1 Review #1

2 General evaluation of the research paper

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

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

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

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

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

28 Detailed comments

29 Introduction:

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

33 We agree, even though we do not look into the effects of changing CO2 concentrations it
34 should be a part of this overview.

35 We added "atmospheric CO_2 concentration" to the list in L39 and added the sentence
36 "Atmospheric CO_2 constitutes the basic resource for photosynthesis and its rising
37 concentration can shift the competitive balance between C3 and C4 grassland species (ref.)."
38 in L39f

We added "if soil moisture levels are sufficient to permit the formation of microbial community." in L46f

We added "by removing moribund plant material and triggering growth (over-)compensation." at the end of the sentence in L50f.

We replaced species with "plant functional types (PFTs) representative of species" in L54 and species with PFT throughout the manuscript when referring to the modelling approach and simulations results. However, we kept the term species when describing the theoretical background and results of field experiments to remain precise.

We added the suggested phrase in L56.

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 kept the naming as is but added a phrase pointing towards the term savanna rangeland.

We replaced "hot steppe" with "savanna rangeland" in L111 and added "[…], following the Köppen-Geiger climate classification (Kottek et al., 2006)" in L113f.

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

77 separation we distinguish between the scenarios that were predefined by the data and those we
78 selected for further analysis. When defining the scenarios for further analysis, we decided to
79 use extreme cases to test the effect of different limiting resources (e.g. infinite nutrient
80 availability) instead of choosing different fertilizer levels. Regarding the defoliation intensity,
81 we agree that analyzing a gradient of different intensities provides another interesting
82 experiment. However, we decided to put our main focus on the resources and believe that the
83 defoliation intensities of the experiment already cover a sufficient range.

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

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

97 We picked this up in the discussion L813-825 (see also reply in L760-783).

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

100 We changed the title of section 2.1 to the reviewer's suggestion.

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

104 We thank the reviewer for pointing this out. Indeed the tropical grass is a C4-type.

105 We added the photosynthetic pathway to the listing of the different PFTs in L131.

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

107 We added the reference to (Jägermeyr et al., 2015) in L156.

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

111 We added the time to the unit in Table 1 and changed the terminology to forage offtake which
112 we define in L35f.

113 line(s) 166-168: Does this new scheme also account for root biomass distribution in different
114 soil layers, and therefore varying water availability between different soil layers? So that the

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

122 We added "First, the PFTs access to water from different soil layers is calculated as described
123 in Schaphoff et al. (2018)." in L195 and replaced "The new parameter (k_{root}), which is a
124 proxy for root properties associated with morphological properties of the root network (e.g.
125 branching and spread)." With "Second, the amount of water available for the PFT is
126 determined considering its root biomass and the new parameter (k_{root}), which is a proxy for
127 root properties associated with morphological properties of the root network (e.g. branching
128 and spread)." in L196-198.

131 Yes, it is a constant but as stated in our reply to a previous comment (L89-97), we agree with
132 the reviewer that there is great potential in exploring the entire continuum.

133 We now discuss this in L813-825 (see also reply in 760-783).

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

148 We added the following phrases and sentences:

149 • "that do not distinguish between forbs and graminoids:" in L131.
150 • "The only additional cause of mortality was negative leaf and/or root biomass after
151 allocation as a result of prolonged stress. While this may be caused by water stress,
152 additional causes of mortality from water stress such as embolism (Jacobsen et al.,
153 2019) as well as heat stress were not considered." in L231ff.
154 • "We did not implement additional causes of mortality such as embolism." in L253f.

157 Using "area-specific" as suggested by the reviewer is in our opinion less explicit since it does
158 not define which area. We instead replaced "available area" with bare ground area.

159 In response to a comment of reviewer 2 we amended substantial parts of section 2.3.3 and
160 removed the respective phrase. In the updated section 2.3.3 we use the term bare-ground area
161 as suggested L235, 244 and 246.

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

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

172 We added "Another important aspect in savanna and other dryland ecosystems is the
173 distinction between annual and perennial plants. In LPJmL, this distinction is not explicitly
174 made. While the R-PFT has a higher replacement rate of average individuals, it is not
175 constrained to a specific growing season, after which it is completely killed to be
176 reestablished the following growing season. Incorporating this distinction into the model is an
177 option to add additional functional diversity and will likely improve model results." in L842-
178 846.

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

187 Indeed, only herbaceous PFTs are allowed to establish on managed grassland stands. We
188 added this to the model description. We agree with the reviewer that the term sapling is
189 misleading in this context and replaced it with the term seedling throughout the manuscript. In
190 addition, since this may create some confusion regarding the sapling LAI parameter, for
191 which we had to keep the term, but explained the origin of the parameter name and its
192 purpose.

193 We added

194 • "Tree PFTs, which are also part of LPJmL, were not allowed to establish on managed
195 grasslands and all further descriptions provided here of or related to PFTs only
196 concern herbaceous PFTs." in L132ff.

197 • "While seedling is the more intuitive term for herbaceous plants and we will use it
198   throughout the manuscript, the subscript in the parameter name refers to saplings
199   because it was adopted from the tree PFTs in the past. " in L312ff.
200 • And replaced "sapling" with "seedling" throughout the manuscript.

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

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

213 We added "In LPJmL, each PFT represent an entire population of adult plants using the
214 concept of average individuals. The PFT describes the carbon and nitrogen stocks of the
215 leaves and roots of an average individual and the number of average individuals in a
216 population. It follows, that the carbon and nitrogen stocks of the population can be determined
217 by multiplying the average individual stocks with the number of average individuals.". in
218 L136-139. We replaced "As a consequence, all grasslands that are not located at the border
219 between climatic regions were simulated using only one of these PFTs to represent
220 herbaceous vegetation." with "Carbon and nitrogen stocks as well as the number of average
221 individuals are dynamically calculated each day from the simulated processes which are:" in
222 L139f and added "Prior to our implementation, each herbaceous PFT was represented by one
223 average plant individual." to L150.

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

226 Thank you for this comment. Actually neither is the case. Depending on the growth
227 efficiency, the number of average individuals is reduced (Appendix A3 L912-951). Actual
228 mortality is derived from the maximum mortality rate - which is the same for all strategy
229 types - and the growth efficiency. The growth efficiency is dependent on SLA, which differs
230 between the strategy types (Appendix A3 Eq. A10). We extended the description in Appendix
231 A3.

