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Interactive comment on "Asymmetric Responses of Primary Productivity to Altered Precipitation Simulated by Ecosystem Models across Three Longterm Grassland Sites" *by* Donghai Wu et al.

Anonymous Referee #2

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The manuscript "Asymmetric Responses of Primary Productivity to Altered Precipitation Simulated by Ecosystem Models across Three Long- term Grassland Sites" presents a smart and well-thought out study to evaluate the performance of a large range of ecosystem models in their abilities to represent grassland productivity under changing climatic conditions. This study provides much needed insights in how ecosystem models perform when compared to field observations and highlights research needs to make such models more useful for climate change studies.

The abstract and introduction section is very nicely written and tightly structured. Unfortunately, I found that the result and discussion sections didn't follow this nice and

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logical structure.

Most of the results do not fully account/present uncertainty estimates. Some do, but it is often insufficiently explained what measure of uncertainty/variation is presented. This makes it at time difficult to appropriately evaluate the relevance of patterns found in the results.

I miss an explicit discussion of the potential discrepancies in spatial scale between observations and model simulations. These can be particularly relevant for often fine-scale heterogeneity in soil moisture dynamics. I also miss a discussion on the caveats of the specific approach that was used for precipitation manipulations (fixed percentage increase/decrease for each rainfall event). It is not clear that this is what is happening under climate change; and precipitation event size distribution has large impacts on soil moisture dynamics (e.g., Lauenroth, W.K. & Bradford, J.B. (2012) Ecohydrology of dry regions of the United States: water balance consequences of small precipitation events. Ecohydrology, 5, 46–53.

Specific comments:

Introduction

- page 3, lines 13-15: rephrase to make the assumption explicit that “adaptation of plant communities over long time scales” is adaptation to typical “water received from rainfall for growth” – and not just any amount of water

- page 3, lines 13-17: placing all citations at the end can be interpreted that all these citations only support point 2 and that there is no citation to support point 1

- page 3, lines 17-20: The argument why temporal relationships are more informative for climate change impacts studies than spatial ones is not clear to me. It seems that effects of climate change on ANPP have not only a temporal trend (as stated here), but also include changes in species (and their adaptations) due to migration/extinction when tracking climate – thus spatial patterns may also be relevant if chosen carefully

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to reflect projected climate differences.

Materials and Methods

- page 4, lines 19-26: I am surprised by the selection of the three study sites: two are located in the USA and represent naturally occurring grassland ecosystems where fire is an integral part whereas STU is located in Europe and is a man-made habitat that otherwise would be forested. These stark differences should at least be mentioned and caveats discussed.

- page 5, lines 24-25: Please provide some details on how the gap-filling was conducted and how much of the data were filled in – at least for precipitation. Various approaches can lead to considerable differences in precipitation values, e.g., seasonal biases in missing data.

- page 6, lines 20-25: * Why do you calculate the "median value of productivities in wet years with annual precipitation higher than the 90th percentile level" and don't simply take f(p95) = productivity value with annual precipitation at the 95th percentile? Aren't they the same? And equivalently for med(f(p10)) = f(p5)?

* It seems that AI simplifies to

** AI = (med(f(p90)) – mean(f)) / mean(f) – (mean(f) - med(f(p10)) / mean(f) # after inserting Rp and Rd and which simplifies to

** AI = (med(f(p90)) + med(f(p10))) / mean(f)

** AI = (f(p95) + f(p5)) / mean(f) # after inserting previous bullet point

* I don't understand why Rp and Rd are defined differently from each other and thus, AI is the sum instead of the difference between the 5%- and the 95%- quantiles. In most cases of somewhat symmetric distributions, f(p95) > mean(f) and mean(f) > f(p5) and thus AI > 0.

* Results presented for instance in Fig. 4 where AI < 0 and AI > 0 suggest that AI is

calculated correctly, as I suggest here, but that the equation is incorrectly written.

* What is meant with “f is the inter-annual productivity” (line 22)? Isn’t f simply equal to “annual productivity”?

Results

- The structure of the result subsections is unexpected. The research questions and methods are tightly structured around the estimation of parameters a and b of Eq. 1, of the asymmetry index AI, and of the sensitivity index S. The result section does not follow this layout. For instance, the first subsection 3.1 could be presented in terms of estimation (and uncertainty) of parameter a. Then, the subsection 3.2 contains really the results (with lacking uncertainty estimates) for parameter b – plus in its current form some results on CUE and ANPP/NPP which have not been motived/introduced so far (which is confusing). The topic of subsection 3.3 spatial/temporal relationships presents the results from the second objective (as listed in the last paragraph of the introduction section); however, the method section does not explain how the observations and simulated values were aggregated and compared to address this question.

- page 7, lines 23-24: I see little support in Fig. 1 for the claim of a “steeper curvature at STU despite saturation above ambient precipitation indicates a steeper decline of productivity for precipitation set below ambient for this site compared to KNZ and SGS (Fig. 1)” – the precipitation treatment at STU did not (or at most barely) cover the curved part of the fitted lines. In most cases, a horizontal line appears to have fitted the data better. The estimates of b remain imprecise for STU, but this uncertainty is unfortunately not quantified.

- page 8, line 11: How was the ensemble model result calculated? Is this the arithmetic mean, median, etc.?

- page 8, line 23: “median value of -0.12±0.11” – what does the error component “±0.11” represent? Is this the MAD?

- page 8, line 23: Why “proportionally” larger? I don’t understand what does could mean, particularly, because Rd and Rp are both calculated relative to mean(f).

- page 8, line 26: Why are the observed AI values presented without uncertainty estimate?

- Figure 4: The dots are too large relative to the figure; they are overlapping each other so much that it is really hard to see what is going on. For instance, the reported 0.1-“significance” with an unnamed test for STU seems dubious as the visible few dots huddle around 0.

- Figures S1 and S2: There are no error estimates for parameters a and b. At least add appropriate error bars to Figs S1-S2. I don’t understand why a and b are presented against each other in a scatter plot. In my understanding, there is no expectation of a relationship between a and b. This is confusing.

- Figures S3 to S6: There is too much on these panels. It is no longer possible to identify responses of individual models.

- Figure S4: Is it correct that the “P” responses represents Rp of Eq. 3 and that “D” represents Rd of Eq. 4. Make this clear and use consistent terminology throughout the manuscript.

- Figures S5 and S6: How to the absolute SWC values compare between observed and simulated?

- Figures S7 to S9: Error estimates are missing and would be crucial to compare between CN- and C-only models.

Discussion

- page 9, lines 21-29: The first paragraph of the discussion reads like an introduction paragraph that identifies the knowledge gaps.

- page 11, lines 25-27: Not clear what is meant here with “arid and semi-arid grasses

[. . .] show relatively strong resistance". Does this refer to varying abilities of grass species to extract soil moisture held at increasingly higher tensions? If this were fixed values in models across sites, then the simulations models may produce too high sensitivities at the drier sites, particularly SGS.

- page 12, lines 1-2: I am confused here: the text continues to discuss "asymmetric responses" and yet refers to Fig. 5 which presents results for the sensitivity index calculated as relative difference among different model runs. So, if this text does refer to result for S, then I don't understand the statement "responses for normal precipitation variability" either because S isn't calculated from "normal precipitation variability" (as are Rd and Rp), but from manipulated precipitation inputs. Thus, S seems to rather represent sensitivity to deviation from 'normal' precipitation.

Data availability - Why are (a relevant subset of) data not deposited in a repository such as Dryad or figshare?

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