General evaluation of the research paper

The paper presented by the authors addresses a very relevant and important topic in the field of DGVM model development. For far too long, the representation of grasses and the herbaceous layer have been given far too little focus in most DGVMs with respect to structural and functional diversity. Only recently, development of more detailed grass layer representations in DGVMs are starting to emerge but compared to tree-layer representation this work is still at a comparatively early stage of development. Grassland ecosystems and savannas cover a substantial fraction of the land surface and provide important ecosystem functions and services to a multitude of people while simultaneously being threatened by the effects of climate change and resource over-exploitation. Therefore, developing vegetation models that are capable of representing within-grass layer dynamics, diversity and processes is crucial to assess the impact of different management strategies and environmental change. I therefore deem the paper a relevant and important scientific contribution.

The CSR theory is a widely known concept and therefore a valid approach to implement functional diversity and trade-offs within the herbaceous layer of the model. One may question whether the implementation in its current form using a Bayesian calibration method to parameterize the new PFTs for three specific sites can be generalized for large-scale application, but in the given context of the study, the approach seems sound and justified to me. The shown results in many cases match ecological expectations and improve results compared to the old model version, further corroborating the chosen approach.

The paper is well-written and clearly structured. I therefore recommend publication pending minor revisions and clarifications detailed below.

We cordially thank the reviewer for their thorough and constructive feedback as well as the positive evaluation of our manuscript.

Below we provide a response to all detailed comments including proposals to achieve the suggested improvements.

Detailed comments

Introduction:

line(s) 36/37: You might also add the role of atmospheric CO_2-concentration. CO_2-fertilization effects can shift the competitive balance in grassland communities in locations where both C3 and C4 grasses are present.

We agree, even though we do not look into the effects of changing CO2 concentrations it should be a part of this overview and we will add a brief description of its role.

line(s) 42: “high temperatures can lead to an increase of microbial decomposition”. Only in combination with sufficient moisture. In arid regions, decomposition comes more or less to a stand-still during the dry season due to the water limitation that affects the microbial community. Rains at the beginning of the wet season then lead to peak emissions when microbial decomposition picks up again.

We will add a phrase to highlight that moisture is a necessary condition independent of temperature.

line(s) 44/45 "...may be beneficial for grassland productivity depending on its intensity". Maybe add: “by removing moribund plant material and triggering growth (over-)compensation.”

We will add the phrase the reviewer suggested.

line(s) 49: “for the species” – “for the functional types”. I’d rather consistently keep the focus on functional types.

We agree and will make the amendments throughout the manuscript.

line(s) 52: “indirectly through alterations of the resource limitations” – add: “…that can cause shifts in the competitive balance between functional types”.

We will add the phrase suggested by the reviewer.

Methods

line(s) 105: “hot-steppe pasture in South Africa”: this is a somehow unusual terminology / vegetation classification. The Syferkuil site usually is referred to as savanna rangeland in other publications.

The terminology for the naming of all sites was derived from the Koeppen Geiger climate zones (in this case hot steppe). At the first mention we decided to add the form of grassland management (pasture). We therefore will keep the naming as is but will add a phrase pointing towards the term savanna rangeland in L105.

line(s) 107/108: That means no tests of fertilizer X defoliation intensity combinations? That could be another interesting experiment to add, at least for the simulations.

Thank you for this interesting suggestion. In this part of the manuscript, we only mention the managements for which experimental data were available and that could therefore be used to parameterize the sites. Not knowing experiments including fertilizer X defoliation combinations, we would be grateful for information and very interested to include such data and combinations in further studies. The additional scenarios are described in 2.5. With this separation we distinguish between the scenarios that were predefined by the data and those we selected for further analysis. When defining the scenarios for further analysis, we decided to use extreme cases to test the effect of different limiting resources (e.g. infinite nutrient availability) instead of choosing different fertilizer levels. Regarding the defoliation intensity, we agree that analyzing a gradient of different intensities provides another interesting experiment. However, we decided to put our main focus on the resources and believe that the defoliation intensities of the experiment already cover a sufficient range.

line(s) 115/116: Are the trait values you use to describe the strategies from within a continuous range, or discrete fixed values? For example, if you use SLA as a trait to distinguish between acquisitive and conservative strategies, then you will automatically cover the extremes as well as in-betweens if you allow SLA to be a continuous trait that can range between a minimum and maximum value (see, e.g., Scheiter et al., 2013, Langan et al., 2017).

