Biogeosciences Discuss., 12, C6290–C6304, 2015
www.biogeosciences-discuss.net/12/C6290/2015/

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

W. Shen et al.

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Received and published: 8 October 2015

Dear Editor,

We are grateful for giving us the opportunity to improve our manuscript. We have carefully studied each suggestion/comment of the reviewer, and incorporated them into this revised manuscript. A detailed list of the responses to the reviewer's comments and of the changes we made in the revised version is provided below and in the Supplement file. I hope that you will find them satisfactory.

Thank you again for reconsidering our manuscript. I look forward to hearing from you.

Sincerely,

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Reply to Referee # 1

Comment #1: 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.

Re: Thanks to the reviewer for considering our modelling study interesting and for providing several constructive comments. We reply to each of your comments/suggestions in the following.

Comment #2: 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.

Re: We agree with the reviewer that the mechanisms that are responsible for the modeled legacy dynamics are already built in the model. We deleted the third question.

Comment #3: 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.

Re: The reviewer suggests a more rigorous way of model calibration and validation. However, the PALS model we used is written in the STELLA platform, which hinders us from making the automated model runs necessary to complete this type of model calibration. We therefore calibrated the model within the platform by adjusting some of the key parameters such as photosynthate allocation ratios, death rates of plant organs, and decomposition coefficients of litter and soil organic matter to reach the best fit between the simulated and observed fluxes.

Comment #4: 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.

Re: We have revised the explanations for the modeled legacy dynamics in the discussion section 4.2 (lines 476-555). We hope these revisions would be helpful in understanding our work.

Comment #5: Line 59: “the savanna ecosystem” , clarify which or where... certainly not all globally?

Re: The mesquite savanna ecosystem is located on the Santa Rita Experimental Range (SRER), 45 km south of Tucson, AZ, UAS. The grassland ecosystem is located on the Walnut Gulch Experimental Watershed (WGEW), 11 km east of Tombstone, Arizona, USA. We have clarified that there is one ecosystem (a savanna and a grassland) in each of the two study (Scott et al. 2009 and Hamerlynck et al. 2013), and both of them are located in southeastern Arizona, USA.

Comment # 6: 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.

Re: We revised the sentence to appreciate the modeling study by Williams et al. 2006 on how increased rainfall variability may influence dryland vegetation production at interannual and interdecadal scales (line 63-66).

Comment #7: Line 70-71: consider reviewing and citing contributions by Huxman et al. 2004 in Nature and Huxman et al. 2004 in Oecologia.

Re: These studies showed the importance of precipitation pulse size and frequency in controlling the activity of plants and microbes in aridland ecosystems. Hysteresis effects between rainfall pulses (i.e., precipitation legacy effects at rainfall event scale) were also analyzed in these studies. We have cited Huxman et al., 2004a, b (the two Oecologia papers) in line 55 and Huxman et al., 2004c (the Nature paper) in line 74.

Comment #8: Line 76 - 77: consider renaming “structural attributes” to replace “attributes”.

Re: The word “attributes” has been replaced with “carryovers” (line 79).

Comment #9: Line 91: consider including citation of Williams et al. 2009 in Oecologia which also shows lagged effects for respiration.

Re: The work has been cited in Line 92. Thanks for the recommendation.

Comment #10: 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 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.

Re: Since these key model details have been presented in our previous publications, we did not describe them in detail in this manuscript. Specifically, the algorithms for calculating plant production, photosynthesis, stomatal conductance, and their relations with water and nitrogen conditions are presented in equations (10) through (14) in Shen et al. (2005, Ecological Modelling, 189, 1-24); the algorithms for calculating autotrophic respiration, heterotrophic respiration, and their relations with temperature, moisture, and nitrogen conditions are presented in equations (A4) through (A11) in Shen et al. (2009, Global Change Biology, 15, 2274-2294). To present all these detail model descriptions, it would take about 5-6 more manuscript pages. We therefore only added some brief descriptions as suggested by the reviewer in lines 165-181 to help potential readers to examine these key mechanisms built into the model.

Comment #11: 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.

Re: In line 219 (line 230 in the revised manuscript), "annual" refers to the calendar year (January-December). For calculating seasonal fluxes, "annual" refers to December (of a previous year) to November (of a current year), which has been defined in lines 231-232. For calculating yearly "or annual" fluxes, we used the calendar year (January-December) for the reason that annual ecosystem carbon fluxes are usually reported in the literature on the basis of a calendar year. We added one sentence to clarify this (Line 235-236).

