**Editor comment**
The authors have addressed the previous reviewer comments in a thorough manner and subject to some very minor further suggestions I will be happy to see this work published.

Dear Roslyn Henry and Reviewers,

thank you very much for your feedback on our manuscript. It has improved greatly as a result. Below, we respond to each of the reviewers' points.

Best regards,
Felix Nößler on behalf of all co-authors

**Reviewer #1**
The manuscript has been substantially improved in response to previous reviewer and editor comments. The authors have made a sincere and commendable effort to address methodological, structural and conceptual concerns, leading to a more focused and rigorous model description and evaluation paper.

This reviewer is satisfied with the revisions for several reasons: model validation is more visible and supported by a broader dataset; the authors openly acknowledge limitations and provide clear reasoning for their model design; while not deeply detailed, the practical context is now present; the decision to defer a quantitative comparison as outside the scope was appropriate; the local sensitivity work adds needed depth, even without global uncertainty quantification; model limitations are discussed honestly; the main text's readability has improved; potential directions for improvement are outlined.

Thank you that our revision was so well received!

However, following the editor's guidance, the removal of the simple illustrative case study makes the manuscript more abstract, even as it clarifies the paper's scope and sharpens its focus on model formulation, calibration and validation, thereby enhancing conceptual clarity. This reviewer recommends that the authors explicitly emphasise the necessity of future research applying the model to specific land management contexts or scenarios.

We agree with the reviewer and added a sentence to the conclusions (L. 712–714): "Further simulation studies, for example, the analysis of different land use scenarios, are required to fully explore the potential of the GrasslandTraitSim.jl model."

In addition, one important concern appears to remain only partially addressed. The authors have replaced a simpler light competition scheme with a more mechanistic height-weighted formulation ("We advocate for using the more complex method … removed the simple method ... to keep the manuscript concise"), citing several prior modelling studies as precedent: "This concept was widely adopted in grassland modelling studies (Schapendonk et al. 1998; Jouven et al. 2006; Moulin et al. 2021; for a review see Pei et al. 2022)". However, they do not appear to provide empirical justification for this formulation or demonstrate that it accurately reflects observed competitive dynamics in grassland systems. Adding a brief paragraph referencing field-based studies that support height as a key determinant of light competition would improve this part. For example, Falster and Westoby (2003) discuss the evolutionary implications of height for light access (https://doi.org/10.1016/S0169-5347(03)00061-2), and Gough et al. (2012) show how height influences species responses to nutrient addition in grasslands (https://doi.org/10.1007/s00442-012-2264-5). Referencing such empirical work would justify the model's assumptions, strengthening its ecological grounding. At present, the omission of this evidence weakens the overall justification for a key component of the model's structure.

Thanks a lot for the remark. You pointed to two interesting empirical studies about light competition. We thoroughly updated our section on light competition and added more literature, including the two mentioned studies (L. 222–233).

We fully understand that citing other modelling studies that used the same approach does not provide a sufficient justification for using this approach. However, you referenced one section of our manuscript ("This concept was widely adopted in grassland modelling studies") that was about the total growth of the community, which is mainly based on the total leaf area index and the photosynthetic active radiation. In this paragraph, we also cited Monteith (1972), who compared the method for estimating total growth extensively with empirical data of different crops. That is why we did not change something in this paragraph.

In conclusion, this manuscript now presents a valuable and well-articulated contribution to trait-based ecological modelling. Incorporating a brief empirical rationale for the light competition formulation would further strengthen its justification, ensuring the manuscript is fully ready for publication. This reviewer recommends acceptance pending these minor revisions.

Thank you for this positive feedback!

**Reviewer #2**

The authors put much effort into the revision and addressed all my previous comments. I don't have any major comments. I just recommend to check the language again and revise statements such as "traits that help plants" (l 267, I think the traits don't help plants but represent strategies with high nutrient uptake, for example), "poor at taking up water" (l 331, unspecific), "at the fast end of the leaf economic spectrum (l 411, unclear), "these groups are about the radiation use efficiency (l 560, not clear to me what that means, groups are related to ...?).

Thank you for the overall feedback on our revision! We revised all the mentioned statements and checked the language in the whole manuscript.

We clarified the following statement that the root trait expression represent different strategies in nutrient uptake (L. 272–276):
"We assume that the arbuscular mycorrhizal colonisation rate (Marschner and Dell, 1994; George et al., 1995; Van Der Heijden et al., 2015) and the root surface area per total biomass (Barber and Silberbush, 1984) represent strategies in the nutrient uptake. High values of these traits lead to increased nutrient uptake rates and, consequently, reduced nutrient stress."

We revised the statement on the water uptake efficiency of plants (L. 340–341):
“Plant growth may be restricted under conditions of low soil water content, particularly if the plants exhibit a limited water uptake efficiency.”

We clarified the statement about the leaf economic spectrum (L. 419–424):
“We linked the senescence rate to the specific leaf area in order to represent the underlying trade-off in the leaf economic spectrum. Plants that employ the 'fast strategy' of the spectrum are highly photosynthetically efficient. They are modelled here with a higher leaf area index per unit of biomass, which is influenced by the specific leaf area (Eq. 10). However, species with a high specific leaf area have a short leaf lifespan and therefore a high senescence rate (Eq. 34). Conversely, plants representing the 'slow strategy' of the spectrum exhibit the opposite characteristics (Reich et al., 1992; Wright et al., 2004; Onoda et al., 2017).”

We reformulated the description of the local sensitivity analysis as follows (L. 569–573):
“We conducted a local sensitivity analysis to identify the parameters to which the above-ground biomass of Lolium perenne is most sensitive (see Table A12 for details). The analysis revealed that the most sensitive parameters were those relating to the processes of radiation use efficiency (γRUEmax, γRUEk, γRAD1 and αRUEcwmH), seasonal adjustment for growth (ζSEA min and ζSEA max) and senescence (βSENsla and αSEN), indicating that small variations in these parameter values lead to substantial changes in the above-ground biomass of Lolium perenne.”

l 537: initial height, shoudn't this be 0.4 m?

Thanks for catching the mistake – yes, the unit of the initial height is metres.

Fig 7: as stated in the text, it is difficult to evaluate the time series as data are provided only at an annual scale such that the variability between those data points remains unclear. One option to assess variability could be to look at remote sensing products such as NDVI for the sites. Even though this is not AGB, it may still provide some insights on variability. I don't suggest to do it in this study, but maybe it could be mentioned or tried in a follow up study.

Thank you for your comment and suggestion! Combining remote sensing products such as the NDVI with local measurements could indeed be a valuable addition to the calibration and validation of the model in a future study.