The reviewer raises a very interesting point. LPJmL-CSR follows the concept of using a small number of PFTs with fixed parameters. Therefore, for example SLA is fixed and each PFT only covers one point of the continuum. We also see the potential for interesting future work following an individual based approach drawing trait values from a continuum similar to LPJ-FIT (Sakschewski et al., 2015) or aDGVM2 (Scheiter et al., 2013). However, the currently implemented management routines of such models are less detailed compared to “classic” DGVMs that include an agricultural component. We therefore see the necessity to continue to improve grassland representation in both model types for the foreseeable future.

line(s) 120 “Overview of managed grasslands in LPJmL” – “Overview of managed grassland representations in LPJmL” seems a more fitting title for this section.

We will adopt the recommendation of the reviewer.

line(s) 123/124: one polar, one temperate and one tropical grass: C4-type photosynthesis for the tropical grass? Knowing classic LPJ, I deem it likely that this is the case, but good to mention explicitly.

We thank the reviewer for pointing this out. Indeed the tropical grass is a C4-type and we accept their proposal to explicitly mention this here.

line(s) 130/131: (no water limitation, ref). – forgot to add the actual reference here.

We will add the reference to Jägermeyr et al., (2015) describing the water management routines.

Table 1: Forage supply [MgDM ha-1]: Terminology not entirely clear: Peak standing biomass? Annual withdrawal quantity (through mowing / grazing)? What is the temporal reference frame – annual?

We will change the unit to MgDM ha-1 yr-1 and add a phrase defining forage supply as annual quantity removed through defoliation from mowing or grazing.

line(s) 166-168: Does this new scheme also account for root biomass distribution in different soil layers, and therefore varying water availability between different soil layers? So that the total water uptake is the biomass-weighted uptake sum across soil layers? Or is it simpler than that?

We thank the reviewer for pointing out that this could be described more clearly. Root distribution between different soil layers was already used to determine the water supply from the different layers in the previous model version (Schaphoff et al., 2018). Our scheme retains this approach and only distributes the sum over the supply from all soil layers based on the root biomass. We will include this in the explanation of our approach.

line(s) 186: I suppose that means that SLA as a trait is a PFT-specific constant? I.e., it cannot vary over the lifetime of individual, or between different individuals of the same PFT?

Yes, it is a constant but as stated in our reply to a previous comment (L78-85), we agree with the reviewer that there is great potential in exploring the entire continuum.

line(s) 191/192: Does LPJmL distinguish between forbs and grasses, and if so, how is this implemented? And for grasses: does it distinguish between C3 and C4 photosynthetic pathway? Is age-mortality the only reason for mortality, or are there other causes implemented as well (e.g., due to negative annual C-balance, due to water stress, due to fire, etc.)?

LPJmL does not distinguish between forbs and grasses and the herbaceous PFTs can include both. C3 and C4 photosynthetic pathways are distinguished and we will add a description in the methods section. In addition to age mortality, the model checks if a PFTs overall root or leaf biomass becomes negative and kills the respective PFTs. Excessive water stress from prolonged drought may be a cause of this. However, additional causes of mortality from water stress such as embolism (Jacobsen et al., 2019) as well as heat stress are not included. Fire on managed grassland has been implemented both as a disturbance (unpublished) and a management practice (Brunel et al., 2021) but is not considered here. We will extend the section on mortality to provide this additional information.

line(s) 193: “a biomass increase of the average individual dependent on the available area” – rephrase? “the area-specific biomass increase of the average individual”

Using “area-specific” as suggested by the reviewer is in our opinion less explicit since it does not define which area. We propose instead to replace “available area” with bare ground area.

section 2.3.3: general question on mortality: does the model distinguish between annual and perennial herbaceous PFTs? I.e., do you have a PFT with enforced death after one growing season? Enforcing annual types should implicitly strongly select for fast resource acquisition at the expense of durable structural components, and a strong focus on reproductive performance (see, e.g., Pfeiffer et al., 2019).

Currently, LPJmL does not explicitly distinguish perennial and annual PFTs and death is not enforced at any time. Implicitly, the establishment as well as the mortality rate control the life cycle of the PFT. High establishment and mortality rates lead to a fast turnover of the population. We see potential in explicitly distinguishing annual and perennial PFTs for example through constraining the period of establishment for annuals to the growing season. We will pick up on this in the discussion.

line(s) 197: “we retained the approach of establishing saplings instead of seeds” – I assume that refers to the tree PFTs? A bit unusual to refer to establishing grasses or herbs as “saplings”. I assume that you must have excluded tree PFTs from the simulations of the grassland sites, allowing grasses/forbs only? Otherwise, it is likely that a forest type or savanna type would have established as potential natural vegetation at least at the German and South African sites. You should add the information of how you handled the tree component of the model in the section where you describe your simulation protocol. Also clarify how establishment is done specifically for the grasses / herbaceous layer.