Comment #12: 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.

Re: The main purpose of using SPI is to indicate whether a particular year is a wet, a normal or a dry year (see Fig. 1). SPI is also used in the Spearman correlation analysis (see Table 1). We actually tried both SPI and absolute PPT amount in this analysis; both indices received exactly the same correlation coefficients and the significance levels. In Fig. 6, SPI is also used to indicate year type (wet, normal or dry) and to show whether the direction of legacy effects differ among year types. The quantification of legacy effects is solely based on the carbon fluxes simulated (see the equation on page 16), not on the PPT amount or its anomalies.

Comment #13: 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.

Re: Intuitively, the model fit should be better for the calibration than the validation period. But that is not the case in our study, mainly because the three validation years have much larger precipitation variation (229-404 mm) than that in the four calibration years (285-329 mm). The larger precipitation variation in the validation years results in larger GEP, Re and NEP variations (see the new Fig. 2) that are better captured by the model simulations. In the original Fig. 2, NEP actually is always less than 0 (i.e. C source; see the open dots in Fig. 2d) in the calibration period, while there are two years with NEP>0 (i.e., C sink; see the open dots in Fig. 2h) in the validation period. We don't know what confused the reviewer, but this now can be seen more clearly in the new Fig. 2d.

Comment #14: 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.

Re: We agree with the reviewer that pooling all seasonal and annual data together to conduct a regression analysis is logically wrong. Thanks to the reviewer for the constructive comment. We re-conducted such analysis separately for each of the two growing seasons and the calibration and validation years. A new Fig. 2 has been created to present these new results. However, we only showed the comparisons between the observed and simulated fluxes at the annual scale in the new Fig. 2 (left panels), with seasonal comparisons being presented in the supplementary Figure S3, since this modeling analysis is mainly focused on the interannual and interdecadal scales.

Comment #15: 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.

Re: Following the reviewer's suggestion, we conducted a new analysis on the relations of the observed (and simulated) fluxes versus precipitation under the baseline PPT conditions (i.e. without changing the previous- or current- year precipitation). The results are shown in the new Fig. 2 (right panels). It is noted here that although the portion of the annual carbon flux variations can be explained largely (R2 mostly > 0.70) by current-year precipitation, that inseparably contains the legacy impacts from previous-years. This is also the main logical basis of our simulation design, i.e. by changing the previous- and current-year precipitation separately to discriminate the previous- and current-year precipitation effects on current-year carbon fluxes.

Comment #16: 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)

Re: Based on the new Fig. 2, we revised the section to report the new results (Line 262-299). "faster" has been replaced with "larger" (line 263). "explanative" has been corrected to "explanatory" (line 292).

Comment #17: 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.

Re: The model is calibrated by pursuing a best fit between the simulated and observed gross primary production (GEP) and ecosystem respiration (Re) in four calibration years (2004-2007). It is therefore not surprising that the model performed well in terms of GEP and Re with the R2 being larger than 0.6 (see new Fig. 2b, c), but performed poorly in terms of NEP in these four years with the R2 of 0.0001 at the annual scale (Fig. 2d), because NEP is actually calculated from GEP and Rec. We identified that this was mainly due to the poor performance in 2006, a year with an extremely dry cool growing season. If the data of this year were excluded, the R2 for NEP could reach above 0.70. The model performed very well in the three validation years (2008-2010), with R2 values for different fluxes being all larger than 0.9 (see Fig. 2, left panels). These model calibration and validation results indicate that the model is capable of capturing the annual variations of ecosystem-level fluxes including NEP in 6 out of the 7 years (2004-2010), with 2006 being an exception. We think that the poor performance in 2006 is mainly because the built-in empirical relations between the rate of tissue death (or plant mortality) and the influential factors (e.g., air temperature, soil moisture, and plant phenology) account for more "normal" climate conditions rather than extreme conditions. Although there are many studies that have documented that extreme drought can cause more severe plant mortality, the quantitative or empirical relation between drought severity and plant mortality rate for the studied mesquite savanna ecosystem is still lacking, which hinders us to incorporate more robust relations into the model. Considering such extreme cool-season drought as in 2006 only occurred once in the 30-year simulation period, we therefore think the overall model performance is acceptable. We briefly explained such possible reasons in the discussion section (see lines 550-556).