232 We replaced "[…] that day." In L943-947 with "since the last allocation and $C_{ind,PFT}$ is the
233 biomass increment from photosynthesis since the last allocation. The growth efficiency $\Delta bm \cdot$
234 $C_{ind,leaf,PFT}^{-1}$ is the ratio of the net carbon change and the carbon stock of the leaves, which is
235 lower for old plants. The SLA influences the maximum age of the different strategies
236 assuming that plants with a low SLA and faster metabolism reach a lower age compared to
237 high SLA plants.".

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

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

248   We added

249   • "In theory, however, the number of PFTs that could coexist within a grid cell is not
250   limited." In L135f.
251   • "In grasslands with a high growth efficiency and frequent defoliation establishment
252   may lead to a continuous increase of the number of average individuals. To avoid
253   numerical errors that could results from this, we prohibit the number of average
254   individuals to exceed 250 $Ind. \cdot m^{-2}$." In L950f.

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

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

270   We replaced "age dependent individual mortality" with "age-mortality" and added "The
271   growth efficiency is the ratio of the net change in the individual carbon stocks (the result of
272   net photosynthesis and turnover) and the individual carbon stocks. Assuming that old plants
273   grow more slowly this is used as a proxy for population age and resulting age-mortality." in
274   L251ff.

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

280   We abandoned the terminology abiotic and biotic gradient. When writing the original draft,
281   we found that it provides a clear distinction between the parameters related to each gradient.
282   However, as the reviewer correctly noted, this creates an additional layer of terminology to
283   understand when reading the manuscript.

284   We abandoned the terms "abiotic dimension" and "biotic dimension" and instead now directly
285   refer to the "stress gradient" and the "disturbance gradient" throughout the manuscript.
286   Additionally, we replaced the dimension column in Table 2 with a column that provides
287   information on the predominant gradient.

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

289   We added "based on our expertise" in L378.

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

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

302   We added "We assumed all parameters to be independent from each other. While we are
303   aware that SLA and the light extinction coefficient k_{beer} are correlated in reality because the
304   transmissivity of leaves increases with SLA we have to treat them as independent because in
305   LPJmL, the light extinction coefficient does not describe the transmissivity of a single leaf but
306   of the entire vegetation layer. Stacking a high number of high transmissivity leaves may result
307   in the same light extinction compared to a lower number of low transmissivity leaves. In
308   LPJmL-CSR, a similar k_{beer} would be assigned for both cases because it represents the light
309   extinction coefficient of the entire vegetation layer." in L338-344.

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

312   We changed the unit to $[\text{mm d}^{-1}]$.

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

317   We agree that there are cases were the distinction between biotic and abiotic is not so clear.
318   As already stated earlier (reply in L281-288) we abandoned the terms and only retain the
319   terms stress and disturbance gradient.

320   line(s) 240/241: The light extinction coefficient describes the fraction of light intercepted by
321   each additional leaf layer, right? As the amount of light that can transmit a leaf layer depends
322   on the thickness of the leaf, one would expect kbeer to be correlated with SLA, which, unlike
323   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 L293-311. We now also describe more clearly, which parameters play a role for the stress or the disturbance gradient or for both gradients.

In addition to the changes described in our reply in L293-311, we added a column for the subsidiary gradient in Table 2.

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 changed offspring size to offspring biomass and added an explanation of the relationship to SLA. We also revised the discussion to reflect both SLA and sapling LAI when discussing offspring biomass.

We incorporated a more accurate description in the overview of the parameters in section 2.4.1 which contains the following sentence "In LPJmL 5 and in LPJmL-CSR, it is used to calculate the above-ground biomass of a seedling using the PFTspecific SLA". in L333f.

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

We swapped columns variable and site.

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 changed "some processes within the model" to "the processes, listed in sect. 4.1.2," and removed the reference to section 4.1.2 at the end of the sentence (L389f).

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 added the following to the modelling protocol section.

"Before simulating managed grasslands, the model was run for 30000 years with natural vegetation to obtain an equilibrium of the carbon and nitrogen cycle during a spinup simulation. Afterwards, a second spinup of 390 years was conducted to account for the effects

363   of historical land-use change on soil conditions." in L402ff and replaced "390 years" with
364   "the second spinup period" in L406.

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

367   All processes are executed on a daily time scale. We also compute the outputs on a daily
368   timescale but aggregate to a monthly or annual resolution for some of the results.

369   We added "LPJmL-CSR simulates all processes and provides all outputs with a daily
370   resolution. If necessary, outputs are aggregated to a monthly or annual resolution in the
371   postprocessing." in L401f.

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

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

381   We added

382   • "The initial community composition is not prescribed. Instead, upon initialisation,
383   each PFT is established based on the PFT-specific establishment rate and offspring
384   biomass (sect. 2.3.3 and 2.4.1). The community composition during each time step
385   emerges from the competition for resources dependent on the processes described
386   above." in L150-153.
387   • "Furthermore, in LPJmL-CSR the initial community composition is not dependent on
388   additional data which facilitates the application at different sites or at larger scales." in
389   L606f.

390   Results

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

394   We added the units to the caption and added the temporal dimension of the unit to the
395   subtitles in the figure.

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

403 The reviewer raises an important point. Indeed the current implementation (Rolinski et al.,
404 2018) does not consider preferences for specific PFTs. We now briefly mention this when
405 describing the model and discuss this in the section on future need for development.

406 We added

407 • "In this study, we use the mowing and the daily grazing option. The daily grazing
408   option does not account for animal preferences (Rolinski et al., 2018)." in L154f.
409 • "Plant species have adapted to grazers in manifold ways, one of which is grazing
410   avoidance by being less or even unpalatable. This is a successful strategy in grazing
411   systems because in contrast to mowing, which is indiscriminate, grazing animals show
412   preferences for plants with a higher palatability. Selective grazing and grazing
413   avoidance through palatability are currently not represented in LPJmL but can have a
414   strong effect on the community composition (Newman et al., 1995; Parsons et al.,
415   1994). Including preferences for example for high SLA PFTs may improve simulation
416   results further." in L851-856.

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

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

431 We included a thorough description of the processes controlled by each parameter in section
432 2.4.1, L284-306 and L315-335 (see also reply in L1019-114). We extended section 3.2.1 in
433 L477-482 by "However, this is likely an artefact of the parameterization. As stated in sect.
434 2.4.1, both SLA and $lmro$ do not only underpin the compensation of defoliation but can also
435 play a role for resource uptake and distribution. In the ungrazed scenario (C0), no defoliation
436 has to be compensated and both parameters are only needed for their secondary role for
437 resource uptake and distribution which likely affected the selection of g_{min}. In contrast in the
438 grazed scenario (C1), they are needed for their primary role and g_{min} and E_{max} become more
439 important for resource uptake and distribution.".

