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Interactive comment on "Precipitation legacy effects on dryland ecosystem carbon fluxes: direction, magnitude and biogeochemical carryovers" *by* W. Shen et al.

Anonymous Referee #1

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This is an interesting modelling study examining how dryland ecosystem carbon fluxes respond to precipitation anomalies arriving at interannual and interdecadal time scales. Results are unsurprising but make a few valuable points about the nonlinearities (thresholds and filters) in carbon flux responses to wet and dry events. Findings are heavily dependent on the model's approach. Interpretations need to be revisited in a few places. The writing needs to be improved. Insights regarding mechanisms get disappointingly little attention in terms of quantitative analysis. But overall this paper makes a useful contribution.

Major Critiques:

Line 121: If the third question is to identify the mechanisms that are responsible for legacy effects, why then do you make an assumption that allows only a single answer? The methods chosen do not seem to allow for you to identify the mechanisms responsible. Instead, the mechanisms are hard-wired into the PALS, pulse-reserve modeling framework that has been adopted, so there is no real discovery to be had.

Section 2.3: Calibration / Validation makes incomplete and weak use of the data: The approach for model cal/val should be improved with cross-validation and bootstrapping. Fit the model (calibrate it) many times with different subsamples of the observations and then select model parameters based on the best-fit results from validation with the remaining observations.

Mechanisms are not deeply explored and evidenced, which is especially disappointing given that this is a modeling study in which case you know everything and how everything works. A revision should seek to give more attention to exposing the specific mechanisms that give rise to the reported dynamics.

Specific Comments:

Line 59: “the savanna ecosystem” , clarify which or where... certainly not all globally?

Line 64: consider examining Williams et al. 2006, which does explore legacies on interannual and interdecadal time scales to some degree, and citing as appropriate.

Line 70-71: consider reviewing and citing contributions by Huxman et al. 2004 in Nature and Huxman et al. 2004 in Oecologia.

Line 76 - 77: consider renaming “structural attributes” to replace “attributes”.

Line 91: consider including citation of Williams et al. 2009 in Oecologia which also shows lagged effects for respiration.

Section 2.1: Some key details of the model need to be presented a little more fully. -What phenomenological model has been adopted for representing canopy stomatal

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resistance, and plant photosynthesis (e.g. Jarvis-type, or Farqhar and Ball-Berry)? - What are the details of how soil moisture influences plant productivity, plant respiration, and heterotrophic respiration? -Is the model's allocation strategy trained to respond to seasonal, interannual and interdecadal variations in water availability? This is a key for the present study but the data rarely exist to parameterize such dynamic behaviors in models.

Line 219 +: Explain what is "annual" for this paper. This may seem like a detail but it can be really important for assessing "legacies" or carry-over effects. Is it water year (October to September) or calendar (January to December) or some other time period? How does it encompass the two growing seasons and dry seasons? It would be most logical to start your "annual" period at the end of the longer of the two dry seasons, meaning the end of your warm dry season, or end of June.

Line 240: Why do you use SPI to assess legacies? Using a standard-normal, statistical translation of absolute values can significantly distort the physiological / ecological meaning or implication of a precipitation anomaly. I recommend you consider sticking with the absolute precipitation anomalies to avoid creating artificial, spurious lags or legacies.

Figure 2: It seems odd that the model fit for NEP is so poor for the calibration period while so strong for the validation period. Note that the calibration period always has NEP > 0 while the validation period has a year of NEP < 0.

Figure 2: is the R2 shown here for all seasons pooled together? That seems odd. They should each be regressed independently or else only show one of them. The R2 for each season (CS, WS, Annual) pooled is ill-advised.

Your analysis should show early on (e.g. before Fig 3) observed carbon fluxes versus precipitation for annual, CS, and WS periods to describe a baseline portion of variation explained without considering legacy effects.

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Section 2.3: the writing in this section is poor and needs to be improved. Line 252: "faster" is odd diction Line 253: ".. of the variations in observed ones" has awkward diction and syntax. Line 257: "explanative" is incorrect (explanatory)

Year 2006: The model performed poorly for this year, and it was suspected that this is because of an extreme drought impact. Taking this to be the case, doesn't this imply that the model is not capable of capturing drought responses, and if so, doesn't this call into question the use of the model for the intended application... to study lag or legacy drought impacts which are likely to be strongest and most important in the extreme cases?! Even if you intend to study "non-extreme influences of legacies (Line 265)", the fact that the model performance bounces back to being just fine following the 2006 drought seems to argue that there are only negligible legacy effects from extreme precipitation anomalies. This point should be brought out and discussed more critically.

