Answer Reviewer 1

We thank the reviewer for their thorough and constructive feedback. We are pleased that the methodological contribution of this study and the relevance of integrating fire and grazing management into LPJmL were appreciated. Below, we provide a point-by-point outline of how we plan to address each of the reviewer's comments in the revised manuscript.

All comments related to the figures are grouped together at the end. Minor comments, such as typos or small wording changes, are not listed individually but will be integrated as proposed.

Major comments:

> **The main thing I'm concerned about here is how grazed nitrogen is handled in the model. There's no citation in the Methods, as far as I noticed, pointing to information about how LPJmL grazing works at all, actually. N isn't mentioned alongside C at L131-133 as being partially returned to the soil via animal waste after grazing. That initially led me to think it wasn't. Then at L267, "manure (in grazing systems)" is mentioned as one input to the soil. But what fraction of the consumed N? And is it only feces or is it also urine?**
>
> **[…]**
>
> **If a realistic fraction of the grazed N isn't returned to the soil, I have serious doubts about the LPJmL model's fitness for the purpose of analyzing impacts of grazing on soil N and thus leaf C:N ratio too. See Selbie et al. (2015, DOI 10.1016/bs.agron.2014.09.004): "Ruminants excrete as much as 70–95% of the nitrogen (N) they consume."**

We thank the reviewer for pointing out this omission and fully acknowledge the lack of detail in the current manuscript. This was indeed an oversight on our part.

In our model, 66.7% of the nitrogen contained in grazed biomass is returned to the soil via excreta, primarily as urine. This value lies within the lower range of empirically observed values (typically 70–95%), which reflects the fact that livestock are not permanently present in the field. Our assumption accounts for periods when animals are housed or moved, during which excreta are not deposited directly onto the grazed area.

We have included this information in the Section 2.1.2, Managed grassland and grazing, as follow (l. 132) :

For nitrogen, 66.7% of the grazed nitrogen is returned to the soil, primarily as urine and dung, and is allocated to the NO_3 pool in the first soil layer. This value lies at the lower end of the empirically observed range of 70–95% reported by Selbie et al. (2015), reflecting the fact that cattle are not continuously present on the pasture. Periods during which livestock are housed or moved off-site are thus taken into account by this assumption.

> **Then again, in Fig. 10, neither manure nor urine are explicitly mentioned. They might be included in "harvest N" with that color showing net harvest N loss, but that's not explained. And finally, Fig. 11 does have manure again.**

Thank you for pointing this out. Figure 10 represents the ecosystem nitrogen budget, which includes only external nitrogen fluxes. Since manure is not treated as an external nitrogen input in our model setup, but a conversion of plant to soil nitrogen, it is not included in this figure. The component 'harvest N' in Fig. 10 contains only the nitrogen that is grazed and removed from our model domain so that it does not include the nitrogen in the manure.

In contrast, Figure 11 focuses on the soil nitrogen budget, which explicitly accounts for manure as an input to the soil nitrogen pool. The distinction between these two perspectives is clarified in the figure captions, both of which refer to the section “Nitrogen in- and out-fluxes” (previously Section 2.4.3, now moved to Section 2.1.4), where the compositions of the budgets are described in detail.

To clarify the point raised, we have added explicitely that manure does not contribute to the ecosystem budget in its description (l.154):

The ecosystem nitrogen budget is determined by the balance of nitrogen inputs (biological nitrogen fixation and atmospheric deposition) and outputs (leaching, denitrification, volatilisation, plant uptake, harvested nitrogen and NO_X emissions from fire). Harvesting nitrogen occurs in grazing system, and it excludes the part returned to the soil as manure. Emissions from fire occurs in burning scenarios.

> **If the N analyses are kept, some text needs to be added to the Methods describing biological nitrogen fixation (BNF) in LPJmL. Are both symbiotic and asymbiotic BNF represented? How do they work?**

We thank the reviewer for raising this important point. We acknowledge that the current version does not provide sufficient information on this important process.