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

444 Thank you for the interesting question. We did not look into this in detail. Since biomass is an
445 important variable when calculating FPC, we believe it is likely that forage supply and
446 growing season FPC are similar. However, there might be differences when averaging over

447 the entire year. We analyzed our results regarding this and the results confirm our hypothesis
448 that above-ground biomass and FPC are similar. This is not surprising since above-ground
449 biomass is used to calculate the FPC. Proportional differences between the PFTs' FPC closely
450 resemble differences in their above-ground biomass. Deviations are a results of the PFTs'
451 different SLA and k_{beer} values. We believe that adding this will not provide any additional
452 value to the manuscript and therefore did not make any changes.

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

456 This was also surprising and counterintuitive to us. We already provide an explanation in the
457 discussion in L706-710, which we now reference to in the sentence in L555f.

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

463 We already touch upon this in L666-669 but agree that this can be discussed in more detail.
464 We added a reference in L589 and extended the discussion in section 4.1.2.

465 We added

466 • "Whether or not this is the new equilibrium state or the community is still
467   transitioning is crucial (sect. 4.1.2)" in L589.
468 • "Less than two years is a very fast transition and while the shares of the leaf biomass
469   seem to have reached a new equilibrium after one or two years of irrigation, it is likely
470   that the soil carbon and nitrogen pools are not in equilibrium yet. This is especially
471   interesting when considering that the overall increase in leaf biomass may promote
472   litterfall and the formation of inorganic nitrogen. This in turn may lead to reduced
473   nitrogen limitation and additional changes in the community composition.
474   Furthermore, biological nitrogen fixation is dependent on soil moisture and may
475   therefore also contribute to decreasing nitrogen stress under irrigation. However,
476   irrigation also leads to increased leaching and could therefore also decrease inorganic
477   nitrogen availability." in L673-679.

478 Discussion

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

485 The reviewer raises several interesting questions that go beyond this study. We are currently
486 working on a globally applicable set of PFTs, which will form the basis of another study in
487 the near future. For that study, we retain the fixed PFT parameterization of classic DGVMs.
488 However, we are generally open and very much interested in further developing the model.

489 As already mentioned in the reply in L89-97, it would be very interesting to combine the
490 approach of LPJmL-CSR and aDGVM2 or LPJ-FIT.

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

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

497 We added "We identified two reasons for the faster growth. First, the new implementation for
498 biological nitrogen fixation (Appendix A4) reduced nitrogen stress and promoted higher
499 photosynthesis rates. Second, while the parameters used for LPJmL-CSR were tuned for
500 performance under the site specific environmental conditions and management, the
501 parameters used in LPJmL 5 were defined for large scale simulations with different
502 management." in L611-614.

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

507 The livestock density refers only to the spin-up and the historical periods for which no data on
508 actual land use were available. Therefore, it is entirely unrelated to the transient simulations
509 that reproduce the mowing experiments.

510 We replaced cows with LSUs and "[…] that […]" with " […] for the land use spinup
511 simulation (see Sect. 2.5 and SI) to prescribe a fixed grazing pressure, which […]" in L623f.

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

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

519 We added "The increased soil carbon input had three reasons. First, the trade-off between
520 SLA and leaf longevity lead to higher turnover rates and in turn higher litterfall compared to
521 LPJmL 5. Second, accounting for mortality explicitly constituted an additional input into the
522 litter layer. Third, our simulation included manure application which provided an additional
523 carbon input into the system." in L626-629.

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

527 We adopted this suggestion see reply in L188-201.

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 added 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.

We added "The dominance of the S-PFT independent of grazing is plausible considering the
pronounced dry vs. wet season dynamics at the site that impose water stress and potentially
also nitrogen stress." in L651ff.

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 L587f 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 (L656-662) but we agree that this is very brief.
We now added more detail and highlighted the transition time more prominently. We also
provided an explanation for the fast transition, which was 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 added

- "LPJmL does not simulate seed bank formation and reproduction is not limited by the
  amount of seeds available in a seed bank. Instead, the establishment depends on the
  bare ground area and the PFT-specific establishment rate." in L234f.
- "Regardless of the finality of the transition, its velocity is likely overestimated by
  LPJmL for two reasons. First, the C- and R-PFT can establish quickly despite their

571   limited presence before the onset of irrigation because LPJmL does not simulate a
572   seed bank which would in reality be small at least for the C-PFT limiting its
573   establishment. Second, in reality growth of established individuals is limited and
574   reproduction and dispersal, which slow down population biomass increase, are needed
575   for such a transition. In LPJmL, already established individuals continue to grow and
576   the population biomass increases even without additional establishment." in L680-685.

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

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

581 We replaced "However, periods of drought can induce an additional disturbance (Wang et al.,
582 2019) creating a niche for R strategists (Kooyers, 2015; Norton et al., 2016)." with "The
583 success of both the C- and the R-PFT is likely determined by the similarity of their SLA, k_{beer}
584 and $lmro$ which become more important compared to E_{max} and g_{min} if there is no water
585 limitation. Potentially larger differences in these parameter would lead to the success of one
586 of the two instead." in L669-672.

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

592 We agree that our use of forage supply was ambiguous because we use it to define the amount
593 of biomass removed through mowing or grazing for the temperate grassland and the cold
594 steppe but also for the amount of leaf biomass available for grazing for the hot steppe. This
595 was an attempt to use common terms for all sites, which appears to be confusing instead of
596 helpful. We therefore changed the term forage supply to forage offtake for the temperate
597 grassland and the cold steppe and use the term leaf biomass for the hot steppe.

598 We added a definition for forage offtake in L34ff and replaced "supply" with "offtake" for the
599 temperate grassland and the cold steppe and "forage supply" with "leaf biomass" for the hot
600 steppe.

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

606 As in the previous comment we acknowledge that using the term forage supply creates some
607 confusion which we resolved as stated in the reply in L601-609.

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

We agree with the reviewer that the model results provide strong evidence for overgrazing and added a phrase explicitly stating so. We also added 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.

We added "indicating overgrazing" in L693 and "Additionally, LPJmL simulates a different community composition compared to the low grazing intensity. The relative share of the C- and to some extent also the R-PFT is higher for the high grazing intensity (Fig. SI 9 b and 12 h) because such strategies are better suited to tolerate grazing." in L694ff.

