

Interactive
Comment

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

Anonymous Referee #2

Received and published: 11 September 2015

This manuscript uses a calibrated ecosystem model (PALS) to study precipitation legacy effects on a single calibrated site near Tucson, AZ. The authors designed a series of rainfall manipulation experiments and conducted the sensitivity analysis in the PALS model. Overall, the manuscript studied the legacy effect in an interesting way, but I find the current results/discussion are very premature. I have some serious doubt on the results part (see my following details). Another major issue of this manuscript is the lack of discussion on the probable mechanisms, which is very disappointing. I suggest major revision at most.

I found the results in Fig 3 very suspicious. First, if the previous-period PPT change is 0% and the current-period PPT change is 0%, then your results of any legacy terms

C5195

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



should be zero. But I simply find this is not the case. Furthermore, the results shows a positive legacy when there is a negative change in previous-period PPT (Fig.3a), i.e. the result here is saying if there is a decrease of rainfall in past, the current-period GEP will increase compared with no prior rainfall change. This is a striking result (also highly suspicious), and the authors failed to provide convincing explanations on that. The soil nitrogen argument (i.e. more N in soil becomes available during dry years) is really a stretch and with little support (Fig. 5 does not support this point at all, as it only shows the scenarios of increasing current rainfall). Still in Fig. 3, the authors claimed “wet legacies imposed mostly negative impacts on current-period GEP” (Fig. 3a), which is simply not true from your figure.

Based on the results in Fig 3-5, I have serious doubt about the scientific robustness of this work.

Besides the points raised above, the authors neglected the rich literatures on the dry-land ecohydrology that discusses the intra-seasonal rainfall effects (e.g. rainfall frequency, intensity) on ecosystems. Please search literatures by Rodriguez-Iturbe, Porporato, Albertson, etc and incorporate them in your manuscript. It has to be recognized that the proposed rainfall change in this manuscript is only changing the rainfall intensity (i.e. simply multiplying a ratio to all the rainfall events) but does not change any rainfall frequency or seasonality. This is fine as your approach has been largely used elsewhere, but recognizing its limitation is necessary.

The manuscript in general is very hard to follow esp. in the results and discussion section. The authors defined “legacy” term only for NEP, and you should add “legacy terms for other variable of interest follow the same definition”.

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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)