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Interactive comment

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.

Anonymous Referee #1

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Review of Hein et al. Unravelling the impacts of precipitation, temperature and land-cover change for extreme drought over the North American High Plains.

Summary

Hein et al. present a modeling study, which evaluates the impact of (hot and dry) atmospheric forcing anomalies on the land surface/subsurface water balance over the North American High Plains. It studies the nonlinearity and scale dependency of these feedbacks through single and combined perturbations of temperature and precipitation in the atmospheric forcing. In a second step, Hein et al. evaluate the impact of land use on these feedbacks and they try to address the importance of groundwater in alle-

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Discussion paper

viating these feedbacks. The general idea behind this work interesting and novel. Unfortunately, the manuscript shows major flaws in the presentation of results and would benefit from more precise (and, in my opinion, intuitive) research question. Although the proposed research questions may be evaluated as adequately addressed, both, methods and findings are not adequately presented. Yet, the idea behind this work is interesting, and could, more precisely formulated, address a much more relevant topic (see below). The model simulations along with the nonlinearity aspect provide all the "ingredients" required to address this. I hence recommend major revisions. These revisions should address (1) a clear and potentially more relevant message at hand, (2) a better representation of results and (3) some minor corrections and clarifications.

(1) Message and relevance.

The manuscript misses a substantial differentiation between meteorological and hydrological drought. More precisely, it misses the differentiation between (i) the impact of meteorological drought and (ii) the response of hydrological drought to the former. I am referring to meteorological drought as hot and dry atmospheric forcings, and to hydrological drought as e.g. water shortages and anomalously low groundwater levels, soil moisture, ET and runoff. The combination of both aspects, along with a focus on the nonlinearity of feedbacks, would make this manuscript worth publishing. The authors do, however, only address the first aspect, i.e. they evaluate the impact of meteorological drought conditions (hot, dry, hot and dry) on an annually averaged water balance without referring to the land surface/subsurface state. And according to their own introduction, the fact that precipitation deficits are the main driver for (hydrological) drought, is not novel. Yet, the authors do not explicitly show that their model does simulate a hydrological drought and how the forcing perturbations impact this drought. This is why, at the end of the manuscript, the reader is left wondering what the impacts of anomalous dry and/or hot conditions on an existing drought are, and even more simple, if a hot temperature anomaly alone can initiate or aggravate a hydrological drought. I personally like the analyses of the nonlinearity of feedbacks, which should, in my opinion, be the main topic of the manuscript and could help to increase relevance. Specifically, what would be really interesting, is the combination of both drought aspects along with the nonlinearity analysis. This would intuitively lead to interesting questions, such as: Does the nonlinearity of the land surface feedbacks to meteorological drought forcing aggravate or dampen the hydrological drought? Does it change extremes (as indicates in title!)? How does it impact severity and extent of hydrological drought? How does land use buffer the impact of (the nonlinear feedbacks of) meteorological drought on hydrological drought? The simulation experiments seem to be designed to address exactly these questions, but the manuscript does not.

(2) Methodology and presentation.

In addition, both, methodology and analyses would benefit from a more precise description. In the following, I will list a couple of (important) issues that remain unclear to me and hampered my understanding:

- Model selection. I understand the advantage of ParFlow as a numerical, physics-based model which simulates lateral flow over other models, such as VIC and SWAT, and the advantage is clear from the description. I do, however, not see the need to "badmouth" other models if they are neither being used and compared, nor validated against observations. Moreover, I do not understand the comparison of lateral flow/ no lateral flow influence on ET in Fig. 13. This is not connected to the research questions proposed and setup and results are not well explained. Do you apply a constant water table in the free drainage runs? If so, did you perform a separate spinup for those runs? Or might the differences in Fig. 13 simply arise because you have different water table depths?

- Numerical experiments. The use of different experiment names, e.g. "Hot and Dry", "hot/dry", "Hot and Dry" (are they all the same "Hot and Dry" run (6) from Tab. 2?) is really confusing and makes it hard to follow. Please unify.