The current implementation in our model is relatively simple. BNF is calculated from the 20-year average of annual evapotranspiration following the function from Cleveland et al. (1999). The resulting BNF is added to the NH_4^+ pool in the first soil layer. The description of this process as well as the equation are added in the section 2.1.3, Soil nitrogen pools, as follow (l. 143):

BNF is calculated from the 20-year average of annual evapotranspiration (etp; in mm yr^{-1}) following the empirical relationship from Cleveland et al. (1999); von Bloh et al. (2018a) (Eq. 1) and does not distinguish between the symbiotic and asymbiotic. The resulting BNF is added to the NH_4^+ pool in the first soil layer.

$$BNF = \begin{cases} \max\left(0, \dfrac{0.0234 \cdot \text{evapotranspiration} - 0.172}{10 \cdot 365}\right) & \text{if } C_{root} > 20\,\text{g C m}^{-2} \\ 0 & \text{otherwise} \end{cases} \quad (1)$$

__Other comments__

> **1) I would like to see some text added to the Discussion or Conclusion about what work would be needed in order for this feature to become commonly enabled in LPJmL runs. Is it just livestock density maps (both historical and for future scenarios) that are holding it back?**

The main prerequisite for applying the pasture burning version of LPJmL and the Chalumeau module in other regions is a solid understanding of local burning practices. In this study, we focused on Brazil, where we could base the implementation on existing literature and documented fire management practices. While the pasture burning functionality itself is flexible and can be technically applied to other regions, its meaningful use requires region-specific calibration regarding fire season. Indeed, the broader application of the Chalumeau algorithm would require a careful reassessment, especially in temperate regions. The algorithm relies heavily on climate inputs and seasonal patterns, which may not translate well across different biomes without further development and validation. We have added a few lines addressing these aspects in the newly titled "Limitations and Outlook" section of the manuscript (l. 464):

Applying the pasture burning version of LPJmL and Chalumeau module beyond Brazil would require region-specific information on fire management practices. While the current implementation is technically flexible, meaningful application in other regions depends on adapting fire-use assumptions. In particular, the Chalumeau algorithm, which is driven by seasonal and climatic constraints, should be carefully re-evaluated before application to temperate regions.

2) The Appendices are strange. Appendix A has only one figure—why not combine the two Appendices? That figure, Fig. A1, is also the very last to be mentioned in the main text (and only as "Sec. A", not "Fig. A1"); it should thus be last in the Appendix as well. Finally, there are four Appendix figures not mentioned in the main text: Figs. B2–3 and B5–6.

We acknowledge the inconsistency in Appendix structure and figure referencing. Appendices A and B have been merged into a single section. We have revised the order of figures so that Fig. A1 (currently referenced last) appears at the end and we have ensured that all Appendix figures are explicitly referenced in the main text with appropriate labels.

3) All figures except Fig. 1, or at least certain labels in those figures, seem have JPEG artifacts. In most cases these should be replaced with entirely vector-based figures (.eps or .pdf). Failing that, PNG should be used. JPEG should only be used for photos (and don't just convert JPEGs to PNG!).

We have revised all figures accordingly. Specifically we have replaced current figures with vector formats (.PDF).

4) The "matrix"-type figures (Figs. 4–9) need a fair amount of work:

a. All of these figures should have color bars. It should also be made obvious when subplots share a color bar.

b. There are many cells in these figures with black text on a dark background. This should be avoided, for instance by adding a white outline or "glow" around all text that overlays a non-white background.

c. Some of these seem to be true values, while others are changes relative to a baseline. This is hard to keep track of and introduces an extra mental load in interpreting them. Please consider standardizing on one or the other.

d. In some of these, white represents "excluded because of insufficient biomass for grazing," whereas in others it's burgundy. This should be standardized to burgundy (or even better for colorblind readers, black). I say this because white is confusing: In Figs. 4-5, the lightest color (yellow) is low impact, the darkest color (dark red) is high-impact, and pure white is the highest impact of all. The color scale goes light-dark-light.

e. Most of the cells in these matrices represent a "bad" impact, so it makes sense they are represented by a yellow-red color scale. However, some represent a "good" impact —e.g., some treatments in Fig. 8 showing soil N enrichment. In such cases, they should not be on the same color scale, because they are qualitatively different. Something other than yellow-red should thus be used—e.g., blues.

f. An extra column should be added to the right side of every matrix giving the results with no fire.