Indeed, only herbaceous PFTs are allowed to establish on managed grassland stands. We will add this to the model description. We agree with the reviewer that the term sapling is misleading in this context and will replace it with the term seedling throughout the manuscript. In addition, since this may create some confusion regarding the sapling LAI parameter, for which we have to keep the term, we will explain the origin of the parameter name and its purpose.

line(s) 199/200: So just to make clear that I understand correctly: the average individuals are clones, i.e., all of the same PFT, but you introduced the clone-concept to be able to account for PFT-specific reproduction aspects, such as seed numbers, germination rates, and seedling survival probability? If so, you should make it clearer than it is currently. It goes in the direction of the problems faced by models that simulate actual, true individual plants and their reproduction and establishment.

The reviewer raises an important point here. Indeed, the concept of the average individual should be explained in more detail to prevent confusion with individual based approaches. We will add a section in the methods explaining that each PFT can be seen as a representative for a population with certain attributes that describe the population (e.g. number of average individuals, individual biomass). In addition, we will discuss our approach in comparison to an individual based approach to show advantages and disadvantages.

line(s) 203: age-dependent mortality: hard set (at a specific age), or based on an age-dependent likelihood? And: the age-dependency differs between the different strategy types?

Thank you for this comment. Actually neither is the case. Depending on the growth efficiency, the number of average individuals is reduced (Appendix A3 L745-753). Actual mortality is derived from the maximum mortality rate - which is the same for all strategy types - and the growth efficiency. The growth efficiency is dependent on SLA, which differs between the strategy types (Appendix A3 Eq. A10). We will extend the description in Appendix A3.

And what is the allowed maximum number of average individuals, and the maximum number of grass-layer PFTs that can now coexist within one grid cell?

We thank the reviewer for this question. It made us realize that we did not include this in Appendix A3. There is no hard maximum number of individuals. However, if the total number of individuals exceeds 250 /ind/m2, 5% of the individuals die. We will add a qualitative description in the method section and update the equations and explain the underlying reasoning in Appendix A3. The number of PFTs per grid cell is in theory not limited, however we decided to use one PFT for each main strategy for the purpose of this study. For future studies this number can be increased, however this will also increase the computation requirements. We will mention this in the model description.

line(s) 205/206: “It can be assumed that few individuals that maintain a high cover and biomass must be larger…” – I assume all individuals that are part of one PFT have the same size and biomass, given that you are still using the average individual concept? So, adding new young individuals will lower the size and decrease the age of all clone individuals within the PFT due to the averaging. But this implies that a strongly reproduction-oriented PFT strategy would automatically have a smaller average individual size, a young average age, and a larger number of clone individuals representing the PFT. This has implications for the age-dependent mortality, as highly reproductive strategy types are then less likely to reach the age where age-dependent mortality hits. Did you consider this aspect?

The reviewer raises an important point. We do not simulate the age of the average individual. Our implementation of mortality depends on the growth efficiency. This describes the change in carbon from photosynthesis and turnover per average individual compared to the average individual carbon pools. In this ratio, the number of average individuals cancels out and the key aspect is the GPP to turnover ratio, which should be smaller in older populations leading to a higher mortality. We will also include this explanation in the method section on the mortality.

Table 2: Maybe add a column that specifies the predominant gradient associated with the parameter. You mention it in the text of this section, but it would be helpful to also have it as a brief overview in the table. I find the distinction between biotic and abiotic dimension a bit arbitrary/confusing with respect of the definition. Referring directly to the respective gradient (stress gradient for biotic, disturbance gradient for abiotic) would seem more intuitive for me.

We will abandon the terminology abiotic and biotic gradient. When writing the original draft, we found that it provides a clear distinction between the parameters related to each gradient. However, as the reviewer correctly noted, this creates an additional layer of terminology to understand when reading the manuscript. For this purpose we will modify the manuscript to follow the terminology stress and disturbance gradient as proposed by the reviewer and add a column to the table.

Table 2: Hierarchy: How did you determine the hierarchy? Based on your expert assessment?

We will add a phrase stating that the qualitative hierarchy of the parameter values for each PFT was derived from expert assessment by all co-authors.

Table 2: Light extinction coefficient: Independent from SLA, or correlated? High-SLA leaves should have more transmission than low-SLA leaves.

We agree with the reviewer that transmissivity of single leaves and their SLA are correlated. However, we had to deal with the challenge that LPJml does not simulate multiple leaf layers and cannot distinguish between the transmission of single leaves and the entire vegetation layer. To account for the difference between leaf and entire vegetation transmission at least implicitly, here the light extinction coefficient is not a measure of the transmissivity of a single leaf. Instead it is the transmissivity of the entire vegetation layer of a PFT. Therefore, we assume that PFTs, which have a high SLA can still have a high light extinction if many high transmissivity leaves are stacked. In the current version of the manuscript this is only touched upon in the discussion (L663-666). We will describe this in more detail in the methods section.

Table 2: Maximum transpiration unit [mm] – if this is to be a rate, then the time part of the unit is missing. [mm/day]?

