity in the southern High Plains of the United States. They then describe which feedbacks are present between these variables responsible for drought, and finally they discuss how these feedbacks change with spatial scale within the modeled region. It is novel and interesting because it successfully answers each of its research questions, providing new knowledge with respect to how causes of drought impact each other, as well as demonstrating new and sophisticated drought modeling methods. It seems appropriate to me to gain a better understanding of how drought might change across scale, and that understanding the feedbacks between variables responsible for drought is a meaningful advancement of our understanding of drought. This paper is clearly in the scope of this journal, as it directly studies phenomena in hydrology. The work is generally well-researched, and the writing quality is good. While I would strongly recommend this work for publication, there are some minor revisions that I believe need to be addressed before it is truly of publication quality. This review has two parts: more specific arguments for the work's novelty and quality, followed by revisions that I think would make the paper stronger.

This paper is relevant because it fills a clear knowledge gap in the field of drought models. The authors claim that there are many studies that use models to forecast drought scenarios or compare the accuracy of models to historical data. However, the authors claim that these studies either isolate variables responsible for drought and describe their effect on drought, or they describe or forecast the general impacts of com-

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binations of factors without considering specifically what feedbacks might be present between variables responsible for drought. I agree with this assessment, from reading recently published literature on drought models and from reading articles the authors of the present paper cite. Zhao and Dai (2016) report on the accuracy of various drought models when compared to observed trends in Palmer index, where models have forcings in greenhouse gas concentration changes, precipitation changes, and other variables. However, in this paper, the authors make no attempt to ascribe the impacts of drought to specific forcings, rather describing model general accuracy as a result of a complex combination of forcings. Deo et al (2016) develop a method to forecast drought using a sophisticated wavelet analysis, but they only consider “water accumulation,” which is based on rainfall, rather than a combination of factors, to arrive at this forecasting. Rad et al (2017) develops a new method for drought prediction incorporating both likelihood for drought onset and likelihood for drought persistence, but their prediction is based on the relationship between rainfall and streamflow, which again does not consider the feedbacks between many variables responsible for drought. Su and Dickinson (2017) consider in depth the feedback between soil moisture and precipitation in the 2011 drought in the same area studied by the present paper, but other feedback relationships are left out. By contrast, in this paper, the impacts of drought in ET, runoff, water table level, and soil moisture are compared across runs with individual perturbations and runs with combinations of hot, dry, and crop failure perturbations. In this way, we see how for example a combination of hot and dry conditions affects ET differently than the sum of impacts due to individual hot and dry scenarios, or how the sum of impacts in water table depth from single-perturbation runs with hot, dry, and crop failure conditions differs from the impact in water table depth given by a scenario that considers all three of those perturbations, etc. So, by giving a quantification of the differences (nonlinearity) between single- and multi-perturbation model runs, the authors of the present paper provide new knowledge on the feedbacks between land use changes, precipitation changes, and temperature changes.

Specifically, the paper does this via three research questions, and it clearly provides

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a satisfactory answer to each. The first research question involves quantifying which of land cover change, temperature change, or precipitation change has the biggest individual effect on drought via changes in ET, groundwater depth, runoff, and soil moisture for ranges of perturbations found during extreme droughts. It achieves this by comparing single-perturbation runs to a baseline run, and determines that changes in precipitation have by far the largest affect on drought. This agrees with Luo et al. (2017), who use the variable infiltration capacity model (VIC) to demonstrate that reductions in temperature have a larger impact on agricultural drought than increases in temperature.

While the first research question is already somewhat well-studied, the second and third research questions are not. The second research question quantifies the feedbacks present between factors responsible for drought. In order to do this, it runs the model for scenarios that include multiple perturbations (hot and dry conditions, and hot, dry, and crop failure conditions). Then, it compares the drought impacts from these runs to the sum of drought impacts from single-perturbation runs. The percent difference between these impacts is called the nonlinearity, and describes how the factors responsible for drought affect each other. It goes on to provide a theoretical physical justification for these differences, and finds that the nonlinearity is largest in ET and runoff.

The third research question aims to determine how linearity changes across spatial scales. In order to do this, it aggregates linearities from the pixel level to increasingly large scales, all the way up to the sale of the entire model. The result is that variation in linearity decreases and linearity increases as the spatial scale increases. That is, there is more disagreement between sums of impacts of single-perturbation runs and impacts of multi-perturbation runs at the small scale, whereas at the large scale both approaches yield similar results. The paper then shows how inclusion of small scale detail is still meaningful for large-scale results by demonstrating that removing the small-scale process of lateral flow changes sign and magnitude of ET across a

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large portion of the model area. Thus, all three research questions are thoroughly answered, providing novel quantification of feedbacks between processes responsible for drought at different spatial scales.

This paper also represents the next step in modeling sophistication. Upon reviewing the author references on model choices, I agree that the inclusion of lateral flow is more computationally expensive and yields more detailed results, that VIC does not allow lateral flow and generally has less detailed parameter options (and sees its greatest use in large-scale modeling) (as can be seen in Liang et al. 2003), and SWAT can only have non-groundwater lateral flow in depths of zero to two meters, which might not accurately reflect lateral flow in conditions where there is deeper groundwater (as can be seen in Green et al. 2006). ParFlow is evidently an advanced model in the field of drought modeling, and this study is made even more sophisticated by its fine grid resolution, which allows for a meaningful comparison of model qualities across spatial scales (albeit at computational expense).

This paper is socially relevant in that it could have important implications for drought management policies in the studied region. Specifically, the existence of large variations from regional trends at the pixel (1km) scale is well-established in this paper. Any drought response or preparation plan could take this into account, expecting variability in necessary resources or action on the small scale.

