Review of Paudel et al. Savanna ecosystem structure and productivity along a rainfall gradient: the role of competition and stress tolerance mediated by plant functional traits

This manuscript aims to disentangle the relative contributions of competition and abiotic stress in defining the distribution of plant strategies and productivity along a rainfall gradient in the northern Australian tropical transect. The authors use a trait-based DGVM to explore this. The model development performed, the methodology used to do this, and questions the authors aim to answer are important and relevant to Australian contexts (where fire vegetation interactions are of significant importance), global contexts, and of general interest to the readership of the present journal.

I like what the authors have done to re-parameterise their model for PFTs that are more appropriate to the Australian context using observed trait distributions.

I believe the MS has the potential to be published, however, I have a number of concerns/questions which need clarification.

First, the authors validate their model by comparing simulations done using their newly parameterised version of LPJ-GUESS to flux tower deriver GPP and evapotranspiration measurements. The ET comparisons (Fig. 2) look good. The GPP comparisons, with the exception of Stuart Plain, look ok. However, there is no control to compare the results of this new model parameterisation with. How should a reader assess whether the presented new version is an improvement compared to the standard version of LPJ-GUESS?

Second, the authors acknowledge that “Good model performance in terms of replicating compositional patterns along environmental gradients may then provide confirmation of assumptions as to the eco- evolutionary basis of plant traits as encoded in the model.” (line 61). Yet, the authors have not dedicated any formal analysis to assess whether their model does indeed perform well in terms of replicating the compositional patterns along the environmental gradient. I also find very little text which discusses matches or mismatches. It should be possible to add observed PFT lines to Figs. 4, 5, and 6 and provide a map of simulated vs observed PFTs.

Lastly, and most crucially for the current conclusions presented in the manuscript, LPJ-GUESS includes fire and its impacts on vegetation structure. Based on the simulation protocol it appears the BLAZE fire module was used for this study and fire was turned on for these simulations. The vegetation of this region has evolved with fire. Fire is a regular event in the study area. There is an extensive literature documenting this. How can the authors claim to have identified the mechanisms underlying the distribution of vegetation when they do not consider fire? How well does simulated fire (burnt area, fire return interval) match observations? Does fire and fire frequency, in combination with precipitation reductions, perhaps influence the change in the dominance of PFTs from tall eucalypts to C4 grasses along the gradient?

In summary, I believe model development which improves the representation and simulation of Australian vegetation is a noteworthy advancement as vegetation strongly interacts fire, particularly in an Australian context. Such development work is important and should be published. The authors demonstrate that their model development provides good simulation results compared with GPP and ET, contextualising this by displaying the performance of standard LPJ-GUESS would improve the manuscript. The authors should also provide better

evidence that their improved model version can adequately simulate the distribution of PFTs across the study region, a recategorization of the vegetation type map in Fig. 1 using the PFT classifications presented beside simulation output would probably suffice. Crucially, the authors need to include simulated fire effects in their analyses, without doing this they cannot reasonably claim to have identified "water availability as a primary driver of vegetation structure, composition and productivity" across the study area.

Minor comments

- Line57: remove double full stop.
- Line 286: Where do SSR and SST come from? I don't see them in the preceding text. What were they used for? Perhaps link these to the calc for R2 or leave them out.
- Tables 1 and S1 are the same? Why duplicate them?
- Table S1 and corresponding text. Please use consistent terminology. Aren't the parameters for your PFTs your traits? Isn't this what makes it a trait-based approach? Though all DGVMs always had trait values that defined the behaviour of PFTs. Traits, not parameters, mentioned in the title. For example, in your text you call all of the traits as paramaters "The parameters included leaf phenology (evergreen or rain green), leaf longevity, wood density, specific leaf area, shade tolerance, leaf turnover rate (calculated as 1/leaf longevity), and the ratio of leaf area to sapwood cross-section area (k_latosa)."
- Supplement "Traits and parameter values". Please specify the "various sources" used to collect trait data and provide references.
- The PFT cluster names (Fig. S.1), PFT names (Figs. 5, 6, Tab. 1, Tab. S.1) don't match the PFT names used in Figs. S2, S3, S4. I can only guess which pft matches which pft.