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. We added this explanation in section 4.1.3.

We added "Similar to the hot steppe, it is possible, that our time frame is too short for the soil pools to have reached a new equilibrium. As described in Sect. 4.1.2, irrigation alone already affects processes that could increase nitrogen supply by biological nitrogen fixation and litterfall, but also decrease it by leaching. Both biological nitrogen fixation and mineralisation are dependent on soil moisture as well as on temperature which is low in the cold steppe limiting the increase of inorganic nitrogen. Therefore, it is possible that only an intermediate state emerges during our simulation period. Especially when also considering the increased leaching, we expect that the cold steppe is still nitrogen limited under irrigation, therefore combining irrigation with fertilisation could further reduce nitrogen limitation leading to increased productivity and changes in the community composition. However, the leaf biomass increase may also be limited by higher maintenance respiration which is connected to leaf nitrogen content. Additional analysis is needed to enhance the understanding of these complex interactions." in L723-731.

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 and added "because leaf biomass and in turn carbon inputs into the soil did not increase" in L715f.

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 NO_3 and NH_4 pools from mineralization and

655　biological nitrogen fixation which may take longer than the simulated time frame (see also
656　reply in L558-585). We added this to the discussion.

657　See reply in L634-655.

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

661　Indeed an improved representation of Savannahs would be a major step for DGVMs. In order
662　to achieve this, we see the need for additional model development as discussed in Rolinski et
663　al., (2021).

664　We added "Furthermore, the coexistence of tree and grass species, which is typical for
665　savanna sites, is not implemented in the LPJmL model. However, this is crucial to adequately
666　represent such ecosystems (Rolinski et al., 2021) and should be a focus of future model
667　development." in L840ff.

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

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

677　We

678　　• replaced "Generally" with "In LPJmL-CSR" in L754
679　　• added "In reality however, a change in resource availability may also increase the
680　　　mortality for specific strategy types affecting the already established community as
681　　　well." in L756f.
682　　• added "LPJmL-CSR only represents age mortality, i.e. the effects of mortality from
683　　　other causes such as frost, heat and embolism are not represented. Especially under
684　　　changing climatic conditions, specific strategy types may show increased mortality
685　　　and lose their advantage to the advantage of other strategy types. Including additional
686　　　causes of mortality may introduce additional trade-offs and enhance the differentiation
687　　　between strategy types." in L847-850.

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

690　Facing the challenge of adding new PFTs to a classic DGVM, our aim was to reduce
691　complexity as much as possible at first. This included restricting ourselves to add as little
692　PFTs as possible. Grouping N-fixers with non-fixers halved the number of PFTs. We believe
693　this is reasonable because the model will only fix additional N if the demand is not fulfilled.
694　In an approach with two separate PFTs, this would mean a change in community composition
695　and an increase of the N-fixer PFT at the expense of the non-fixer. In our approach, this

696 simply means an increase in biological nitrogen fixation. One could say, that implicitly the
697 PFT is a fixer if needed and not if not needed and could determine this status using the
698 biological nitrogen fixation output. We added the necessary detail to the description of
699 biological N fixation in Appendix A4.

700 We added "While in reality, biological nitrogen fixation is a feature restricted to legume
701 species, in LPJmL we decided to not distinguish in fixing and non-fixing PFTs to keep the
702 number of PFTs as small as possible. This is reasonable because a PFT can be representative
703 of multiple species and will only fix additional nitrogen if its demand cannot be fulfilled by
704 other sources of nitrogen uptake and if its NPP is sufficient. One could say, the PFT has the
705 ability to fix nitrogen only if needed comparable to a community containing legumes only if
706 they are advantageous." in L970-974.

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

712 Yes, grazing is not preferential. As stated in our reply in L406-419 we included this in the
713 model description and briefly discuss the limitations of the current approach.

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

718 We thank the reviewer for raising this point and included grazing avoidance through
719 palatability in the limitations together with preferential grazing (reply in L723f).

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

724 We thank the reviewer for their suggestion.

725 We added "Because LPJmL does not account for differences in the palatability of different
726 strategy types the parameterization could not select for such likely successful strategies
727 leading to a potentially biased community composition." in L781ff.

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

735 We added "However, grazing avoidance in time, which is the only type simulated by LPJmL
736 will not be successful as it would mean shifting biomass production to the non-growing

season where the environmental conditions do not allow growth." in L785ff to acknowledge that the model is not 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 emphasize this more in the discussion.

We added "Generally, the approach of using a small number of PFTs with a fixed set of parameters has been criticised (Quillet et al., 2010) leading to the development of next generation DGVMs that apply an individual based approach such as LPJmL-FIT (Sakschewski et al., 2015) or aDGVM (Scheiter et al., 2013). These models simulate the competition between individual plants for which parameter values are drawn from predefined ranges upon establishment. Given sufficient time, only successful strategies will survive. Such models provide a much more nuanced representation of function diversity compared to classic DGVMs with their coarse division into fixed PFTs but are also more computationally substantially more expensive because of the high number of individuals for which all processes have to be calculated. Past studies have therefore often focused on specific regions such as the Amazon rainforest (Sakschewski et al., 2015), European forests (Thonicke et al., 2020) or South African semi-arid rangelands (Pfeiffer et al., 2019). In contrast, classic DGVMs are still widely applied on the global scale for example to calculate the global carbon budget (Friedlingstein et al., 2022) and we see the need to continue their development for the foreseeable future. Combining our approach of distinguishing between PFTs that follow the main strategies of the CSR theory with an individual based approach making use of the full parameter range instead of single points provides an interesting opportunity for future research of diverse grasslands." in L813-825.

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 amended the model description and refer to this in the discussion.

We added "We here deviate from the common interpretation of the light extinction coefficient, which is usually defined as the light absorption of a layer of leaves. However, as explained in Sect. 2.4.1, LPJmL represents the entire vegetation as a single layer and we therefore define the light extinction coefficient not for a single leaf but a stack of leaves. Taller plants likely produce more layers of leaves corresponding to a larger stack and a thicker vegetation layer with a higher light extinction. However, thickness of the vegetation layer is not explicitly represented in LPJmL and we represent the described differences by using lower light extinction coefficients for small stature plants for which we assume a lower thickness of the vegetation layer and higher light extinction coefficients for large stature plants." in L832-837.