We appreciate the detailed suggestions and have incorporated them into the revised version. We are aware that the complexity of these figures, due to the large volume of data, can hinder readability. Improving their clarity is a priority, as it enhances both the accessibility and the overall understanding of the paper.

All mosaic figures now include a color bar. The color scale has been changed to blue–white–red, as mentioned in point (e), to clearly represent both positive and negative impacts. This adjustment also prevents dark text from appearing on a dark background.

We fully agree that the current mix of true values and relative changes can be difficult to follow and adds unnecessary cognitive load. Following your suggestion, we have standardised all results to be expressed as relative values, using the undisturbed scenario as the reference. Since the focus of the figure is on the impact of burning, we believe that expressing results as percentage changes relative to the reference scenario improves clarity and facilitates interpretation. As a consequence, including an additional column with the values of the no-fire scenario is no longer relevant.

6) L157-166 (Sect. 2.2.1): Since the Brunel et al. (2021) and Waha et al. (2012) papers are not open-access, more detail should be given here (or in an Appendix/Supplement) on the Chalumeau algorithm and its implementation. For instance, how often is the burning date updated? Does it use a rolling window to calculate seasonality variables? What's the difference between the "burning date" vs. the strategies (e.g. "early spring")? Etc.

Thank you for the suggestion. We have included additional details on the Chalumeau module in the Appendix A to improve clarity and accessibility. Briefly, Chalumeau identifies the dormant season and extracts annual burning date using daily temperature and precipitation data. A 10-day moving average of temperature and a 10-day accumulated precipitation sum are used to identify periods of temperature- or precipitation-driven dormancy, respectively. These metrics distinguish the dormant season as either a cold (winter) or dry period. Depending on the selected burning strategy, one representative burning date is then assigned per grid cell and year within the identified dormant season.

8) L224-233: Is other fire allowed to happen during these experiments? I.e., are the only ignitions allowed due to intentional pasture management burning, or is the rest of SPITFIRE operating at the same time?

Only the burning practices are applied to the pasture during the simulated experiment. Other fires do not occur. This choice was made to simplify the experimental setup and to focus on the analysis exclusively on the effects of intentional burning. We have clarified this point in the section 2.3.2, Model configuration and experimental setup adding this sentence (l.245) :

To keep the experimental setup and the analysis as simple as possible, burning practices are the only fires applied to the pasture during the experiment.

9) L227-229: It's not until this sentence until I understood what this paragraph was supposed to be describing; until then I was pretty confused. Please move it to the top of the paragraph and edit as needed for flow.

Upon re-reading the section, we agree that the structure was somewhat confusing. We have moved the respective sentence to the beginning of the section (l.224), hoping that this will better guide the reader through the description of the experimental setup.

10) L230-231: I'm not sure I understand this correctly. How many replicates does this result in? 2 + 5 + 10 = 17?

Indeed, the number of replicates corresponds to the length of the burning frequency: the 2-year frequency scenario includes 2 replicates (starting in year 0 and year 1), the 5-year frequency includes 5 replicates (starting from year 0 to year 4), and the 10-year frequency includes 10 replicates (starting from year 0 to year 9). We have clarified this in the manuscript by adding the following sentence in (l.248):

This results in 2, 5, and 10 so 17 replicates for the 2, 5, and 10 year burning frequency scenarios respectively.

11) L233: Are the four strategies something that the Chalumeau algorithm produces for each gridcell? Or are they things you switch between for different experiments?

Thank you for this question. The burning strategy is provided as an input parameter to the Chalumeau algorithm for each experiment. Based on the selected strategy, Chalumeau determines the corresponding burning dates. We have clarified this point in the Appendix A, as mentioned in our response to comment 6.

14) L262-268 (Sect. 2.4.3): Why is this in the "Post-processing" section? It would be more appropriate near Sect. 2.1.3 ("Soil nitrogen pools").