We will change the unit to [mm d^{-1}]

line(s) 237/238: The root efficiency coefficient does affect the competitiveness between plants (biotic interaction), but it also relates to the stress gradient (abiotic) with respect to water uptake capacity. This is an example illustrating why using “biotic” and “abiotic” as dimensions is maybe not the best way to make the distinction.

We agree that there are cases were the distinction between biotic and abiotic is not so clear. As already proposed earlier (reply in L209-214) we will abandon the terms and only retain the terms stress and disturbance gradient.

line(s) 240/241: The light extinction coefficient describes the fraction of light intercepted by each additional leaf layer, right? As the amount of light that can transmit a leaf layer depends on the thickness of the leaf, one would expect kbeer to be correlated with SLA, which, unlike kbeer, you define as abiotic dimension. It would be good if you sort this out more clearly.

We agree and refer to our proposal from the related comment in the reply in L220-229. We will also describe more clearly, which parameters play a role for the stress or the disturbance gradient or for both gradients.

line(s) 241/242: the leaf area index of a sapling represents the offspring size - What do you define as "offspring size"? The height of the offspring, or its starting biomass, or its projected foliar coverage? I'm not sure LAIsap is a good description of offspring size, as its meaning is rather vague without a clearer definition. Whether a seedling/sapling of given leaf biomass has a high or low LAI is a function of its SLA, so LAIsap for a given unit of leaf biomass essentially is nothing else as another way to refer to SLA.

In LPJmL, the leaf area index of a sapling is only used to calculate the sapling biomass using SLA. So instead of assuming a given leaf biomass, we assume a given SLA and calculate the leaf biomass. Using the same SLA, a higher sapling LAI is equal to a higher sapling biomass. We will change offspring size to offspring biomass and add an explanation of the relationship to SLA. We will also revise the discussion to reflect both SLA and sapling LAI when discussing offspring biomass.

Table 3: Flip order of columns “variable” and “site”, as site is unique and variable is tied to site and non-unique.

We agree and will apply the proposed change.

line(s) 287/288: “the current representation of some processes within the model” – which processes specifically?

We here refer to section 4.1.2 where these processes are listed. We will change “some processes within the model” to “the processes, listed in sect. 4.1.2,” and remove the reference to section 4.1.2 at the end of the sentence.

line(s) 299: 390 years - your spin-up duration? Did you add a transient simulation period after the spin-up (how long? For what time-period?). One can only guess based on the time-axis labeling in the figures that follow in the results section. Please specify this with some more detail.

We agree that additional information is needed. We first conducted a potential natural vegetation spin-up simulation of 30000 years followed by a spin-up including land use of 390 years after which the transient simulation start. We will add this to the modelling protocol section.

Modelling protocol: What is the temporal resolution the CSR-model version runs on? Monthly, or daily?

All processes are executed on a daily time scale. We also compute the outputs on a daily timescale but aggregate to a monthly or annual resolution for some of the results. We will add a sentence on this to the modelling protocol.

How do you initialize community composition with respect to present PFTs and shares of PFTs at the beginning of the simulation? Based on the field-based observations? If so, how would you do it in a situation where you did not know the field situation of sites, e.g., for a large-scale or global simulation? (Question for the discussion, I guess).

Upon initialization, each PFT is established dependent on the respective establishment rate and biomass (derived from sapling LAI, SLA and leaf to root ratio). Therefore, initially a PFT with high values in both has a higher share in the community. However, if its strategy is not suitable this will change over time. This means, that no data on initial community composition or similar is needed. We will add this explanation to the model description.

Results

Figure 1: Please specify temporal reference frame for panels a, d, and g - is it the annual sum (yield), the peak season leaf biomass (leaf biomass), the grazing period duration offtake (grazing offtake)?

We will add a more thorough explanation in the caption.

General question on all scenarios that included animal grazing: Is preferential grazing, i.e., selection of more palatable over less palatable PFTs, accounted for by the new CSR model version? Unlike mowing or biomass removal by fire that is indiscriminate, biomass removal by herbivores can alter community composition quite substantially, especially under high grazing pressure. If preferential grazing is not yet implemented, this should be added as a limitation in the respective section of the discussion, and could be pointed out as a future need for development.

The reviewer raises an important point. Indeed the current implementation (Rolinski et al., 2018) does not consider preferences for specific PFTs. We will briefly mention this when describing the model and add it to the section on future need for development.

line(s) 365-368: Ecologically, the shift towards more investment into above-ground biomass (growth (over-)compensation) and towards a more resource-exploitative strategy (construction of “cheaper” leaves with reduced life duration is plausible. However, I do not see right away why the minimum canopy conductance should decrease due to grazing?