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 added "Additionaly, LPJmL-CSR does not consider mechanical stress caused by trampling of animals and potential strategy dependent damage. Incorporating this may add another dimension of stress to distinguish different PFTs." in L856f.

Minor editorial comments

We appreciate the thorough reading adopted 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…"

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

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

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

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

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

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

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

833   line(s) 359 correct typo: resourCe

834   line(s) 420 contribute – contributed

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

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

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

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

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

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

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

844

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

846   line(s) 69: "disturbance prone environments" – "disturbance-prone environments"

847   line(s) 73: "multi species communities" – "multi-species communities"

848   line(s) 181 "stress prone ecosystems" – "stress-prone ecosystems"

849   l- 203: "age dependent individual mortality" – "age-dependent individual mortality"

850   line(s) 231 "plant specific resource availability" – "plant-specific resource availability"

851   line(s) 249 "site specific conditions" – "site-specific conditions"

852   line(s) 296: bias adjusted data" – "bias-adjusted data"

853   line(s) 374, 375 "water saving strategy" – "water-saving strategy"

854   line(s) 397 resource limited – resource-limited

855   line(s) 473: "S dominated community" – "S-dominated community"

856   line(s) 496: "neither water nor nutrient limited" – "neither water- nor nutrient-limited"

857   line(s) 542, line(s) 543, line(s) 579 "S dominated" – "S-dominated"

858   l- 580 "nutrient limited" – "nutrient-limited"

859

860   References

861   Scheiter, S., Langan, L. and Higgins, S.I., 2013. Next-generation dynamic global vegetation
862   models: learning from community ecology. *New Phytologist*, *198*(3), pp.957-969.

863   Langan, L., Higgins, S.I. and Scheiter, S., 2017. Climate-biomes, pedo-biomes or pyro-
864   biomes: which world view explains the tropical forest–savanna boundary in South America?.
865   *Journal of Biogeography*, *44*(10), pp.2319-2330.

866

867   Review #2

868   This study is predicated on a novel way of quantifying CSR plant functional types (PFTs) for
869   species, and comparing these with frameworks including the leaf economics spectrum. This
870   forms the basis of the entire analysis, and so it is fundamental that the way the PFTs are
871   derived represents CSR theory and can be compared against the LES. There are a number of
872   basic problems with the approach used here, however.

873   We cordially thank the reviewer for taking the time to review our manuscript. When
874   comparing the reviewer's perception of our topic with our short summary, we gained the
875   impression that there has been a misunderstanding regarding the main focus of our study. In
876   turn, we believe that this led to a number of misconceptions, which we address in detail in our
877   responses. We are confident that our approach is solid and we hope that our changes to the
878   manuscript resolved the issues raised by the reviewer.

879   Using the trait specific leaf area (SLA) to represent both leaf economics and also within the
880   CSR calculations means that to two measures are very likely correlated, potentially leading to
881   a Type 1 statistical error in which the conclusions are accepted despite the statistical test not
882   being sufficient to assign a realistic probability.

883   Thank you for pointing this out, as it shows that our approach was not clearly described. We
884   agree that Type 1 statistical errors need to be avoided. However, we are not representing a
885   statistical but a functional relationship between SLA and leaf longevity here (L175-186 and
886   709-714). The connection between SLA and leaf longevity is well established following leaf

887 economics (LES, Wright et al., 2004), but was so far not implemented as such in the LPJmL
888 model for grasslands.
889 In the original LPJmL model version, SLA was only used to compute the leaf area index
890 (LAI) from the internally computed leaf biomass. In order to represent the establishment of
891 different C-, S- or R-strategists, it is important to represent advantages and disadvantages of
892 the leaf structure in the model. Thinner leaves (high SLA) have a shorter longevity and while
893 they grow quickly to intercept light, they need to be replaced frequently. Neglecting the need
894 to replace thin leaves more frequently would lead to an advantage of high SLA values under
895 all circumstances, which is in contrast to ecological theory and observations (e.g., Díaz et al.,
896 2016; Reich, 2014; Wright et al., 2004). This trade-off had been implemented and applied to
897 tropical (Sakschewski et al., 2015) and European forests (Thonicke et al., 2020) before. The
898 implementation in this study provides a functional relationship of the SLA-LL relationship, as
899 part of the LES, and CSR theory through SLA in grasslands (section 2.3.2 and appendix A2).
900 This newly implemented functional relationship controls the productivity of the different
901 PFTs and the resulting shares of the C-, S- and R-PFTs. However, we do not compare the
902 LES to the C-, S- and R-PFT shares, which would be comparing inputs to outputs and would
903 certainly show a correlation.

904 We

905 • replaced „The trade-off described by the leaf economic spectrum (LES) is mainly
906 associated with the traits Leaf N concentration and specific leaf area (SLA) (Wright et
907 al., 2004). In LPJmL, both traits are used to calculate the photosynthetic activity. The
908 leaf nitrogen concentration is a function of the assimilated carbon and nitrogen and
909 can vary within a PFT-specific range (von Bloh et al., 2018) to account for the growth
910 under different resource availabilities." with "The LES describes correlations between
911 several plant functional traits. Among these are the specific leaf area (SLA) and the
912 leaf longevity, which can be used to express the differences between resource
913 acquisitive vs. resource conservative growth strategies (Wright et al., 2004). The
914 resource acquisitive strategy is associated with fast growths of leaves at low
915 construction costs with a high SLA and a short longevity. In contrast, the resource
916 conservative strategy promotes slow growth of long-lived leaves with low SLA leaves.
917 Therefore, to represent the trade-offs involved with the differences between these
918 strategies a functional relationship between SLA and leaf longevity can be used.
919 Despite the importance of SLA and leaf longevity for several processes within LPJmL,
920 the SLA v. leaf longevity trade-off has not yet been implemented for managed
921 grasslands in LPJmL before." in L200-207.
922 • added "The leaf longevity was represented through turnover rates, which determine
923 the amount of leave biomass transferred to the litter layer (Schaphoff et al., 2018). As
924 long as differences between ecological strategies were not considered and only one
925 PFT was used to simulate a managed grassland, this approach was sufficient..
926 However, this means that grasslands along a stress gradient only differed in their
927 productivity but not in other aspects of the community. Yet in reality, slow growing,
928 resource conservative plants in stress-prone ecosystems are not only less productive
929 and supply less forage with a lower nutrient content (Lee, 2018; Onoda et al., 2017).
930 Such ecosystems are also more vulnerable to overgrazing (Liu et al., 2013) and
931 recover more slowly from disturbances (Teng et al., 2020). Incorporating the *SLA* v.
932 leaf longevity trade-off is essential to account for the differences between ecological
933 strategies, which are important to adequately represent ecosystem functions of
934 managed grasslands under different climatic conditions and management." in L209-
935 217.