We agree that the paragraph does not fit well within the "Post-processing" section. We have moved it in the section 2.1 LPJmL modelling concept and it labels now as section 2.1.4. We think that this new placement ensures better alignment with the content and improves the overall structure of the Methods section.

17) L288: What nitrogen deficit? It hasn't been previously established that NPP is Nlimited under any conditions or treatment.

Thank you for this observation. We have revised the sentence to clarify that the nitrogen deficit in leaves is a result of the burning practices (l.300):

Burning practices lead to a nitrogen deficit in leaves, significantly affecting the nutrient balance of the vegetation.

19) L297–299: How is this result possible?

This result is indeed discussed in more detail in lines 420–433 in the Discussion section. To summarise it, the difference in responses between biomass pools (AGB) and fluxes (dry matter intake) arises from their distinct sensitivities to disturbances. While AGB shows a gradual, cumulative decline over time due to repeated grazing and burning, DMI (a flux) initially drops but stabilises as the system adapts. This is because DMI responds more directly to the balance between net growth and losses: as biomass decreases, fire intensity and thus disturbance also diminishes, allowing DMI to reach a new, stable equilibrium despite lower overall biomass.

To improve clarity for the reader, we have now included a brief indication in the Results section to highlight the key factor behind this result and guide the reader to the relevant discussion (l.312).

This pattern results from differences in the response of biomass pools and fluxes to repeated disturbances, as discussed in Section 4.

> **20) (L298) Fig. 4d: Why were no scenarios excluded (colored white) due to biomass being too low for grazing?**

As mentioned in Section 2.4.1, scenarios are excluded when the average dry matter intake falls below 80% of the livestock's feed requirement. Biomass levels can be lower, but still sufficient to meet this threshold. The reasoning behind this result is discussed in the Discussion section (lines 420–433) and summarised in our response to Comment 19.

> **21) L304:**
> **a. Why does recently-established grazing have such a higher average dry matter intake?**
> **b. Are these numbers for the "no burning" treatment specifically?**

Thank you for your question. Yes, these values refer specifically to the no burning treatment. The higher average dry matter intake under recently-established grazing results from the fact that vegetation biomass is still relatively high at the beginning of the simulation, before the full effects of grazing disturbance accumulate. After reworking the manuscript and deciding to present relative values, we chose to remove references to absolute values to avoid confusion.

> **22) (L311) Fig. 6:**
> **a. It'd be nice to have the site names in the figure title, as is done for similar figures.**

Thank you for pointing this out. Initially, the site names were placed to the side of the figure, since Fig. 6 exceptionally displays results for two sites simultaneously. However, to improve consistency and clarity, we have now also included the site names in the figure title, in line with the formatting of similar figures.

> **b. C:N normalized to a percentage feels wrong, I think because it's a result of both the numerator and the denominator. Consider changing to actual values instead of percentage.**

We understand the concern. Our choice to normalize C:N ratios was motivated by the need to focus on the relative impact of grazing and burning across sites with very different baseline conditions. As explained in Section 2.4.2, the C:N ratios are normalized using the reference C:N value for each site under undisturbed conditions. This approach enables meaningful comparisons of treatment effects across heterogeneous environmental contexts. See as well our answer to the point 4.c).

> **23) L316-317: What is this "optimum" value? Optimum for what?**

Thank you for the comment. The "optimum" value refers to the soil C:N ratio considered ideal for promoting healthy nutrient cycling . We have clarified this in the revised text (l.329):

In fact, even in undisturbed scenarios, the soil in the Cerrado site is not rich in nitrogen, as indicated by a soil C:N ratio of 16.65, which is above the target value of 15 considered an optimum for maintaining nitrogen availability to plants (Gerber et al., 2010).

25) L321: What "initial nitrogen deficit"? Deficit relative to what?

Thank you for the comment. We have clarified in the manuscript that we are referring to a soil nitrogen deficit (l.334):

However, in the recent disturbance scenario, we notice that the introduction of burning practices helps to alleviate the initial soil nitrogen deficit, decreasing the C:N ratio up to 1.6% without grazing and with frequent burning.