We agree that the decrease of the minimum canopy conductance is unlikely to be related to grazing directly. More likely, the high and similar minimum canopy conductance of the ungrazed scenario (C0) is an artefact of the parameterization. All parameters can be assigned primary and secondary processes that they affect. The leaf to root ratio and the SLA are different in the two scenarios and act as a compensation of defoliation from grazing (primary process). However, to some extent these parameters also control access to and distribution of resources (secondary processes). In the ungrazed scenario, these do not need to be adjusted to compensate for the defoliation but can still play a role in the competition for water. Therefore, more parameters can control resource access and distribution and it is likely that this will affect the parameterisation of minimum canopy conductance. We will amend the description of the parameters to account for the primary and secondary processes affected and add the explanation to the discussion.

line(s) 406/407: How does the relative contribution of the S- and R-PFT to the forage supply compare to their relative abundance or relative contribution to FPC? I.e., did they contribute more or less than could be expected according to their relative abundance within the community?

Thank you for the interesting question. We did not look into this in detail. Since biomass is an important variable when calculating FPC, we believe it is likely that forage supply and growing season FPC are similar. However, there might be differences when averaging over the entire year. We will analyze our results regarding this and amend the manuscript if we discover additional interesting results.

line(s) 442/443: “In the irrigated scenario, only the S-PFT contributed to forage supply.” - That is a bit surprising? One would expect that irrigation reduces stress resulting from water limitation, therefore opening the community more strongly for the C-PFT.

This was also surprising and counterintuitive to us. We already provide an explanation in the discussion in L579-583, which we will now reference in the sentence in L442f.

line(s) 473/474: “…still dominated by the S-PFT.” - Is this a legacy effect from the pre-irrigation time period's community composition? If run long enough without resource limitation (i.e., with irrigation on), would the S-PFT type be replaced by the C-PFT type, and if yes, how long do you expect this would take? Can be part of the discussion, if not already discussed there.

We already touch upon this in L542-547 but agree that this can be discussed in more detail. We will add a reference in L473f and extend the discussion in section 4.1.2.

Discussion

General remark: how do you intend to use the CSR-model in the future, if you ideally need an a-priori determination of the ideal PFT parameterization depending on site, community, and management? And how can communities respond to changing management or environmental conditions if the parameterization of the PFTs cannot be dynamically adjusted during the simulation based on a selection mechanim that filters for the best-suited parameterization under the given circumstances?

The reviewer raises several interesting questions that go beyond this study. We are currently working on a globally applicable set of PFTs, which will form the basis of another study in the near future. For that study, we retain the fixed PFT parameterization of classic DGVMs. However, we are generally open and very much interested in further developing the model. As already mentioned in the reply in L78-85, it would be very interesting to combine the approach of LPJmL-CSR and aDGVM2 or LPJ-FIT.

line(s) 494/495: “IN LPJmL-CSR, growth of the vegetation was faster than in LPJmL 5.2, which led to higher yields for all cuts.” – Elaborate briefly on the causes for the faster growth in the new model version.

The faster growth compared to LPJmL 5 has two reasons: First, the new implementation of biological nitrogen fixation led to less nitrogen stress and higher photosynthesis. Second, this is also a result of the new parameterization, which was tailored to this site. We will add this explanation after L494f.

line(s) 504: “but selected a livestock density of 1.0 cows ha-1” – use “livestock units” rather than cows (how about steers, heifers, etc.); And: Is this to determine the amount of manure input? The temperate grassland was not grazed but mowed, so livestock density does not make much sense with respect to grazing off-take?

The livestock density refers only to the spin-up and the historical periods for which no data on actual land use were available. Therefore, it is entirely unrelated to the transient simulations that reproduce the mowing experiments. We will rephrase this paragraph to make this clear.

line(s) 506: Briefly describe the processes / mechanisms that lead to increased carbon input to the soil in the CSR-version compared to the old version.

We identified three causes for the increased carbon input: First, the SLA longevity trade-off we implemented led to an increase in turnover supplying more carbon to the litter layer. Second, implementing explicit mortality of average individuals created an additional input into the litter layer. Third, accounting for the carbon added through the application of manure fertilizer also constituted an additional carbon input into the system. We will add this explanation after L506f.

line(s) 526/527: Here finally the information that I was missing in the methods section. You should add this information to the modeling protocol section (that you did exclude the tree PFTs from your site-scale simulations.

We will adopt this suggestion (see also reply in L152-157).

line(s) 528/529: You should try to give a reason for the "why" of this, instead of simply repeating the result. For example, an explanation could be that grazing was not the only / the main stress for herbaceous vegetation at this savanna site. The site has a pronounced dry-vs-wet season dynamics, and therefore water limitation as a stress factor, maybe also N-limitation, may be causes for the dominance of the S-type irrespective of the grazing management.