936 • Replaced "Representing the SLA leaf longevity trade-off was thus essential to
937 incorporate different ecological strategies in LPJmL-CSR." with "The SLA v. leaf
938 longevity trade-off has already been implemented in the related LPJmL-FIT model
939 and applied to tropical (Sakschewski et al., 2015) and European forests (Thonicke et
940 al., 2020). For this study, we implemented the SLA v. leaf longevity trade-off for
941 managed grasslands using a functional relationship between the two based on trait
942 observations. Similar to Sakschewski et al. (2015),[…]" in L218-221.
943 • deleted "While this implementation was sufficient to represent the growth or resource
944 acquisition component of the LES, it did not represent the leaf longevity trade-off.
945 Instead leaf longevity was the same for all PFTs independent of their SLA. Therefore,
946 a PFT with a high SLA corresponding to a resource acquisitive strategy had an
947 advantage even under resource-limited conditions, because its competitor with a lower
948 SLA had the same leaf longevity and could not outlive the acquisitive PFT. As long as
949 only one PFT was used to simulate an entire community only the overall
950 photosynthetic capacity of the community had to be determined by the model. It was
951 thus acceptable to neglect the SLA v. leaf longevity trade-off because differences
952 between ecological strategies were not considered and the trade-off would have added
953 unnecessary complexity to the model. However, slow growing, resource conservative
954 plants of stress prone ecosystems supply less forage with a lower nutrient content
955 (Lee, 2018; Onoda et al., 2017). Additionally, such ecosystems are more vulnerable to
956 overgrazing (Liu et al., 2013) and recover slower from disturbances (Teng et al., 2020)
957 which is an important property when simulating stressed grasslands." in L207.
958 • replaced "which is used to calculate the PFT-specific leaf longevity" with "This power
959 law provides a functional relationship between SLA and leaf longevity, which is used
960 to calculate the PFT-specific leaf longevity from predefined SLA values within
961 LPJmL-CSR (A2)." in L222f.

962 With regard to stress, the authors state that "According to CSR theory, the stress gradient
963 expresses the level of stress a species is exposed to in a certain habitat. It ranges from
964 unstressed to severely stressed, but does not distinguish individual stress categories (e.g.
965 temperature, water or nutrient)" thus "different strategies for water-resource use can be used
966 to distinguish C- and R-strategists (low stress tolerance) from S-strategists". Thus the traits
967 used here are specific to water stress, and the definition of stress recognised in CSR theory
968 (constrained metabolic efficiency and thus biomass production) is not cited nor considered.

969 The reviewer raises a valid point. Of course, stress is not restricted to water stress and other
970 traits that are related to (too high or too low) temperature or to nutrient stress could be used to
971 distinguish PFTs. In principle, the LPJmL model also considers stress arising from
972 temperature and nutrient availability in addition to water stress in its phenology and nitrogen
973 acquisition routines. However, the grassland steppe sites that we simulated in our study are
974 predominantly limited by water. Therefore, we decided to focus only on water stress in this
975 first application of LPJmL-CSR. This allows for a better understanding of the underlying
976 processes and the resulting pattern. In addition to the traits related to general stress tolerance,
977 we therefore only include traits related to water stress. However, we agree that the
978 implications of this simplification should be discussed.

979 We

980 • added "LPJmL represents the response of the vegetation to temperature, water and
981 nitrogen stress but disregards additional causes of stress such as other nutrient
982 deficiencies, salt, heavy metals, ozone or UV radiation." in L142ff.

- We replaced "According to CSR theory, the stress gradient expresses the level of stress a species is exposed to in a certain habitat." with "According to CSR theory, stress is defined as constrained metabolic efficiency limiting biomass production and can be caused by a variety of factors (Grime, 1977)." in L270f
- added "Since the LPJmL model only represents a subset of possible stress factor (Sect. 2.1), only stress arising from temperature and water as well as nitrogen availability can be considered. Within LPJmL-CSR, some traits are linked to a more general response to stress, while other are used to represent adaptation to specific stressors. Since the grassland steppe sites that we simulated in our study are predominantly limited by water, we decided to focus on water stress in this first application of LPJmL-CSR. This allows for a better understanding of the underlying processes and the resulting patterns." in L276-280.

Any stress (including water stress - but also factors such as nutrient stress or 'non-resource' stressors such as temperature) limits metabolic performance and thus growth and biomass production. Internal, inherent metabolic traits (such as photosynthetic capacity and dark respiration rate) or growth traits (such as relative growth rate) would have been acceptable to demonstrate limitation, but the authors provide no evidence that, for instance, that specific adaptations determining canopy-level conductance can represent the extent of general tolerance to stress.

We agree with the reviewer that limited metabolic performance is the result of various types of stress. Depending on the complexity of the model, responses to stress can be computed internally (reduced growth rate or reduced photosynthetic capacity) if these are implemented as dynamic functions in the model responding to, e.g., non-optimal temperatures or nutrient limitations. In LPJmL, SLA is important to determine photosynthetic activity and therefore affects the growth rate (L175ff). The leaf-to-root ratio affects the photosynthetic activity as well by controlling the investments into additional leaves. Therefore, we do not only consider traits related to tolerance to water stress but also traits related to a general tolerance to stress. We realize that the original version of the manuscript may not have been sufficiently clear in this regard and improved the description of the role of the different traits and how they represent tolerance to stress in section 2.4.1.

We replaced "To represent the stress gradient, we used functional traits associated with resource use, which we defined as the abiotic dimension of our trait space. To represent the abiotic dimension, we selected the maximum transpiration rate (E_{max}), the minimum canopy conductance (g_{min}), the specific leaf area (SLA) and the leaf to root mass ratio (lmro). With "To represent the stress gradient, we used functional traits associated with the growth rate and water-resource use. We selected the maximum transpiration rate (E_{max}), the minimum canopy conductance (g_{min}), the specific leaf area (SLA) and the leaf to root mass ratio (lmro)." in L280-284.

We deleted The maximum transpiration rate (E_{max}) and the minimum canopy conductance (g_{min}) determine the amount of water transpired and distinguish between strategies that save leaves. The leaf to root mass ratio ($lmro$) describes the investments into above- vs. below-ground biomass and reflects the resulting plant specific resource availability. In L280.

