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Interactive comment on "Differential effects of extreme drought on production and respiration: synthesis and modeling analysis" *by* Z. Shi et al.

Z. Shi et al.

zheng.shi@ou.edu

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Reply to Reviewer #2

Response:

We wish to thank reviewer #2 for the detailed comments on our manuscript, which are greatly helpful for us to improve our manuscript. Please see detailed reply to each comment. Reviewer's comments are in grey, italic font, and our responses are in black font.

Reviewer:

The paper by Shi et al. seeks to synthesize across-study findings about how ecosystem production and respiration respond to droughts, and attempts to examine underlying mechanisms with a terrestrial ecosystem model applied to represent four grassland sites. The paper is topically appropriate for the special issue, and it might make a suitable contribution but offers disappointingly little in the way of new insights despite the paper's bolder claims.

Response:

The reviewer understood the scope and objectives of our study well. It is unfortunate that the reviewer did not see the potential contributions our manuscript could make. As we will offer more detailed responses to each of the reviewer's comments below, here we wish to highlight unique contributions our study could make to the special issue at least in two aspects. First, our study, to the best of our knowledge, is among the first to combine data synthesis to reveal general patterns with modeling to reveal probable mechanisms underlying the general patterns. In the past, studies were done with either data synthesis or modeling of drought effects on biogeochemical processes. The combined approaches offer much deeper insight into ecosystem responses to extreme climate events than either of the approaches individually. Our study is among the first that not only revealed variations of differential drought impacts on photosynthesis and respiration with drought severity and ecosystem types but also evaluated relative contributions of carbon input, soil water dynamics, and soil carbon change to reduced heterotrophic respiration.

Second, our data synthesis offers insights that previous publications have not. There are papers published in the literature to demonstrate differential effects of drought on photosynthesis and respiration (e.g., Schwalm et al., 2010a). But none of the studies has showed how the differential effects vary with the severity of extreme events and vegetation types. Our synthesis has shown that the differential sensitivity between production and respiration increased as drought severity increased and occurred only in grassland ecosystems.

Nevertheless, thanks to the reviewer's comments (even though it is tough to read such negative ones), we have done more synthesis and modeling during the revision. The revised manuscript shows that drought has differential effects on gross primary production (GPP) and ecosystem respiration (ER) only in grassland (Fig. 2b) and our modeling evaluated relative contributions of carbon input, soil water dynamics, and soil carbon change to reduced heterotrophic respiration (Rh) (Fig. 7).

Reviewer:

I have the following concerns. 1) Synthesis Is Not New: A significant part of the set-up for the manuscript is about synthesis of past work. Unfortunately, the presentation does not take us beyond the basic conclusion that ecosystem productivity declines more than respiration in response to drought, and that extreme droughts cause larger responses. There are no substantive insights about how responses vary by ecosystem types aside from the notable difference in rainforests.

Response:

Thanks to the reviewer's comment, we did more synthesis during the revision and added Fig. 2b, which shows drought responses of GPP and ER in different ecosystem types. Together with Fig. 2a, which shows variations of drought effects with severity of the drought events, the synthesis of our study, to our knowledge, makes those points that are not only novel (none of the previous publicaitons has made so far) but also are valuable for us to understand ecosystem responses to extreme climate events.

Reviewer:

2) Interpretation of Different Effects of Reduced Amount or Reduced Frequency is over-stated: It is suggested that the effect of drought differs depending on how drought is delivered, either by fewer events or smaller events of the same number. However, the results of the model experiment and the analyses presented do not support this interpretation and conclusion. First of all, the graphical presentation makes it difficult to

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compare the two cases. More importantly, statistics are not presented to evaluate if these treatment effects do, in fact, differ significantly. Even if they are statistically different, the magnitudes are very similar and the direction of their unique effect sizes varies across sites, if their effect sizes are in fact different. Thus the results section describing this (section 3.2, lines 232+) needs to be revised. Also the discussion should be revised to remove claims about these differences (section 4.3, Lines 339+) and corresponding implications (section 4.4, Line 361+) for future experiments.

Response:

We greatly appreciate the comments by the reviwer. In response, we have done a paired sample T test (paired by year) for each of the grassland sites and found that the difference was significant bewteen the two rainfall reduction treatments. Thus, our results were statistically supported. The statistics has been updated in Table S2. To clarify the direct comparision between the two treamtments, the two treatments effects on the carbon variables were plotted together as illustrated in the supplementary figure (Fig. S1).

