Response to Anonymous Referee #2

We would like to thank both Anonymous Referees for their review and suggestions for improvements.

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:

Response: We thank the reviewer for her/his positive overall appreciation.

a) Model selection

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.

Response: The use of LPJ-GUESS for this study mainly follows from practical reasons, as there was already a great expertise of running and developing the model in our group. In addition, the model is known to give a reasonable representation of large-scale sensitivities to drought in drylands at the global scale, and for Africa specifically (Ahlstrom et al., 2015; Brandt et al., 2017, 2018). It is our ambition to use this model at a larger scale for upcoming studies. We will add this motivation more clearly in the introduction. As already written in the Discussion section, we agree that dryland model performance may benefit greatly from using a model which resolves soil hydrology and plant hydraulics with a higher degree of detail. Therefore we are currently parameterizing and running the latest version of the ED2 dynamic vegetation model (Longo et al., 2019; Xu et al., 2016) for these sites.

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 is drought deciduousness. Can the authors provide more information on how this was implemented?

Response: Phenology of the deciduous PFTs is based on a water stress scalar (ω) in the model. Low values of this scalar represent stress due to reduced soil water content, leading to a reduction of photosynthesis through stomatal closure. When this variable drops below a given threshold (ω_{min}), the dry season starts and deciduous trees will shed their leaves. Likewise, when this scalar rises above ω_{min} new leaves will be produced, taking into account a prescribed minimum dormancy period (Smith et al., 2014). This further elaboration on drought deciduousness will be added to the Methods section.

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?

Response: Yes, we will elaborate on this in the discussion section. A model version which includes daily carbon allocation for grasses has been developed for studying grass dynamics and grazing impacts within a year (Boke-Olén, 2017). Comparing the outcomes of both model versions could be an interesting topic for a follow-up paper.

b) Data

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.

Response: We used reanalysis data based on ERA-Interim for the temperature and incoming shortwave radiation data only (Weedon et al., 2014). However, the most important driver in drylands is precipitation, for which we used Multi-Source Weighted-Ensemble Precipitation (MSWEP) data, which performed best in two large-scale evaluations that included ERA5 data as well (Beck et al., 2017, 2019).

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).

Response: As discussed in section 2.5, loss of autocorrelation was avoided by restricting the data resampling to stay as close in time as possible to the original DOY, thereby retaining synoptic patterns as much as possible. As it was not always possible to find a day with an amount of rainfall that matched the goal, neighbouring pixels or years may be consulted. In the latter case, temporal autocorrelation will likely be lost. We will elaborate on this potential limitation in the discussion.

c) Results

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 species co-existence occur in reality?

Response: Yes, our sites consist of a sparse woody cover in a herbaceous matrix (Table 1). The model indeed predicts coexistence of plant functional types at all sites as well. This co-existence will indeed have an influence on our results through the competition for resources. We will elaborate further on this in the discussion.

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?

Response: The long legacies in our results originate mainly from the water content of the lower soil layer and the allocation of woody biomass. These pools respond relatively slow, compared to the timeframe of the simulated disturbances. Empirical observations of the disturbance impact legacies are lacking for drylands, so it is hard to evaluate our results against current data. Differences in model parameterization (i.e. dryland specific or not) and experimental setup may explain contrasts with earlier studies such as Kolus et al. (2019).

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.

Response: Agreed. We will add a second Taylor diagram which only evaluates the model during the growing season.

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.

Response: See the response to Anonymous Referee #1 on this issue. In short, we will add summarized results for the other sites to the main text, together with a more detailed comparison in the discussion.

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