Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-175-RC1, 2020

Interactive comment on "Contrasting responses of woody and herbaceous vegetation to altered rainfall characteristics in the Sahel" *by* Wim Verbruggen et al.

Anonymous Referee #1

Received and published: 16 July 2020

This study used a model to investigate the impact of high and low precipitation years on ecosystem carbon and water cycling in the Sahel region, Africa. Annual precipitation anomalies were simulated by changing rainfall intensity, event duration, or rainy season length and holding the remaining two variables constant. The resultant fluxes were then analyzed with respect to contributions from the three major regional plant functional types. In this framework, grasses demonstrated a flashier response to all precipitation scenarios than trees, and changing the length of the rainy season had the greatest impact on vegetation due to changes in runoff and surface evaporation associated with precipitation intensity and duration. No single-year precipitation treatments had a significant effect on the long-term (decadal) simulated carbon or water fluxes.

General comments: I thought this research was creative and well designed, and work from understudied regions like Africa is especially valuable. However, the stated research questions (L91-93) are left mostly unanswered by the study that presents data from only one site in its current form.

I don't understand or agree with the decision to exclude three of the four sites from the main manuscript text. I realize that these results are available in the supplement (without interpretation or discussion), but they're needed in the main text insofar as they're key to meaningful interpretation of your work and its scope. To me, the impact of this manuscript, and thus the suitability for the journal, depends on its applicability to other dryland systems. To establish this, (1) model performance must be evaluated at all sites and (2) systematic biases (or lack thereof) subsequently analyzed with respect to relevant climate/soil/vegetation, etc. characteristics, in order to generate transferable biogeoscientific insight.

Specific comments: L16-19: Are extremely high precipitation years also forecast for the Sahel? Only drought extremes are invoked on L17.

L20: "The rainy season" is confusing as you have yet to establish precipitation seasonality. Also "signature" could be more specific e.g., "the impact of the rainy season on the ecosystem carbon balance" or similar.

L24: Please clarify what's meant by "meteorological consistency" in this context. Reading ahead, I see it now, but if you wish to invoke this term/concept in the abstract, it should stand alone.

L29: Is it your intention to use "semi-arid" and "dryland" interchangeably? Maybe better to just choose one term and stick with it, at least in the abstract.

L30: After reading the entire manuscript, I disagree with the use of "long-term" here.

L54: And presumably in other dryland areas right? It's to your advantage to keep the results as broadly applicable as possible.

L71-72: Shouldn't mean annual precipitation (MAP) be less in arid areas than in semi-arid areas? Also 650 mm described as the cutoff for mesic (L45) but then 700 mm cited as a cutoff for "less arid" (L73). Please establish a clear precipitation classification scheme.

Table 1: Does "fallow bush" imply 0% tree cover at the Niger site?

L103: Most readers will be unfamiliar with this area and would benefit from a site map.

L108: I think including the measurement years for each of the flux tower sites would be warranted here or in Table 1. They're used to validate the model, so the length of the instrumental record is particularly relevant. Also it bothers me when eddy covariance measurements are simply presented as "the truth"; please indicate at minimum the mean uncertainty associated with the flux observations.

L140-141: This is unexpected given "soil control on surface water balance" (L28) and the different soil classifications in Table 1.

L158: How was the length (1 to 37 years) of any particular meteorological cycle determined?

L160: Just the rainy season or the entire year (as implied by the Figure 1 y-axis)? This may seem common knowledge to you, but most readers will be unfamiliar with the climatology of the Sahel. Maybe a "percent of MAP during the rainy season" metric could be added to Table 1?

Figure 1: I'm unsure how to interpret the frequency variable; would a value of 0.5 day-1 correspond to one rain event every two days? Perhaps an explanatory sentence could be added to the caption (or just a pointer to Table 3)? How was the length of the rainy season determined quantitatively? I'm more familiar with growing season length calculations that can greatly affect interpretation of results. Reading ahead I see a definition in terms of "climatological anomalous accumulation" in Table 3, does this imply a typical lack of any precipitation outside of the rainy season? If so, please

state clearly in the site description.