Comment #18: 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.

Re: $R2>0.5$ was suggested as a rough criteria to assess hydrological models (Moriasi et al. 2007, Transactions of the ASABE, 50, 885-900). For the three calibration years, the R2s for the observed versus simulated fluxes were all >0.9 (see new Fig. 2, left panels). We therefore deleted the sentence and the citation.

Comment #19: Model experiment designs for both interannual and interdecadal variations look good.

Re: Thanks.

Comment #20: 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.

Re: At the inter-decadal scale, we divided the 30-year period into two sub-periods based on the baseline PPT conditions showing in Fig. 1. While calculating the subperiod-scale fluxes, we used the cumulative fluxes throughout the subperiod and the legacy effects were further calculated based on the cumulative flux anomalies. This is analogous to what we would get annual fluxes by summing up all daily fluxes in a year.

Comment #21: Fig 3: typo in (a) for "Cuurent"

Re: Fixed.

Comment #22: 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 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.]

Re: It was puzzling to us too that a prior dry period/year elevates current-period/year GEP, since aboveground net primary production (ANPP) has been found to have a negative response to a prior dry year. By the notion, GEP and ANPP should all reflect "production". But ANPP of dryland ecosystems is often estimated by harvesting biomass, so we argue that field observed ANPP is actually "biomass". Our simulation results showed that biomass had a negative response to a prior dry year (see Fig. 5a,b and Fig. 8a, b), which is consistent with what has been found in field studies for ANPP. In the PALS model, GEP is calculated based on the photosynthesis rate that is linearly related to nitrogen availability, indicating that accumulated N in a prior dry year can stimulate GEP in a current year especially when water is not limiting. That explains why a prior dry year imposes mostly positive legacy impacts on current-year GEP when the current-year PPT was increased (see Fig. 3b) but impose no impacts or even negative impacts on GEP when the current-year PPT was reduced (see Fig. 3a).

Comment #23: 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.

Re: The statement was made in terms of the sign (positive or negative) of the numbers plotted in Fig. 3a, which can be better seen (see the attached Fig. 3) with the zero lines being added. But in terms of the magnitude of the numbers, the effects are indeed very small. We therefore replaced “mostly negative” will “little” in the text (line 350).

Comment #24: 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.

Re: No, those are not absolute flux values. They are the legacy effects calculated as the difference between the current-period flux with previous-period PPT change and that without previous-period PPT change (see the equation in page 17 for how we define legacy effects). To avoid ambiguity, we added a delta in front of each label as the reviewer suggested and explained what that means in the figure caption (lines 864-866).

Comment #25: 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.

Re: The legacy duration means how long the legacy lasts after a PPT perturbation in one particular year. It is quantified as the number of years until the impacts on NEP vanish (i.e. the carbon fluxes equal to those under baseline PPT conditions). For example, a decrease in PPT by 30% in year 1982 caused carbon flux changes in the following 4 years (i.e. 1983-1986) compared with the fluxes without changing 1982 PPT, then the legacy duration is 4 years (see Fig. 6a). We added one sentence in the caption of Fig. 6 to clarify this. Similar to the direction and magnitude of the legacies, the lasting duration of the legacies were very variable as well, mainly because yearly PPT (see Fig. 1) and the corresponding PPT alterations were very variable.

Comment #26: 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.

Re: The odd result (or exceptionally high value) is actually for year 1999 (see Fig. 6, left column), which is a result of a -30% PPT change in 1998. We double checked our dataThe odd numbers in 1999 and 1984 were resulted from a mistake during legacy calculation and they have been corrected in this revision (see Fig. 6).

Comment #27: Line 452: the second mechanism is poorly explained. please clarify, particularly regarding what is meant by “if the resources produced ... were not completely lost...”. Comment #28: Line 459: The third mechanism is not a mechanism at all. What is being stated here?

Re: The first mechanism explains why a biogeochemical carryover (e.g. SOM) can cause changes in flux rates. The second mechanism explains why biogeochemical materials (e.g., biomass or SOM) can be carried over. The third mechanism explains why different types of biogeochemical carryovers (e.g. nitrogen) can form legacy impacts on carbon fluxes. We have revised the descriptions of these mechanisms (see line 476-492).

Comment #29: 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?