26) (L329) Fig. 8:
a. Title and first sentence of caption should say that these numbers represent the change in C:N ratio. That's different from all the other such figures.

Title and the caption have been changed accordingly.

b. Cells with negative values (indicating enrichment) should not also be yellow. Maybe a light blue instead.

As noted in our response to comment 4.e), the color scale of all matrices has been changed to blue–white–red, with positive values shown in blue.

c. C:N change as a percentage feels wrong, I think because it can result from either a change in the numerator or the denominator. Consider changing to actual values instead of percentage.

For details, please see our reply to points 4.c) and 22.b).

d. Subplot (d): Why were no scenarios excluded (colored white) due to biomass being too low for grazing?

We kindly refer the reviewer to our response to point 20.

29) (L338) Fig. 9b, d: Why were no scenarios excluded (colored white) due to biomass being too low for grazing?

This is addressed in our response to point 20, to which we would like to refer the reviewer.

30) L343-344: This statement doesn't seem to be true for grazing alone, except maybe for the Caatinga site.

The sentence seems to be misleading. We have readjusted it (l.355):

In the first 20 years after the introduction of an intensive disturbance (first group of bars on the left), such as 2 year burning frequency, nitrogen losses are at their maximum.

31) (L346) Fig. 10:
a. This figure was very confusing at first. I was eventually able to understand it: The colors only represent N loss mechanisms, so the top of the bar is N inputs (as the authors mention), and then the colors go down from there, with the bottom of the bar representing the N balance. I think that last point should be explained in the caption. It is much more typical for these kinds of plots to have inputs stacking on top of the zero

line and losses below, with a star or something to note the net flux.
b. And indeed, that's what you do in Fig. 11! I strongly suggest switching Fig. 10 to this format, using the same colors for deposition and BNF as in Fig. 11

Thank you for this valuable feedback. We agree that the current version of the figure can be confusing at first glance, particularly regarding the nitrogen inputs, as they are not explicitly shown. The original figure focuses solely on the nitrogen deficit. To improve transparency and enhance understanding, we have decided to revise the figure to display the nitrogen inputs, namely BNF and atmospheric deposition. The revised figure follows the same structure as Figure 11, with nitrogen inputs shown above the zero axis and nitrogen outputs below.

c. Dark blue color label should be "Nitrification + denitrification".

The label has been corrected.

32) L351-352: Are you saying that both BNF and litterfall are directly attributable to the plant C and N pools? Is that because LPJmL doesn't represent asymbiotic BNF?

Thank you for pointing this out. The original sentence was indeed misleading. We intended to say that BNF and litterfall originate from the vegetation pools. In the model, both are attributed to the soil nitrogen budget: BNF is allocated to the NH_4^+ pool, while litterfall contributes to the SOM-N pool. We have revised the sentence accordingly (L364):

In the case of the soil nitrogen budget for the Cerrado site, the BNF and the litterfall, directly derived from the plant nitrogen pools, are the primary input fluxes.

For clarification regarding BNF, LPJmL5-Pasture-Burning produces a crude estimate of total BNF without distinguishing between fixation mechanisms and without accounting for the energetic cost to the plant for nitrogen fixation.

34) L355-356: My interpretation is that this is because low-biomass plants can't "pay" for much symbiotic BNF—is that right? This should be explained.

As explained in our response to the main comment, the BNF computation in LPJmL does not account for symbiotic BNF. On rereading, we recognise that the original phrasing was misleading, and we therefore propose the following revised version (l.370):

In the Caatinga region, extreme water stress strongly constrains vegetation pools. This is reflected in Fig. 11 c, where litterfall and uptake fluxes are markedly lower than in the other locations. Across all scenarios, the net ecosystem and soil nitrogen budgets remain negative. Indeed even without fire practices, the ecosystem and the soil experience a net nitrogen loss (Fig. 10 c) and Fig. 11 c).