We added

SLA: The specific leaf area is the ratio of leaf area to leaf dry mass and a measure of the amount of biomass required to produce a unit of leave area. It is predominantly associated

1028 with the stress gradient in the CSR theory. SLA is used within four processes of LPJmL-CSR:
1029 First, it is used to calculate the LAI, which controls light interception and thus productivity
1030 determining the area occupied by a PFT in competition with other PFTs. Second, SLA is used
1031 to determine the above-ground biomass of newly established seedlings from the seedling LAI
1032 (see explanation of LAI_{sapl}. Third, it is used to determine the actual mortality rate (A3).
1033 Fourth, it is used to calculate the leaf longevity controlling tissue turnover and litterfall (Sect.
1034 2.3.2). The SLA can used to determine the trade-off between short-lived, acquisitive and long-
1035 lived, conservative leaves. In contrast, in LPJmL 5 it was only used in the first and second
1036 process.

1037 lmro: The leaf mass to root mass ratio ($lmro$) is the target ratio of above- and below-ground
1038 biomass. It is predominantly associated with the CSR stress gradient but since it controls
1039 investments into above v. below-ground biomass it also affect the PFTs response to the
1040 removal of above-ground biomass. lmro is used within two processes of LPJmL 5 and
1041 LPJmL-CSR: First, to determine the allocation of the current day's productivity into above-
1042 and below-ground biomass pools to approach $lmro$. Second, to calculate the below-ground
1043 biomass of newly established seedlings from the above-ground biomass of newly established
1044 seedlings (A3). The lmro can be used to differentiate between strategies on investing
1045 assimilates for above- or below-ground growth and the resulting access to resources.

1046 E_{max}: The maximum transpiration rate defines the upper limit of transpiration per day. It is
1047 predominantly associated with the CSR stress gradient. In LPJmL 5 and LPJmL-CSR, E_{max} is
1048 used to calculate the water supply. Here, E_{max} presents the upper limit and actual transpiration
1049 is reduced depending on root the PFT-specific root distribution, atmospheric water demand
1050 and the soil water content. E_{max} can be used to distinguish different water saving strategies.

1051 g_{min}: This defines the minimum canopy conductance in mm per second that is independent of
1052 photosynthesis and a result of other processes controlling the lower limit of transpiration. It is
1053 predominantly associated with the stress gradient. In LPJmL 5 and LPJmL-CSR, g_{min} is used
1054 in the calculation of the total canopy conductance as a part of the photosynthesis routine. g_{min}
1055 can be used to distinguish different water saving strategies."

1056 in L284-306.

1057 We deleted "We introduced the root efficiency coefficient (k_{root}) as a substitute for
1058 information on root functional traits such as branching and density of the root network to
1059 account not only for root biomass but also the below-ground morphology of different species.
1060 The light extinction coefficient (k_{beer}) is a determinant for shading and used to distinguish
1061 large from small stature plants. The establishment rate (k_{est}) reflects the potential amount of
1062 offspring and the leaf area index of a sapling (LAI_{sapl}) represents the offspring size." in L307.

1063 We added "

1064 k_{beer}: The light extinction coefficient is a parameter describing the amount of light absorbed
1065 by a vegetation layer. It is predominantly associated with the CSR disturbance gradient but
1066 since it is used in the calculation of the FPC, which also determines resource access, it is also
1067 associated with the CSR stress gradient. In LPJmL 5 and LPJmL-CSR, k_{beer} is used to
1068 determine the FPC controlling the PFT-specific area share and its access to light. k_{beer} can be
1069 used as a proxy to distinguish large (rarely shaded by competitors and have high light
1070 absorption capacity) from small (potentially shaded by competitors and have high light

1071 absorption capacity only if dominant) stature plants and is essential for the competition for
1072 light and space.

1073 k_{root}: The root efficiency coefficient is a parameter used as a proxy for root functional traits
1074 such as branching and density of the root network. It is predominantly associated with the
1075 CSR disturbance gradient but it also affects PFT-specific water access. k_{root} was introduced
1076 in LPJmL-CSR and is used to represent the below-ground morphology controlling the PFT-
1077 specific share of the below-ground and its access to respective resources. k_{root} can be used as
1078 a proxy to distinguish sparse and constrained from dense and spread root networks and is
1079 important for the competition for water.

1080 k_{est}: The establishment rate describes the maximum amount of additional seedlings
1081 established per day. It is predominantly associated with the CSR disturbance gradient. While
1082 in LPJmL 5, k_{est} was used to determine the increase of the biomass of the average individual,
1083 in LPJmL-CSR, k_{est} is used to calculate the increase of the number of average individuals
1084 from establishment on bare ground area. k_{est} can be used to distinguish the number of
1085 offspring and thus reproductive capacity of different strategies.

1086 LAI_{sapl}: The seedling LAI is the leaf area index of a newly established seedling. It is
1087 predominantly associated with the CSR disturbance gradient. In LPJmL 5 and LPJmL-CSR, it
1088 is used to calculate the above-ground biomass of a seedling using the PFT-specific SLA. It
1089 can be used to distinguish the biomass of offspring which we use as a proxy for the
1090 competitive strength of the offspring of different strategies."

1091 in L315-335.

1092 We also agree with the reviewer that minimum canopy conductance and maximum
1093 transpiration rate do only relate to water stress. However, we selected four traits associated
1094 with the stress gradient to represent differences between the strategies. Two traits that are
1095 associated with general tolerance to stress through their importance for plant growth and two
1096 traits that are specific for water stress. As stated in a previous reply in L992f, we did not
1097 select additional traits that specifically relate to other types of stress that are represented in the
1098 model (temperature and nitrogen). With a better emphasis of our focus on water stress, the
1099 selection of traits relevant for water dynamics is hopefully more comprehensible. As already
1100 stated in our reply in L985-995, we agree that our description of the representation of
1101 different types of stress and our reasoning to focus on water stress needs to be improved. We
1102 included this in the changes we made regarding our reply in L1042-1013.

1103 See reply L1020-1038.

1104 Line 233: the authors state that "plant stature … can be used to distinguish C- and S-
1105 strategists (low disturbance tolerance) from R-strategists". No: S-selected species can be
1106 small (e.g. Salix herbacea) but some may become large over a long life-span (i.e.
1107 Sequoiadendron giganteum). What matters is the C-selected species get large quickly, S-
1108 selected species can become large eventually over a long life-span, and R-selected species
1109 cannot. This is more a reflection of longevity and how rapidly plants achieve adult size.