- Simulation period and dependency on land surface/subsurface state. I am totally confused about the simulation period and the setup of the numerical experiments: Which years are simulated? Why is the model set up with data from 1984? Do you simulate a hydrological drought period, and if so, why do you not evaluate the impact of your forcing perturbations on evolution and extent of drought? Even if you do not simulate a hydrological drought, it is still important to evaluate the differences in relation to the land surface and subsurface states. What is the relative importance compared to the natural (modelled) variability? E.g. the reader does not know if a water table difference of $\sim$1 m (Fig. 5) is on the order of natural, (inter-)annual variability and if it occurs in a region of shallow or deep water tables.

- Time scales. The authors only show annually averaged differences, which do not allow to address drought and extreme impacts (as indicated by the title!). The limitation of presenting annual averages becomes evident in Fig. 10, which shows the relation between "antecedent soil moisture" on ET. First of all, I do not really see a "clear break" (p. 17, l. 9). Secondly, do you use an annual average as antecedent soil moisture? Soil moisture varies at much shorter time scales; and a grid cell (region) might move from an energy limited towards a soil moisture limited state within a year, and especially during a drought. Maybe it makes sense to look at shorter time scales ... otherwise I do not see the merit of Fig. 10. Whether a grid cell is soil moisture or energy limited also depends on the soil texture, doesn't it? I am not sure that this can be as simplified as the text does it.

- Spatial scales. Sec. 3.3 remains unclear to me, though it could be potentially very interesting. This may be mainly due to my lack of understanding what is shown in Fig. 12. Could you please clarify? Does Fig. 12 show the same comparison as in Fig. 9 but the sum? But then, which scenario is shown? What are HUC6 and HUC8 basins? And which are the major basins? Which basins do you actually show in Fig. 5?

(3) Minor points.

- If I understand correctly, "anomalies" are neither climatological anomalies, e.g. of soil moisture or runoff to determine drought extent, nor are they the anomalies of forcing during the 1934 (?) drought, which are used to perturb the forcing. If I understand correctly, anomalies in the manuscript are simply the difference between a scenario and the baseline simulation. Please clarify and consider rephrasing.

- A lot of references are missing (e.g., Loon 2015; Eltahir 1998; Seneviratne 2010; Betts 1996; Koster 2004; McEvoy 2016; . . .)!

- p. 11. l. 7-9: The description of Fig. 5 in the text is misleading. If I see this correctly (and as is later on in the manuscript mentioned), Hot (and Crops) have higher ET! Also, please be precise what "lower" WTD means. . . it's deeper?

- Fig. 9: The differences in Fig. 9 (c-d-e-f) are not percentages, are they? 0.4 % difference in Fig. 9 seems rather small and not significant (or do you mean 40% as mentioned in the text on e.g. p. 17?). What do grey colors in Fig. 9 mean? Do I understand correctly, that Fig. 9 shows (a,c,e,g): Hot (3) + Dry (4) - "Hot and Dry" (6) ? (b,d,f,h): Hot (3) + Dry (4) + Crops (5) - "Worst case" (7) ? (with numbers referring to runs from Table 2)

- There are no tables representing results; hence the paragraphs on p. 13 l. 8-11 and p. 16

- l. 6-9 are confusing. Please remove/rephrase.

- p. 14 l. 8-10: this relates to the forcing perturbations, does it? So, if I sum up the precipitation changes from Fig. X over the domain, they correspond to a total change of 40% ?

- p. 17 l. 17-18: Is this related to the results presented in Fig. 9c? (Here, radiation probably has a stronger impact than temperature).

- Conclusions. (i) contradiction of "real world scenario" to what you describe in the introduction and the methods. (ii) "ranges of variation typical of major droughts" (p. 24 l. 7) - I cannot find this classification in the presentation of the results. Could you please expand?

- Might the perturbation of radiation be more important than temperature perturbations? (because this really indicates a limitation of energy)

- p. 11 l. 11: Figure 3c?

- p. 13 l. 10: there is no Table 4.

- p. 13 l. 16 and 19: Figure 6?

- p. 16 l. 11: How do you end up with 93% with 102 and 105 mm?

- p. 16 l. 6-9: there is no table.

- p. 16 l. 16: do you mean Figure 8?

- p. 17 l.8: observed is “modeled” ?

- p. 5 l. 9: 102 m?

- p. 4 l.3: 1930s ?

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