We agree with the reviewer that this should be explained and share their opinion of the underlying reasons. We will add a sentence to explain the dry wet dynamics of the site and that these are independent of grazing, which therefore does not affect the water stress level allowing the S-PFT to remain advantageous.

line(s) 540/541: You could test this by specifically allowing no other PFT than the S-type to enforce a monoculture.

We discussed the possibility to investigate this further, but decided against because LPJmL would limit us to simulating an S-PFT monoculture already before the beginning of the irrigation, which would likely lead to different initial conditions when starting irrigation. This would make it difficult to interpret the results.

line(s) 544/545: Was your simulation time period with irrigation long enough to allow establishment of a new steady state with respect to community composition? In my experience, community composition shifts are one of the slower processes and can take quite a number of years before reaching a new steady-state after a change in forcing has occurred.

We touch upon this in section 3.4.2 L475f by saying that “the transition occurred within the first one to two years”, which is much faster than we would expect. We mention this when discussing the change in soil organic carbon (L532-538) but we agree that this is very brief and will add more detail and highlight the transition time more prominently. We will also provide an explanation for the fast transition, which is related to the removal of competition for water. In a water scarce environment, the S-PFT as a water saver was advantageous and the C- and R-PFT were subordinate. Under irrigation, the S-PFT's slow growth becomes a disadvantage and the C- and R-PFT can exploit resources more efficiently. Both increase their biomass rapidly until a different limitation prevents further increase, while the biomass of the S-PFT remains similar. This is comparable to real world ecosystems. However, existing individuals cannot grow infinitely and need to reproduce producing new individuals. This process of reproduction and dispersal may slow down the transition. In LPJmL, the PFTs increase their biomass independent from the establishment of additional individuals which speeds up the transition. We will add this explanation to the methods and discussion sections.

line(s) 545/546: "However, periods of drought can induce and additional disturbance." – Correct, but not in this case, because due to the irrigation you had drought eliminated.

The reviewer is correct. A plausible explanation is that the parameterization allows the R-PFT to coexist with the C-PFT if the main resource limitation is removed. We will update the explanation.

line(s) 549: "LPJmL 5.3 underestimated the observed forage supply…" – I'm not sure about your usage of the term "forage supply" (generally throughout the manuscript) - is forage supply, according to your definition, the potentially available biomass offered by the rangeland, or do you actually rather mean "the amount of feed required by the animals" (which should then be termed as "forage demand"?

We agree that our use of forage supply was ambiguous because we use it to define the amount of biomass removed through mowing or grazing for the temperate grassland and the cold steppe but also for the amount of leaf biomass available for grazing for the hot steppe. This was an attempt to use common terms for all sites, which appears to be confusing instead of helpful. We will therefore change the term forage supply to forage offtake for the temperate grassland and the cold steppe and use the term leaf biomass for the hot steppe. We will add a definition of forage offtake in the methods section and explain why we use a different term for the hot steppe.

line(s) 552/553: I do not understand: how does feed demand change forage supply? Forage supply is a biomass potential offered by the plant community. Increased feed demand, as described here by your correction, should not increase the forage supply of the plant community (unless through growth overcompensation), but rather reduce the supply due to the increased demand from the animal side?

As in the previous comment we acknowledge that using the term forage supply creates some confusion and refer to our proposal to change this (reply in L424-431)

line(s) 554/555: The fact that animal demand could not be met AND above-ground biomass collapsed is a rather clear indication of over-grazing / exceeding of rangeland carrying capacity. In this context, maybe also discuss changes in the PFT community composition, i.e., changes in the prevailing strategy types. It can be expected that such a shift in strategy types occurs under such circumstances.

We agree with the reviewer that the model results provide strong evidence for overgrazing and will add a phrase explicitly stating so. We will also add a sentence discussing the change in community composition which shows an increase of the C-PFT (and also to some extent the R-PFT) as shown in Fig SI 9 and 12.

line(s) 562/563: You did not combine fertilization with irrigation, right? Do you expect that fertilization in combination with irrigation would increase leaf biomass beyond the level reached with irrigation alone?

Generally, irrigation alone already affects processes related to inorganic N inputs and losses. Biological N fixation and mineralization increase with increasing soil moisture. However, irrigation also leads to higher leaching. We therefore expect that the PFTs are still N limited even though irrigation may already increase but could also decrease inorganic N availability. Additional inorganic N from fertilization may remove the N limitation leading to an additional leaf biomass increase but may also lead to higher maintenance respiration limiting leaf biomass growth. Therefore, we cannot give an unambiguous answer and will add this explanation in section 4.1.3.

line(s) 575: “Fertilization had no effect on SOC” – Not surprising, given that fertilization without irrigation did not increase leaf biomass and therefore C-input to the soil.