There are two main points we tried to address in this part. First, the two drought types have different effect on ecosystem carbon cycling; second, the effects vary among different grassland sites. Specifically, even size reduction (ESR) reduced NPP and Rh more in the two sites with higher rainfall amount, whereas reduced event number (REN) reduced NPP and Rh more in the other two sites. The finding that the effects vary across sites was supported by a field study (Heisler-White et al., 2009). The reviewer is right that the differences in the drought impacts between the two types of treatments are small. In response, we further reduced rainfall frequency based on REN and found the difference became larger (data not shown), because the soil water variation was increased more (Knapp et al., 2002). This finding that different levels of frequency affect effect size was also supported by the field study by Heisler-white et al. (2009). Since different levels of rainfall frequency were not the focus of this study, the data were not reported for further reduced rainfall frequency. Additionally, another objective of this part of the modeling analysis was to generate interesting hypothesis for field experiments to explore. Therefore, our findings and corresponding implications for future experiments are still valid.

Reviewer:

3) Mechanisms Not Really Revealed: The second objective of the study is to use an ecosystem model to examine mechanisms that may underlie differential sensitivity of production and respiration. The analysis of the model output does not examine mechanisms, except to show the long-term change in soil carbon and this is not sufficient to illuminate the causes. The basic idea is not all that new: Rh is supplied partly by slow-changing soil carbon sources whereas GPP (and to some degree by extension, NPP) results from short-term physiological response closely tied to weather, and this causes Rh to have a milder response to drought that then accumulates with continued exposure. The fact that this emerges in a model that simply works this way is not especially revealing. Furthermore, the analysis presented does not make much of the details that are available from modeling. The luxury of modeling is that you can look at everything. For example, to what degree is Rh sensitive to the reduced supply of photosynthate imposed by drought as opposed to the declining soil C stocks, and how does this relative importance shift over time with continued exposure? What are the partial roles of soil water limitation, soil temperature, and carbon availability in driving changes in Rh in response to drought, and how does their relative importance shift over time with continued exposure? It is disappointing not to see better use of the data on hand.

Response:

Excellent comments made by the reviewer in spite of the negative tone. In response, we have made additional analysis and added a figure (Fig. 7) to illustrate the relative importance of the three factors (i.e., soil water content, NPP and soil carbon content) in controlling the drought response of Rh over time. The figure showed that the reduction in Rh was jointly caused by reduced soil water content (SWC), carbon input (i.e., NPP) and soil carbon content together. Over time, the relative importance of reduced soil carbon content became greater. We have updated the manuscript to reflect the new findings (Lines 268-272 and lines 327-335).

Reviewer:

4) Limitations of Using a Model to Assess Long-term Responses to Drought: Models may well miss ecological processes that become important at longer time scales, such as acclimation, mortality, and species shifts, all influencing the physiological capacity of the ecosystem and potentially causing it to respond differently to continued forcing. Given that such processes are either absent or parameterized with limited observational information, it is not clear that the modeling experiment shown here is justified as a tool for diagnosing effects of long-term exposure to drought. This might need further, more open discussion as a limitation, particularly regarding conclusions about the long-term decline in soil C and corresponding long-term decline in the differential drought-sensitivity of productivity and respiration (Lines 311+).

Response:

Thanks to the reviewer's comment. Model limitation in simulating species shift (or vegetation dynamics) under long-term climate change was discussed in the original manuscript (Lines 371-380). In response to this comment, we added section 4.5 to further discuss model limitations in simulating biotic adaptation (Lines 418-445) as follows:

"Ecosystem carbon models have often been used as a tool to investigate effects of global changing on ecosystem carbon cycling (Norby and Luo, 2004; Parton et al., 2007; Luo et al., 2008; Schwalm et al., 2010b; Grant et al., 2011). At present, most of the models, however, do not represent photosynthetic and respiratory acclimation (Smith and Dukes, 2013), mortality (McDowell et al., 2013), and species shift (Sebastia et al., 2008) well yet due to limited understanding. As a consequence, their regulations may not be well captured in the modeling results when models are used to simulate long-term effects of climate changing factors on ecosystems. In this study, we used data from space-for-time studies to support our model results. For example, soil C declined linearly with decreasing precipitation in observations along precipitation gradients (Anderson et al., 2011; Talmon et al., 2011), which is consistent with our modeling results indicating a long-term drought-induced decrease in soil C content. This consistency between model and empirical studies suggest that the responses of ecosystem variables to extreme climatic changes are unlikely to be overridden by biotic adaptation (Anderson et al. 2011). Rather, the extent of the responses might be attenuated or exacerbated (Smith 2011, Reichstein et al., 2013). Nonetheless, further research is needed to incorporate acclimation, adaptation and vegetation change into ecosystem models to improve ecological forecasting."

Additionally, we added discussion on mortality in grasslands in section 4.5 as follows:

"Vegetation mortality due to carbon starvation or hydraulic failure or both (McDowell et al., 2008) is likely to occur if the drought is severe enough and can therefore have legacy effect on most aspects of ecosystem carbon cycling (Liu et al., 2011). It is difficult for ecosystem models to accurately capture plant mortality due to the lack of thorough understanding on the mechanisms (Xu et al., 2013, McDowell et al., 2013, Reichstein et al., 2013). Mortality in grasslands differs from that in forest ecosystems. In a forest ecosystem, when large area of mortality occurs, the whole ecosystem would have to start over from secondary succession. However, grasslands are characterized by the high recovery potential of plant growth and they would recover to their original states in a very short time and had less impact on carbon cycling than forest ecosystems (Reichstein et al., 2013)."

