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

Anonymous Referee #2

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The manuscript presents a modelling study quantifying the impact of rainfall amounts, as well as its intra-annual variability on semi-arid ecosystem responses, including gross and net ecosystem carbon fluxes, as well as water fluxes (evapotranspiration and runoff). Overall, the study is well put together, but some key clarifications are needed:

a) Model selection a1 - Model selection is of paramount importance in a modelling study as such. The authors should motivate why they opted for LPJ-GUESS. Semiarid ecosystems as the ones analysed here are very sensitive to plant water availability, and the vertical distribution of soil moisture within the root zone will play a key role. Why did the authors choose a model that does not explicitly resolve the Richards equations? A

large fraction of terrestrial ecosystem models does that. a2 – A better representation of the semi-arid vegetation was implemented by altering two traits, the SLA and wood density in pre-existing PFTs. However, a crucial "trait" in such ecosystems in drought deciduousness. Can the authors provide more information on how this was implemented? a3 – L116: The model resolves carbon dynamics, and allocation to carbon pools at the end of the year. As the authors look at intra-annual variability of vegetation responses, that might be problematic, as same year carbon dynamics, might be misrepresented. Also resolving carbon dynamics at the end of the year, might impact the ecosystem legacies. Can the authors further elaborate on this potential limitation?

b) Data b1 – Why did the authors opted for the ERA-Interim reanalysis data, and not for the more recent and better ERA5, which is also available at a much finer spatial resolution. b2- To my understanding, when pooling data from observed days for the synthetic time series, temporal autocorrelation is not conserved. Is that true? If yes what is the potential impact on the results (i.e. lack of strong correlation of temperature – i.e. long-lasting heatwaves etc).

c) Results c1 – To my understanding the model predicts species coexistence in all sites. Such co-existence, will most likely affect the decadal long legacies presented here. Is that in agreements with what is observed at each of the sites? Does s[ecies co-existence occur in reality? c2 – While legacies in drought responses have been widely observed, previous studies (e.g. Kolus et al., 2019, Scientific reports, doi:s41598-019-39373-1) have found that terrestrial ecosystem models underestimate them. Can the authors explain why in their results decade long-lasting legacies (typically longer than observed) occur? Is that primarily due to disturbances? c3 – I agree with reviewer 1 regarding the use of the Taylor diagrams. My main disagreement on their interpretation is that due to the high seasonality of the climate, most of the correlation comes from being able to reproduce the annual cycle, and not reflecting the performance of the model regarding rainfall structure. Possibly a Taylor diagram performed at e.g. seasonal anomalies would be more informative.

d) Presentation I fully agree with reviewer 1, regarding the choice of the authors to present one site and append in the supplementary the analysis of the remaining three. In fact, a detail comparison of the four sites would significantly strengthen the results and provide further mechanistic insights regarding ecosystem functioning.

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