Table 4: It's taken me a while to interpret the "rainfall disturbance" column. I think it means the net change in rainy season precipitation due to perturbation of one of the three precipitation metrics. Perhaps change to "change in rainy season precipitation" or similar to clarify this? And/or list the relevant perturbation first and then the resulting effect second?

Figure 3: Can you speculate as to why there is a much larger difference between modeled and observed data at Dahra in 2010 compared to any other year? This could inform your other results (see general comments).

Figure 4: Many people (including myself) find Taylor diagrams difficult to interpret. Why not just show an analog to Figure 3 for all three sites? This would provide ecohydrological information about each site, as well as highlight periods of relative agreement and disagreement between the modeled and observed data i.e., my previous comment, which could yield additional insights. Why RMSE on the previous figure and RMSD here?

L231: I'm unclear as to how exactly +122% and +54% are meant to be interpreted. For example, is it that increased rainy season length stimulated the C4 grass LAI 122% more than both similarly increased frequency and intensity? Perhaps including some of the actual values would clarify this.

Figure 5: How to interpret (a-c)? Is it simply the aggregate of (d-f)? If so, what information is meant to be taken away from (a-c)? What about the other three sites? Given that this is a recurring framework, please treat this as a major comment.

L253-254: I see no mention of autotrophic respiration, do you mean GPP? How was it calculated? See Chapin et al. 2006 Reconciling Carbon-Cycle Concepts, Terminology, and Methods for relevant definitions.

L255: Excluding changes in respiration, how can it be less than 1x the reference NPP if there's additional photosynthesis?

Figure 6: Same comment as Figure 5.

L272-276: Suggest rearranging the text (or figure) to introduce figure panels in order.

L287: Four sites were introduced in the methods, but so far, we've only seen results from Dahra. This is confusing and requires explanation and justification for your rationale.

L290-292: This harkens back to my comment on L140-141, perhaps the effect of soil type/texture on the results is discussed in the next section...

L298-302: Right on cue, here's a partial response to my comment on L287. In short, I'm not satisfied with your treatment of the other sites. The vast majority of readers will not reference the SI, and even those that do must have model interpretation expertise to gain useful information. This functionally excludes three of the four sites from your analysis. Without these other sites, you're left with a site-specific tuned model that will be of little interest to other dryland or even Sahel researchers. To say it a different way, in my eyes, the strength of your work is the development of a transferrable model that could be used by dryland savanna researchers globally.

This doesn't have to be a fatal flaw as I think you have the data to support a broadly applicable presentation/contribution, they just have to be worked into the manuscript. Especially in light of my comments about the "reference" panels in Figures 5-7, I think you have the space for this. And it's okay if the model performance wasn't as good at the other sites (not saying this is the case), simply presenting the data and discussing potential reasons for periods of better and worse model/data agreement would speak to process-based information and benefit the research community.

L317: You've discussed both NPP and NEP, so the "total carbon flux" is unclear. Also fires emit carbon dioxide, so do you mean to convey that they reduce (not add to) the carbon flux by this amount?

L326-328: Suggest adding an initial discussion paragraph before section 4.1 that summarizes the principal results and why they matter. This is particularly important given the lack of a conclusion section.

355-356: Is there evidence for hydraulic redistribution in these Acacias? If so, the potential for impacts on grass-tree facilitation vs competition must be addressed.

367-368: This is an important result that could elevated to the abstract and/or first discussion paragraph.

L372: Not currently corroborated at all four sites.

L374-376: Recent work by Dannenberg et al. also showed negative asymmetry (with respect to precipitation variability) for trees in semiarid areas:

MP Dannenberg et al. 2019. Reduced tree growth in the semiarid United States due to asymmetric responses to intensifying precipitation extremes. Science Advances.

L389-392: Some kind of sensitivity analysis would go a long way toward reducing the uncertainty associated with this caveat e.g., a comparison of results from model runs where the texture was varied by increments of $\sim$5% within some permissible range.

L393: "Variations between the different sites" have not been shown.

L402-403: A citation would strengthen this claim.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-175, 2020.