1110 We agree with the reviewer that S-strategists generally show a variety of statures as they
1111 nicely illustrated with their examples. This is also clearly stated in Table 2 of Grime (1977) to
1112 distinct species of the different strategies. While you can also find tall S-strategists in
1113 grasslands (e.g. *Brachiaria brizantha*), generally grassland plant species are of approximately

1114 similar height (Gommers et al., 2013; Pontes et al., 2015).
1115 Still, the reviewer raises an important point. If one would only consider stature, an S-strategist
1116 might not be clearly distinguishable from a C- or an R-strategist and our explanation can be
1117 misinterpreted this way. However, we are aware of the importance of growth rate and
1118 longevity when distinguishing C-, S- and R-strategists. To account for this, the LPJmL model
1119 represents the fast-slow economics of the LES as explained in more detail in our reply in
1120 L896-977. Furthermore, we do not prescribe plant stature. Instead, we use a parameter that
1121 just represents the potential stature a strategist can attain. Depending on abiotic and biotic
1122 factors, the C- and S-strategist can become large but the R-strategist cannot. The C-strategists
1123 will grow rapidly if sufficient resources are available. The S-strategist will grow slowly but
1124 accumulate large amounts of biomass over a longer time or remain small if it is disturbed or
1125 outcompeted.

1126 We restructured and amended section 2.4.1 and hope that the distinction between the PFTs is
1127 clearer now.

1128 We added "At the core of the model is the representation of growth dynamics including the
1129 assimilation and allocation of new biomass through photosynthesis and turnover of senescent
1130 tissue. Each day, the GPP is calculated dependent on radiation, temperature, water and
1131 nitrogen limitations for each PFT. Subsequently, NPP is computed by subtracting growth and
1132 maintenance respiration from GPP. In a third step, the assimilated carbon is distributed
1133 between leaves and roots to approach the prescribed optimal leaf mass to root mass ratio.
1134 Finally, senescent leaf and root tissue is transferred to the litter layer." in L144-149.

1135 In the present study only juveniles were investigated, so using the leaf area index of a sapling
1136 is not going to represent the strategy in the main vegetative phase (seedling CSR strategies are
1137 known to be different from adult CSR strategies; Dayrell et al. (2018) Functional Ecology 32,
1138 2730-2741).

1139 We agree with the reviewer that it is important to not only address CSR dynamics of juvenile
1140 plants. However, we would like to stress that we do not focus on juvenile plant dynamics. We
1141 assume that this misunderstanding originates in the description of establishment where
1142 saplings are established on bare ground. Still the model simulates an average individual that
1143 typically represents an adult plant (unless the entire plot has been re-established with new
1144 plants).

1145 We added "In LPJmL each PFT represent an entire population of adult plants using the
1146 concept of average individuals."in L136f.

1147 Also, CSR strategies are phenotypic characters (i.e. attributes of the individual plant that are
1148 subject to natural selection), but establishment rate (kest) [line 237] is not a character of an
1149 individual (the units of measurement are stated in Table 2 as the number of individuals per
1150 metre squared per day – a population measure), and so cannot elucidate the individual
1151 phenotype or adaptations of the species (i.e. the plant strategy or PFT).

1152 We agree that CSR strategies can be defined as a phenotypic characteristic of an individual
1153 and it may be counterintuitive that a measure that is not reported as being per individual but
1154 per meter squared can be used to represent a phenotypic characteristic. However, the
1155 establishment rate is just a parameter used within the model to calculate the actual
1156 establishment (appendix A3). This calculation considers several variables including the
1157 number of individuals and the resulting actual establishment can be reported as individuals

1158   per individual (a phenotypic characteristic). We see the point that this may be misunderstood.
1159   Also, LPJmL-CSR does simulate trait plasticity as well evolutionary processes. Therefore,
1160   phenotypic adaptation is not accounted for and adaptation only occurs at the community level
1161   through changes in its composition.

1162   We added "Each day new the number of average individuals of each PFT is increased if there
1163   is bare ground area available. The bare-ground area is distributed between established PFTs
1164   depending on their establishment rate k_{est}. The total amount of seedlings established is
1165   calculated based on k_{est}, accounting for the bare ground area. Subsequently, the number of
1166   average individuals is increased and the size of the individual specific carbon and nitrogen
1167   stocks is adjusted. LPJmL-CSR does not consider trait plasticity or evolutionary processes
1168   and therefore does not account for phenotypic adaptation This also means, that already
1169   established and newly establish average individuals share the same traits." in L244-249.

1170   In Figure 4, the red, green, blue (RGB) color scheme is used both to represent the extent of C,
1171   S and R and the experimental treatments rainfed (red), irrigated (blue) and fertilised (green).

1172   We agree that the coloring is not enhancing clarity and removed the colors from the axis and
1173   labels of the ternary plots.

1174   We removed the ambiguous coloring from the figure.

1175

1176   Additional changes:

1177   • We added "the original" in L13.
1178   • We deleted "availability of" in L40.
1179   • We replaced "the time frames" with "periods" in L74.
1180   • We added a comma between Sect. 2.3.2 and (Wright et al., 2004) in L182.
1181   • We added "Names, descriptions and usage of the model parameters are based on the
1182   model versions LPJmL4 (Schaphoff et al., 2018) and 5 (von Bloh et al., 2018)." in
1183   L268f.
1184   • We rephrased the caption of Table 2 as follows: "Parameter names, units, ranges,
1185   associated CSR gradient(s) and the hierarchy of the parameters for the C-, S- and R-
1186   PFTs."
1187   • We added "[…]so that different CSR strategies can be represented by the extended set
1188   of PFTs. The selected traits affect a variety of processes within the model and
1189   differentiate the C- , S- and R-PFT along the stress and disturbance gradients. The
1190   selected traits affect a variety of processes within the model and differentiate the C- ,
1191   S- and R-PFT along the stress and disturbance gradients" in L336ff.
1192   • We added "between" in L515.
1193   • We replaced the reference to Fig. SI 7 with the reference to Fig. SI 8 in L520.
1194   • We corrected the reference to the panels of Fig. SI 11 and 12 in L572, L575 and L588.
1195   • We replaced "In LPJmL, herbaceous plants are represented as a number of average
1196   individuals […]" with "In LPJmL, herbaceous plants are represented as average
1197   individuals of a number of different PFTs […]" in L830.

1198

1199   References responses

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