Finally, the writing style is clear and encourages rather than hinders reading.

While this is generally an excellent paper, it could be made tighter in key spots. The following recommendations are in order of decreasing importance.

In order to determine the nonlinearity of a multi-perturbation run, the authors subtract drought impacts (in ET, soil moisture, groundwater depth, and runoff) from the sum of the drought impacts resulting from single-perturbation runs. Though the single- and multi-perturbation runs generally have a duration of three years (the worst case free draining run is the exception), the nonlinearities are reported as single values, sug-

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gesting the authors aggregate the nonlinearities of the three years. I acknowledge that one of the intents of the paper is to determine the feedbacks between factors affecting drought, and this doesn't necessarily involve observing change in linearity across temporal scale. Still, I think it warrants discussion, because 1) certain physical processes in the model have long memories (groundwater, soil moisture) and thus might have varying temporal linearity trends, and 2) various factors' contributions to linearity might be obscured by aggregating over three years. So, if there are no significant temporal trends in linearity, I recommend providing maybe just a paragraph or so detailing this. If there are significant trends, I recommend reporting on them by describing how the linearity changes with each successive year of the model.

This point is simple, but important, and also pertains to linearity. Figure 12 is a key figure for this paper - in it, the authors show the decreasing variability in nonlinearity with increasing scale and the average trend towards linearity with increasing scale. They do not indicate which nonlinearities are reported in this figure. There are three options: nonlinearity between the "worst case" run and the sum of the "hot," "dry," and "crops" runs, nonlinearity between the "hot and dry" and sum of the "hot" and "dry" runs, and nonlinearity between the "worst case" and sum of "hot and dry" and "crops" runs. I'm generally willing to believe the trends in linearity that the authors report, but specifically what we're looking at in figure 12 should be established before any general trends are made. Is this figure the nonlinearity between the "worst case" run and the individual runs, the nonlinearity associated between the "hot and dry" run and the individual runs, or a sum or average of the two? Or, does it incorporate the nonlinearity between the "worst case" and "hot and dry" runs?

While the model is extremely detailed and state-of-the-art, the paper lacks comparison to actual data. I appreciate that the authors provide a physical description of the observed nonlinearity in ET (the hot and dry scenario has a nonlinear ET value because when precipitation decreases, there is less water available for ET even if the temperature rises which would normally mean a higher ET), and that this nonlinearity

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is theoretically supported by McEvoy and Seniviratne. I also appreciate that it might be hard to find “real world data” on exactly what the authors study in this case - after all, you cannot create multiple droughts with exactly the conditions you want, especially not on the studied scale. But finding some physical justification aside from a theoretical validation would make the paper stronger. The authors are already well-poised to do so, in the case of antecedent soil moisture. Finding an area that has data on ET at various soil moistures under hot, dry, and hot and dry conditions, and then comparing these values, could offer further validation to the model.

While the theoretical physical description of nonlinearities is good, it doesn't entirely prove that nonlinearities aren't model artifacts. Perhaps the authors could include a few paragraphs clearly indicating why the observed nonlinearities aren't just due to the nature of the model.

There's some lack of citation. Though the paper uses in-text citations of McEvoy 2016 and Seniviratne 2010, neither of these citations occurs in the references section. So I recommend that the authors include reference information for these two papers and check that all of their references are listed.

There are differing grid scales for PRISM and model data. The ParFlow-CLM model runs at 1km cell size, while PRISM data has a 4km cell size. Because a central point of the paper is how small scale detail affects large scale properties, the paper might be made slightly stronger by addressing how the PRISM forcings are coarser than their model's resolution, and why this matters or doesn't.

The authors indicate that they compare 1920s precipitation and temperature to PRISM reconstructions for each month of 1934, but it is unclear whether they obtain the perturbations from comparing months of 1934 with average months in the 1920s, or if they compare months of 1934 to averages for the entirety of the 1920s. Also, the source for the meteorological data of the 1920s is not indicated.

ParFlow allows for a cell to have 4 of 16 different possible vegetation types present. In

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the paper, they simply indicate that some of the land is covered by “crops.” Perhaps they could include a more detailed parameterization of the crops, as different crops will surely have different impacts on drought (for example, via ET). However, the authors could probably safely ignore this comment, because they do indicate that they’re looking for the presence and degree of linearity across scales and the relative impacts of perturbations, rather than forecasting specific events.

The authors state “combinations of factors become more linear at larger scales,” but it appears as if nonlinearity actually increases specifically in runoff from the “major basin” to “full domain” scales, although variability in linearity does seem to follow the appropriate trend across scales. Though it is just a variation in one drought factor between two similar scales, perhaps it should be briefly explained.

The water year 1984 is used as a baseline. Some brief indication should be given that this is a typical water year, or at least not a year of drought.

General typos Page 3, line 32: you should consider including “water table level” in the text of the first question. Page 4, line 8: change “19390s” to “1930s” Page 4, line 12: include “it” after “because” Page 4, line 13: include “for” after “suited” Page 5, line 14: change units of km to m. Page 7, line 9: delete “and” between “2” and “include” Page 10, line 5: change “focus” to “focuses” Page 11, line 8: change “decrease” to “decreased” Page 15, figure 7: Apparently, the anomalies in water table are not summed for the “sum of hot and dry effects.” Page 20, line 8: change “becomes” to “become” Page 23, line 5: delete a period at the end of the sentence. Page 24, line 32: delete the second comma.

I hope this helps the authors improve their manuscript, and I hope that this paper becomes published.

Sincerely,

Peter La Follette

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2018-485>, 2018.

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