Line 271: there is no single threshold or cutoff for what is acceptable model performance. a cut-off of 50% would seem absurd for some contexts.

Model experiment designs for both interannual and interdecadal variations look good.

Why are legacy effects calculated as a cumulative anomaly over the simulation period? Certainly the effect size would then depend on the year in which an interannual perturbation was imposed, for example, having a large opportunity for legacy effects if a perturbation occurred in 1995 than if a perturbation occurred in 2010.

Fig 3: typo in (a) for "Cuurent"

The model's results of the interdecadal legacy seem rather obvious... not that this is all bad but it does limit the paper's contributions of discovery and insight to some degree, especially because results are model-based. A dry prior period knocks vegetation back such that the current period has more growth and less respiration. A wet prior period allows more vegetation growth which elevates respiration in the current period but has little effect on GEP. However, it is puzzling that a prior dry period elevates GEP. What

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model dynamic explains this? [later it comes out that this is purportedly related to an accumulation of soil nitrogen that becomes available – which is possible but raises some other questions as raised below.]

Line 326: “wet legacies imposed mostly negative impacts on current-period GEP”. This is not consistent with what I see in Figure 3a, where it looks like a wet legacy has little to no effect on GEP.

Fig 5. This must be showing anomalies in states not absolutes, right? This should be clarified in the y-axis labels with a delta in front of each label.

Explain how the legacy duration is quantified. Is it somehow weighted by the magnitude of response so that subtle differences many years later are ignored? Also, explain why, mechanistically, it is so variable.

Explain the odd results of a -30% prior year interannual precipitation perturbation for year 2000, which really stands out. Also, where is this year’s data point in Figure 7? It seems to have been selectively removed, no? There is no reason to treat it as an outlier, this being a set of model results with no room for sampling error as you would otherwise have with observationally based study.

Line 452: the second mechanism is poorly explained. please clarify, particularly regarding what is meant by “if the resources produced ... were not completely lost...”.

Line 459: The third mechanism is not a mechanism at all. What is being stated here?

Lines 460 to 476: The argumentation is unclear here. You point out that your simulation results do not show a soil water carryover effect, but then you go on to state that it should be considered to be a potential mechanism. Do you mean that you think your model is wrong in that it lacks this mechanism? Why? What justifies this speculation, which is inconsistent with your findings? What would be done to include this?

Line 482: If Nsoil is high in a dry legacy because plant uptake has been squashed, why is GEP elevated post-dry period when the plants have to invest in acquiring N

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that they would have otherwise had? THis mechanism in the model seems odd to me. Is a sudden pulse of N better at supporting GEP than a plant canopy that already possessed that N? Perhaps some of that N would have otherwise been tied up in non-photosynthesizing plant parts (stems, roots), but is that what really happens?

Diction and syntax are troubled throughout this section. example: 488: “The N enhancement as dry legacies also explains...”

Overall, it seems appropriate to put the magnitude of these legacy effects into the context of the magnitude of effects from current-year or current-season precipitation anomalies.

Line 523: poor wording here.

Line 523: This paragraph, including speculation and needed new directions, seems out of place in the conclusions section and would be more appropriate at the end of the discussion section.

Citations:

Huxman TE, Snyder KA, Tissue D, Leffler AJ, Pockman W, Ogle K, Sandquist D, Potts DL, Schwinning S (2004) Precipitation pulses and carbon balance in semi-arid and arid ecosystems. Oecologia 141:254-268.

Huxman TE, Cable JM, Ignace DD, Eilts AJ, English N, Weltzin J, Williams DG (2004) Response of net ecosystem gas exchange to a simulated precipitation pulse in a semi-arid grassland: the role of native versus non-native grasses and soil texture. Oecologia 141:295-305.

Huxman TE, Smith, MD, Fay PA, Knapp AK, Shaw MR, Loik ME, Smith SD, Tissue DT, Zak JC, Weltzin JF, Pockman WT, Sala OE, Haddad BM, Harte J, Koch GW, Schwinning S, Small EE, Williams DG (2004) Convergence across biomes to a common rain-use efficiency. Nature 429:651-654.

Williams CA, Albertson JD (2006) “Dynamical effects of the statistical structure of annual rainfall on dryland vegetation”, Global Change Biology, 12, 777–792, doi: 10.1111/j.1365-2486.2006.01111.x

Williams CA, Hanan N, Scholes RJ, Kutsch W (2009) “Complexity in water and carbon dioxide fluxes following rain pulses in an African savanna”, Oecologia, 161(3): 469-480, doi: DOI 10.1007/s00442-009-1405-y.

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