35) L356:
a. This statement doesn't make sense in the context of Fig. 11, which deals with soil N only, not ecosystem N.
b. The "primarily due to minimal grazing" part of this statement is not supported by Figs. 10 or 11. Fig. 10c suggests that most N losses from the Caatinga system are due to leaching, not grazing (harvest N loss). While the harvest N bars are needed in most cases to drop the net flux below the zero line in Fig. 10c, that figure only shows the result for the Caatinga with grazing on. It can't be safely assumed that ecosystem or soil N flux would be positive without grazing, because without grazing many parts of

the system are changed. The authors performed more experiments than are shown here, but if those experiments support this assertion, they should be presented somewhere and cited here.

Thank you for highlighting this inconsistency. We agree that the original statement was misleading in the context of Fig. 11. Indeed, simulations without grazing also show a net nitrogen loss, indicating that grazing is not the only cause of the negative nitrogen balance. We have revised the text to better reflect the findings shown in Fig.10 c and avoid unsupported claims (l.371) :

Across all scenarios, the net ecosystem and soil nitrogen budgets remain negative. Indeed even without fire practices, the ecosystem and the soil experience a net nitrogen loss (Fig. 10 c) and Fig. 11 c).

36) (L358) Fig. 11:
a. Please add a star or something to each bar representing the net N flux.
b. Dark blue color label should be "Nitrification + denitrification".

The figure has been revised. Stars now represent the net N flux, and the legend label has been updated accordingly.

37) L369-371: What drives the ratio of allocated C:N and how it changes after disturbance?

The allocation process follows a strict, non-linear scheme influenced by changes in water and nitrogen availability. We prescribe a target range for the C:N ratio in leaves and roots. Variations in water and nitrogen affect the actual allocation to ensure values remain within this prescribed C:N ratio range. At the same time, water and nitrogen availability influence leaf mass, thereby altering the leaf-to-root mass ratio. Disturbances therefore do not primarily target the C:N ratio itself but rather the leaf-to-root ratio, which in turn can affect the C:N ratio. The underlying mechanism is complex and depends on multiple interacting factors. As this is beyond the scope of the present paper, we have chosen not to provide a detailed analysis here, but we added a short clarification in the manuscript (l. 385):

The difference occurs during the regrowth phase, when the balance of allocation shifts from carbon to nitrogen dominance. The observed shifts result from a non-linear allocation scheme driven by water and nitrogen availability. Over time, the total amount of nitrogen assimilated by the plants decreases compared to the amount of assimilated carbon.

38) L371: Again, what "deficit"? Are plants N-limited?

Thank you for pointing out this misleading sentence. With respect to the plant C:N ratio, it is true that in some locations vegetation pools experience a nitrogen deficit, with C:N values above the optimum. However, the sentence was intended as a general statement. We therefore propose the following revised formulation (l. 384):

Over time, the total amount of nitrogen assimilated by the plants decreases compared to carbon.

39) L378-382:
a. Note that it's still not a beneficial effect even in the Pampas. I think "in wetter regions like the Pampas, fire and grazing can coexist with higher vegetation productivity" should thus be struck, or at least strongly modified. Even the highest-

biomass cell in Figs. 5c-d (i.e., grazing + fire in the Pampas) represents a 17% reduction of leaf biomass relative to the control treatment.

We agree with your point. The text has been revised accordingly to avoid suggesting a beneficial effect of fire and grazing in the Pampas (l. 394):

In wetter regions like the Pampas, fire and grazing can coexist with smaller impacts on vegetation productivity, whereas in drier areas like the Cerrado and the Caatinga, these disturbances severely impact pasture health.

40) L393: "Sec. A" should be "Appendix A". Also, why is Fig. A1 in an Appendix Section all by itself? Wouldn't it be simpler to just have one Appendix with all additional results? In addition, am I right that this is the first time Fig. A1 is mentioned anywhere? In that case it should come later in the Appendix, because other Appendix figures have already been mentioned.

Thank you. The Appendix section has been revised, and we kindly refer the reviewer to our response to point 2 for further details.

41) L409: "an initial reduction in intake" when? With the introduction of fire?

Thank you for pointing out this imprecise sentence. The initial reduction in intake mentioned here indeed occurs with the introduction of fire. The sentence in the manuscript has been revised as follows (l. 422):

Our results indicate that under moderate livestock density, there is an initial reduction in intake with the introduction of fire […] .