We agree with the explanation of the reviewer and will add this to the sentence.

line(s) 580/581: “it seems that an S-strategy remained advantageous” - Again, I wonder about the turnover time required by the model to let a community transition from one steady-state to a new steady-state.

While for the hot steppe we can provide clear evidence, that a new steady state was reached, for the cold steppe the reviewer raises an interesting point. Increased soil moisture from irrigation may lead to an increase of the NO3 and NH4 pools from mineralization and biological nitrogen fixation which may take longer than the simulated time frame (see also reply in L420-423). We will add this to the discussion.

line(s) 600: And it may be interesting how grass-tree coexistence (typical for savanna sites as the one one in South Africa) will affect grass layer community composition compared to a situation where trees are excluded from the simulation.

Indeed an improved representation of Savannahs would be a major step for DGVMs. In order to achieve this, we see the need for additional model development as discussed in Rolinski et al., (2021). We will add a sentence on this in the limitations and further need for research section.

line(s) 606/607: “Generally, a change in resource availability does only change the conditions for the establishment of a community but does not directly affect the established vegetation” – Environmental filtering can also affect the established community by increasing mortality for specific strategy types within the community, not only by changing establishment success of given strategy type. Since you seem to have no other mortality causes aside from age-dependent mortality in the model (at least not for the grass layer), you will not see this effect, but it does exist, nonetheless.

We agree with the reviewer and will extend this sentence to reflect the limitation of our model to age mortality and to discuss potential effects of other causes of mortality.

line(s) 614: Why are N-fixers not separate PFTs in the model? I'm a bit surprised that they are not.

Facing the challenge of adding new PFTs to a classic DGVM, our aim was to reduce complexity as much as possible at first. This included restricting ourselves to add as little PFTs as possible. Grouping N-fixers with non-fixers halved the number of PFTs. We believe this is reasonable because the model will only fix additional N if the demand is not fulfilled. In an approach with two separate PFTs, this would mean a change in community composition and an increase of the N-fixer PFT at the expense of the non-fixer. In our approach, this simply means an increase in biological nitrogen fixation. One could say, that implicitly the PFT is a fixer if needed and not if not needed and could determine this status using the biological nitrogen fixation output. We will add the necessary detail to the description of biological N fixation in Appendix A4.

line(s) 622/623: So the assumption is that grazing is non-preferential, correct? I.e., grazers do not favor one PFT over another, for example based on criteria that characterize palatability / nutrition value. This is a simplification in the model that should be discussed briefly, as herbivores usually do not function the same way as mowing (or fire) that removes biomass indiscriminatingly.

Yes, grazing is not preferential. As proposed in our reply in L301ff we will include this in the model description and include a brief discussion in the limitations.

line(s) 624: "tolerance or avoidance" – Avoidance would for example (aside from temporal avoidance) be realized by being unpalatable. As your grazing is non-preferential, being a grazing avoider type based on palatability would not make a difference in your model as the animals would not discriminate against the avoider. This is a limitation you should mention.

We thank the reviewer for raising this point and will include grazing avoidance through palatability in the limitations together with preferential grazing (reply in L503f).

line(s) 629/630: "… and the PFTs had to follow a grazing-tolerance strategy." - The fact that grazing avoidance can only be achieved through life cycle adaptation and not through palatability likely causes a bias in your community composition. You should at least mention this possibility.

We thank the reviewer for their suggestion and will add a sentence on this at the end of the section (L631).

line(s) 632/633: "At the cold steppe site, grazing only happened during the growing season and both grazing tolerance an avoidance could be useful strategies." – Well, likely not avoidance in the way you can represent it in the model (temporal avoidance). If grazing happens during the growing season, and your only way to implement avoidance is through life cycle adaptation, i.e., temporal avoidance, this will push avoiders to the non-growing season as time when no grazing happens. But I don't see how avoiders could succeed by shifting their existence focus to exactly the season when growth is not possible?

We will add a phrase acknowledging that the model will not be able to simulate the type of avoidance that is likely successful.

line(s) 643-645: This challenge could be circumvented by moving away from a PFT-concept with fixed pre-defined parameter values for each PFT, which implicitly limits the number of strategies that can be realized, for example by defining typical value ranges for the given parameters of a strategy type. Within these continuous ranges, a strategy type can assume many trait value combinations that define its location within the trait space occupied by the strategy type, and therefore allows more plasticity within a strategy type, e.g., a plant could be a moderate, intermediate, or extreme S-strategy type.