Reviewer:

5) Complexity is Not Demonstrated Clearly: The fourth implication mentioned in section 4.4 does not emerge clearly from what is presented. What is shown to motivate

macroscale global change experiments? What is shown to suggest cross-scale interactions? What is shown to suggest differential sensitivities (a little is here maybe)?

Response:

The fourth implication in section 4.4 has been deleted according to the reviewer's comment.

Reviewer:

6) Across Ecosystem Synthesis Collapses to Grasslands When Examining Mechanisms: If not in the results, then at least in the discussion, it would be valuable to have conjecture about how the inferred mechanisms and long-term response patterns might change for the case of droughts in other ecosystem types, for example a range of forest types. This is not imperative, and you can't do everything of course, but the paper's set up is rather grand leaving the reduced emphasis to grasslands a little disappointing.

Response:

Thanks to the reviewer's comment. It is true that we started the study with synthesizing drought effects in a broad range of ecosystem types. Since we only found differential responses to drought in grassland ecosystems (Fig. 2b), we focused on grasslands when exploring mechanisms underlying differential responses.

The mechanisms associated with drought responses in forest ecosystems are likely to be similar to those in grasslands. The primary responses of forests to drought are to reduce productivity and respiration due to water deficit (Dale et al., 2001) and the responses of production and respiration could be different. However, it is also likely that the mechanisms could be more complicated in forest ecosystems than in grasslands and consequently have different long-term effects. For example, deep rooting systems in forest ecosystems could buffer drought effect on production in forest ecosystems; drought associated increase in solar radiation might increase production as we found in rainforest; hydraulic lifting by tree roots could also alleviate drought effect on C pro-

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cesses. As we found out in our synthesis, drought had similar effects on both production and respiration in forest ecosystems (Fig. 2b). As a consequence, the long-term response pattern may be different from that in the grassland ecosystems. Therefore, it is critical to include above possible mechanisms when simulating long term drought effect in forest ecosystem. As suggested, relevant discussion has been added (Line 366-378).

Reviewer:

More Mechanical Concerns: 1) Methods section 2.2.3 presents Fig 1 and Table 2 but these should be presented in the Results section at the front end of the modeling application.

Response:

It has been updated according to the comment.

Reviewer:

2) L121: just rainfall or all precipitation forms?

Response:

It is all precipitation forms and it has been updated accordingly.

Reviewer:

3) L80: Change "We" to "It has been..." because not all of the authors of the manuscript were involved in this hypothesis.

Response:

It has been updated accordingly.

Reviewer:

4) L86: Is it drought that is manipulated, or rainfall / throughfall that is manipulated?

Please reword.

Response:

The sentence has been changed to “Drought has often been imposed . . .”.

Reviewer:

5) L70: To suggest that there has been no synthesis across sites in search of possible general patterns overlooks some work of this sort, including Schwalm et al. 2010, which certainly does perform such a synthesis.

Response:

We apologize for the inaccurate statement. We have revised the manuscript to acknowledge the previous synthesis.

Our synthesis differs from Schwalm et al. 2010a in several ways. First, drought was represented differently. In our study, we investigated reduced rainfall-caused drought which was consistent to our modeling analysis. Schwalm et al. used a drought index, evaporative fraction (EF). Secondly, carbon fluxes data in Schwalm et al. were processed through a couple of steps including deseasonalization and transforming into relative anomalies before being used for analysis. Our analysis was more straightforward. We directly compared fluxes in drought year and normal year, and calculated the drought effect (See method). In addition, we used relative changes, but Schwalm et al. used absolute changes per unit change in EF. Lastly, our conclusions were different. Using our datasets and method, we concluded that differential effects of drought on production and respiration occurred under moderate and severe drought and such differential effects were only found in grassland ecosystems (Newly updated figure 2b according to the first comment). Meantime, we proposed mechanisms for such differential effect in grassland ecosystems. However, we updated this reference in Line 73 to let the readers be noted of the paper by Schwalm et al..

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6) L243: “annul” to “annual”

Response:

It has been updated accordingly.

Reviewer:

7) L299: to suggest that soil carbon content is stable seems to be poorly worded. The idea is rather that there are small changes to a large pool, and that the flux acts on the large pool, so it is only until the small changes accumulate that the effect grows to its maximum. Basically, there is a lag in response to a semi-continuous forcing.

Response:

It has been updated accordingly.

Reviewer:

8) L296: this interpretation seems to miss the role of reduced photosynthate and associated exudates and/or reduced C inputs (litter).

Response:

It has been updated accordingly.

Reviewer:

9) L335: “...two [reduced-] rainfall treatments...”

Response:

It has been updated accordingly.

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Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C7066/2013/bgd-10-C7066-2013-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 10, 16043, 2013.