47) Throughout:
a. Use of "significant" implies a statistical test when none was performed. This mostly happens in figure captions but is also present in the main text

Thank you for pointing this out. We have revised the text and figure captions to avoid the use of "significant" in contexts where no statistical test was performed, in order to prevent any confusion.

Answer Reviewer 2

We thank the reviewer for their thoughtful and constructive comments, which help us improve the clarity, transparency, and scientific robustness of the manuscript. We appreciate the recognition of the methodological contribution and relevance of our study. Below we outline how we plan to address each of the specific comments in the revised version of the manuscript.

> ***1.) Provide a supplementary table that lists every fixed parameter, its value, units, and reference/justification. This will satisfy transparency and help future users turn the feature on in standard LPJmL runs.***

We fully agree with the importance of parameter transparency. Given the large number of parameters involved in the LPJmL model, we would follow this approach:

We have added a new supplementary table listing all parameters that are specific and most relevant to this study. This includes parameters related to the Chalumeau algorithm, the pasture and grazing management system (e.g., fraction of manure returned to the soil), and the nitrogen cycle (including BNF). The table reports parameter names, values, units, and where available the corresponding references or justifications. This table is included in the appendix as Table B1 (l.253):

More specific parameters for this study are listed in the supplementary table B1.

To complement this, the general model parameters, i.e. unmodified LPJmL5 settings, are available in the publicly accessible LPJmL-Pasture-Burning version repository. A direct link to the code is provided at the end of the manuscript in the section code availability. In addition, we have added a reference to it in the Model configuration and experimental setup subsection as follow (l. 252):

A complete list of the general parameters used during the simulations can be found in the configuration files of the model, which are available via the link provided in the Code and data availability section.

We believe this approach ensures both completeness and accessibility while keeping the main manuscript concise.

> ***2.) In the nitrogen-budget discussion the authors state that nitrogen inputs "consist of BNF and atmospheric deposition," and the decline in soil N under burning / grazing is "driven primarily by reductions in BNF. However, there is no description of how BNF is computed (symbiotic vs. asymbiotic, dependence on plant N demand, moisture, temperature, etc.) nor any parameter values.***

We thank the reviewer for pointing out the lack of detail regarding the representation of BNF in the manuscript. We acknowledge that the current version does not provide sufficient information on this important process.

The current implementation in our model is relatively simple. BNF is calculated from the 20-year average of annual evapotranspiration following the function from Cleveland et al. (1999). The resulting BNF is added to the NH4+ pool in the first soil layer. The description of this process as well as the equation are added in the section 2.1.3, Soil nitrogen pools, as follow (l. 143):

BNF is calculated from the 20-year average of annual evapotranspiration (etp; in mm yr^{-1}) following the empirical relationship from Cleveland et al. (1999); von Bloh et al. (2018a) (Eq. 1) *and does not distinguish between the symbiotic and asymbiotic. The resulting BNF is added to the NH_4^+ pool in the first soil layer.*

$$BNF = \begin{cases} \max\left(0, \frac{0.0234 \cdot \text{evapotranspiration} - 0.172}{10 \cdot 365}\right) & \text{if } C_{\text{root}} > 20\,\text{g C m}^{-2} \\ 0 & \text{otherwise} \end{cases} \quad (1)$$

We have also noticed that Figure 10 might be difficult to interpret, particularly regarding the nitrogen inputs, as they are not explicitly shown. The original figure focuses solely on the nitrogen deficit. To improve transparency and enhance understanding, we have decided to revise the figure to display the nitrogen inputs, namely BNF and atmospheric deposition. The revised figure follows the same structure as Figure 11, with nitrogen inputs shown above the zero axis and nitrogen outputs below.

A) Include the key BNF parameters in the requested parameter table (fixation efficiency constants, maximum rates, etc.).

As mentioned in the answer of the point 1, all parameters regarding BNF are included in the requested parameter table.

B) If possible, show one sensitivity test (e.g. ±20 % maximum BNF rate) to demonstrate that the qualitative C: N conclusions are robust.