Re: We wanted to emphasize that water carryover was not a major contributor at interdecadal and interannual scales, but it could potentially be important at seasonal or event scales. Since we did not analyze the legacy effects at seasonal or event scales, we deleted the unrelated descriptions from lines 503-508.

Comment #30: 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 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 nonphotosynthesizing plant parts (stems, roots), but is that what really happens?

Re: The model assumes that plant growth or photosynthesis is directly modified by N availability as in the following equation: Please see the supplement PDF file for the equation where Gj is the amount of daily plant growth (g dry mass m-2) for functional type j, Xlvs is the leaf dry mass (g), SLA is the specific leaf area (m2 g-1), Amax,j is the maximum potential net photosynthetic rate (mol CO2 m-2 s-1), 12 (g) is the mass of C per mol CO2, 0.46 is the average C content (46%) in plant tissues, Rloss is the respiratory loss of photosynthetic production per day, Ft is the temperature influence factor (for forbs and grasses, not for shrubs and annuals), Fc (2/π photoperiod 3600) is a conversion factor (changing time unit from second to day), and is a linear scalar accounting for the effect of leaf N on Amax,j (see Eqn. (13) in Shen et al. 2005, Ecological Modelling, 189, 1-24). Based on this model assumption, high soil N availability would result in larger plant growth or GEP when water is also available. The PALS model also assumes plants take nitrogen directly from soils and allocated to different organs (leaves, stems, and roots). It is biochemically possible the some stored N in stems and roots may be used for photosynthesis in leaves. But this has not been incorporated into PALS.

Comment #31: Diction and syntax are troubled throughout this section. example: 488: “The N enhancement as dry legacies also explains...”

Re: “The N enhancement as” is replaced with “The carryover of N from” (line 521).

Comment #32: 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.

Re: We don’t truly understand the comment. Is that what we have done in Table1, Fig. 4 and Fig. 7?

Comment #33: Line 523: poor wording here.

Re: The description has been reworded (lines 571-576).

Comment #34: 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.

Re: We moved the paragraph to the end of the discussion section and revised it accordingly (Lines 544-550).

Comment #35: Citations:

Re: The five references have been cited in the revised version.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C6290/2015/bgd-12-C6290-2015-supplement.pdf

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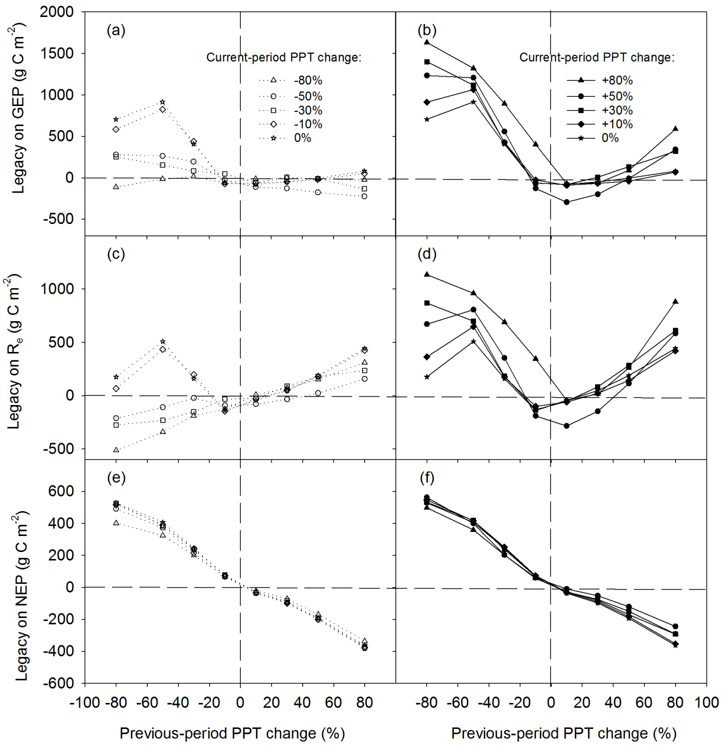
Interactive comment on Biogeosciences Discuss., 12, 9613, 2015.



(a)
(b)
(c)
(d)
(e)
(f)
Current-period PPT change:
-80%
-50%
-30%
-10%
0%
+80%
+50%
+30%
+10%
Legacy on GEP (g C m-2)
Legacy on Re (g C m-2)
Legacy on NEP (g C m-2)
Previous-period PPT change (%)

Fig. 1.