We agree with the reviewer, that moving away from the fixed PFT approach is a suitable way to circumvent many of these issues. As discussed in previous comments one necessity is to follow an individual based approach as in aDGVM2 or LPJ-FIT. We see this as a promising and intriguing topic for future model development of LPJmL-CSR and will emphasize this more in the discussion.

line(s) 645/646: The challenge will be to expand this site-scale-focused approach to a generalized large-scale / global approach, because it will not be possible to parameterize suitable PFTs for all imaginable locations and circumstances. I think the value of what you show in this study is to prove that the CSR-concept can work within a DGVM and is ecologically sound in many points. But to make it general, you will have to move away from the discrete parameterization of your PFT approach, for example by allowing an evolutionary approach that self-selects successfull strategies via environmental filtering from a pool of potential trait value combinations, where each trait is represented by a continuous range of allowed values.

The generalization for a global application indeed poses a challenge. However, for the tree PFTs, researchers managed to find a set for classic DGVMS that represents the broad range of environmental conditions possible. We believe that for herbaceous PFTs it will also be possible to find a suitable set that will improve the representation of grasslands in current DGVMs We hope to present this in a separate study in the near future. In the long term, additional model development including the step towards dynamic PFTs will further improve the representation of different growth strategies.

line(s) 664/665: I do not really agree with this approach. The light extinction coefficient (as I know it) is a constant that describes how much light a respective layer of leaves will absorb and how much it will allow to transmit to the next lower leaf level. As such, it is a proxy associated with leaf characteristics such as leaf thickness or SLA more than overall plant stature. If anything, I'd deem LAI closer to stature than the light extinction coefficient, if you do not have height available as state variable.

The reviewer is correct that the light extinction coefficient usually refers to the transmissivity of a leaf layer. In theory, this is represented as one leaf with a given height and SLA per layer. However, LPJmL and other classic DGVMs do not simulate different leaf layers but calculate the light extinction of the entire vegetation layer of one PFT. Therefore, the model actually calculates the light extinction of a stack of leaves. A larger stack of leaves will transmit less light and therefore has a higher light extinction coefficient compared to a smaller stack of leaves. Following this, the height of several leaf layers (or the vegetation layer) can be interpreted as a function of SLA and the light extinction coefficient. As mentioned in the discussion (L663-666) and previous work (Wirth et al., 2021) we think that this is a major limitation and believe that adding plant height as a state variable would be an important model development. As stated in our reply to the related comment in L218f we will improve the model description and refer to this in the discussion.

line(s) 674: In rangelands, mechanical stress through trampling would be another important aspect to consider.

Similar to the missing inclusion of preferential grazing (comment in L294-300), this is related to the representation of grazing. We will add trampling to the discussion of the limitations of the current grazing approach as well.

Minor editorial comments

We appreciate the thorough reading and will adopt all minor editorial comments below without responding to each of those separately.

line(s) 10: "… a temperate grassland, a hot and a cold steppe…" => "… a temperate grassland and a hot and a cold steppe…"

line(s) 13: at three grassland sites => at the three grassland sites

line(s) 17: Our results show, that => delete comma

line(s) 39: high carbon inputs => high carbon sequestration

line(s) 61: (examples) => delete, seems to be a leftover note from manuscript writing. Or alternatively replace with the examples you were thinking of…

line(s) 183: "recover slower" – "recover more slowly"

line(s) 184: "the SLA leaf longevity trade-off" – "the SLA v. leaf longevity trade-off"

line(s) 328: "While it remained similar…" – "However, it remained similar…"

line(s) 359 correct typo: resourCe

line(s) 420 contribute – contributed

line(s) 456: "we present results on above-ground biomass" – "we present results based on above-ground biomass"

line(s) 490: "this allows to assess" – "this allows assessment of", or "this allows assessing"

line(s) 496: we only assess – we only assessed

line(s) 533: "this can be explained with" – "this can be explained by"

line(s) 535: "and contributed to the litter layer" –"and increased the input to the litter layer".

line(s) 539: "In addition irrigation led to…" – "In addition, irrigation led to…"

line(s) 619: "…which constituted an additional investment." – Rephrase? "…and therefore, a reduction of investment costs associated with N-fixation."

Hyphenation of two-word combinations that are used in the function of an adjective:

line(s) 69: “disturbance prone environments” – “disturbance-prone environments”

line(s) 73: “multi species communities” – “multi-species communities”

line(s) 181 “stress prone ecosystems” – “stress-prone ecosystems”

l- 203: “age dependent individual mortality” – “age-dependent individual mortality”

line(s) 231 “plant specific resource availability” – “plant-specific resource availability”

line(s) 249 “site specific conditions” – “site-specific conditions”

line(s) 296: bias adjusted data” – “bias-adjusted data”

line(s) 374, 375 “water saving strategy” – “water-saving strategy”

line(s) 397 resource limited – resource-limited

line(s) 473: “S dominated community” – “S-dominated community”

line(s) 496: “neither water nor nutrient limited” – “neither water- nor nutrient-limited”

line(s) 542, line(s) 543, line(s) 579 “S dominated” – “S-dominated”

l- 580 “nutrient limited” – “nutrient-limited”

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