We appreciate the reviewer's suggestion to include a sensitivity test to assess the robustness of our conclusions. Our manuscript already presents extensive analyses and adding additional sensitivity tests would likely come at the expense of clarity and readability, while also being computationally demanding given the scale and complexity of the current model setup. Moreover, such an analysis would go beyond the scope of the present study.

That said, we can reasonably anticipate the direction of the effect. The calculation of BNF in LPJmL does not use a maximum rate, but we could instead change the empirical parameters of the equation. Increasing the slope or the intercept would enhance BNF fluxes, thereby increasing nitrogen inputs into the NH_4^+ soil pool. Conversely, a decrease of the parameters would reduce nitrogen inputs, potentially accelerating the decline of soil nitrogen pools. Higher nitrogen input leads to lower C:N ratio and at contrario lower N input leads to higher C:N ratio. However, this does not translate linearly into plant growth, as higher nitrogen availability also accelerates soil transformation processes such as immobilization, denitrification, and leaching.

While we are confident that such a test would not change the qualitative conclusions of our study, we acknowledge its potential value and consider it a relevant avenue for future work.

3.) Nitrogen cycling clarity - The current text never states what fraction of grazed N is returned as dung/urine or how urine is handled, calling the soil-N results into question.

A. Give the exact fractions and pathways in Sect. 2.1.2.

B. Cite a data source and show that the chosen value falls within the empirical 70–95 % range.

We thank the reviewer for pointing out this omission and fully acknowledge the lack of detail in the current manuscript. This was indeed an oversight on our part.

In our model, 66.7% of the nitrogen contained in grazed biomass is returned to the soil via excreta, primarily as urine. This value lies within the lower range of empirically observed values (typically 70–95%), which reflects the fact that livestock are not permanently present in the field. Our assumption accounts for periods when animals are housed or moved, during which excreta are not deposited directly onto the grazed area.

We have included this information in the Section 2.1.2, Managed grassland and grazing, as follow (l. 132) :

For nitrogen, 66.7% of the grazed nitrogen is returned to the soil, primarily as urine and dung, and is allocated to the NO3 pool in the first soil layer. This value lies at the lower end of the empirically observed range of 70–95% reported by Selbie et al. (2015), reflecting the fact that cattle are not continuously present on the pasture. Periods during which livestock are housed or moved off-site are thus taken into account by this assumption.

> ***C. Run a quick sensitivity test (e.g. +20 % manure-N return) and state whether conclusions change.***

We thank the reviewer for this valuable suggestion. As previously mentioned in our response regarding the BNF sensitivity test, the manuscript already includes extensive analyses and running additional simulations is computationally expensive.

Nonetheless, we can anticipate the likely effects of a ±20% change in the manure-N return fraction. Increasing the proportion of nitrogen returned to the soil would enhance nitrogen inputs to the NO_3^- soil pool. Conversely, a reduction in this return fraction would decrease nitrogen inputs, accelerating the decline of soil nitrogen pools.

> **4.) This study does not use any experimental field data for calibration or validation. Its findings are based entirely on mechanistic simulations, and while it emphasizes the importance of future field research, it would benefit from moving that discussion into a dedicated ‘Limitations and Outlook’ subsection**

Thank you for this helpful suggestion. We have renamed the “Conclusion” section to “Limitations and Outlook” and merged it with the final part of the Discussion (starting from l. 437). In this revised section, we have added a paragraph explicitly addressing the lack of experimental field data for calibration or validation of the burning practices. While acknowledging this limitation, we have also mentionned that the LPJmL model and its internal dynamics have been previously validated, and we have provided the corresponding reference (l. 445):

This study does not rely on experimental field data to calibrate or validate the results specific to burning practices. Indeed datasets documenting the impact of burning on vegetation structure, yields, and soil carbon and nitrogen content are either unavailable or entirely lacking for Brazil and the various locations analysed here. However, the LPJmL model itself, along with the underlying process dynamics it simulates, has been previously evaluated (Thonicke et al., 2010; Schaphoff et al., 2018a, b; von Bloh et al.,2018).