

# ***Interactive comment on “Modeling vegetation and carbon dynamics of managed grasslands at the global scale with LPJmL 3.6” by Susanne Rolinski et al.***

## **Anonymous Referee #2**

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In this study, the authors have implemented four grassland management schemes in the model LPJmL, and run global simulations with these four schemes varying some of the parameters. The authors aim to demonstrate the need for DGVMs to include grassland management because its impact on NPP and soil carbon. For this they analyze the effects of each grassland management system on three variables, grass harvest, NPP and soil carbon in a bioclimatic context depending on average temperature and precipitation. In a second time, the analysis focuses on potential applications of the modeling schemes and a comparison of the simulations with European data provides a validation of the order of magnitude of grass yields. Then the authors derive maxi-

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mum livestock grazing densities.

**#General comments** The modeling approach and the scenarios defined are interesting and the results can bring some light into the role of grassland management on ecosystems functioning. However, the description of the modeling approach and underlying processes are not always very clear and should be more linked to the simulations' results. The description of the model is not very detailed and is disconnected from the results that never link the simulations' results with the model's structure causing these results. This link needs to be stronger. The authors argue for the lack of data for model validation but still there should be an effort in explaining how parameters were selected (no mentioning of calibration) in the absence of validation data.

Also, the presentation and description of the results are sometimes too superficial and should be improved for the reader to get the full benefits from this study. In particular, the study is lacking a proper discussion section that explains and interprets the results that are so far only shown and described in a raw fashion.

Also the abstract is somewhat misleading on the main results of the paper. The main result highlighted in the abstract is not the main result developed in the results section. Also, the comment on application of LPJmL for global meat production seems too far a perspective to be in the abstract.

Overall, this work done for this study can be interesting for the geoscientific modeling community but efforts must be made in results presentation

**#Specific comments**

**-Methods** The stand concept used to define vegetation types is not described. What characteristics are homogeneous within a stand that are not within a PFT? In equation (1), what are exactly the variables  $L$ ,  $R$  and  $l_r$ . If they are as described in the text above, then  $L/r=R$  so  $R-L/l_r=0$ . A deeper explanation of this equation is needed. Also it is not clear how the optimal leaf to root ratio plays a role. In equations (3)-(6), parameter

values are set to 1 without stating it in the equations making it confusing. Please write in the equation when you put  $\alpha_{leaf}=1$  and  $N_{ind}=1$ , and explain in the text what is the meaning of setting these values to 1. Also, for  $I_{rp}$ , on line 14p6, it is said that it is PFT-specific and set to 0.75. Is it 0.75 for all grassland ecosystem?

-Calibration Many parameters are used in the model and there is no reference as to where they come from. An example is in equations (3)-(6).

-Results About the presentation of results, the graphical items used to display the results sometimes make the reading hard to follow. The maps and temperature/precipitation (T,P) graphs are redundant. Maps are not bringing any additional information since the modeling is too simplified to give realistic values (no map of grassland, spatial homogeneity of management practices, no fertilization & irrigation, no water feedback) except for some reader who are looking for specific values in some data. Maps can even be misleading. For example on fig.5b, the areas for which the difference is negative seem unrelated when in the T,P plots of fig. 6b, it is clear that there is a similar process in these regions due to their bioclimatic conditions. Moreover maps are difficult to compare visually to each other. They should be moved to appendix. To allow for mental representation of geographical distribution, the separation lines between T,P areas appearing in fig. 2 should be reported in later T,P plots to allow for rapid bioclimatic regions differentiation. Also, even with maps in appendix, because maps and T,P plots show the same information (even if aggregated by deciles of precipitation and temperature in the latter) it would help the reader to use the exact same color scale for both families of plots showing a similar variable. The sequential green/blue color scale used for T,P plots in fig 4 is less likely to introduce an artificial visual bias than the divergent color scale in fig 5.

In several occasions in the text, grass yield and soil carbon patterns are explained from their relation to NPP. The described relationship is not visible from the data shown making the text impossible to follow for the reader. Graphs of yield versus NPP and soil carbon versus NPP would help convince the reader of the significance of the trends

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and relationships described.

Color scale used in the difference plots is counter-intuitive with increase in cold colors and decrease in warm colors. Also, the color scale is too close from the one with absolute values to see right away that what is plotted is a difference. Fig. 5a and 10a are not described, what is described and should be shown (in appendix) is the difference in harvest between scenarios for consistency with text.

The authors attempt to compare their simulation to regional data in Europe. This exercise is very ambitious given the level of simplification of the model, in the spatial homogeneity, diversity of scenarios and processes involved. However it can be an informative comparison if well explained and described. For example, the reason for choosing to compare only the highest harvest GD simulations with data is not explained. If it is supposed to be the more realistic given current practices it should be justified. An interesting result would be to show which management setting leads to the best simulation in each subregion and try to explain it.

About the results in general, the article lacks an analysis of the results. The discussion section is about effects on soil carbon, uncertainties and further developments in the modelling approach. If all these discussion points are interesting, after a very descriptive results section, the reader is also expecting an interpretation of the results, as a full discussion with explanation of the underlying processes and implications. For example, what processes in the model drive the feedbacks? Some of the feedbacks are the simple expression of the relationships coded in the model and this should be identified and its realism described. What is the reason for the pattern in fig. 6,8 & 11 (climatic area  $10 < T < 20$  &  $1200 < P$ , pattern mentioned but not explained in the text)? Does the soil parameterization (texture) play a role in the results or other ignored variables?

#Editorial comments In introduction the abbreviation Mio for Million is not the conventional one. Fig. 1 : axis labels need to be more explicit. P4|23 typo : “1 and 1” P6 text in 2.3.1 introduces 2.3 but not 2.3.2 P6L3 sentence not clear P6|28 ‘are used’ instead

of 'is used' P13L4 the sentence 'average grass yield and soil carbon under these conditions are not substantially different' is confusing. It sounds like grass yield and soil C are the same. Rephrase, maybe use 'homogeneous'.

Fig. 9 please make the figure visually lighter by using only one color bar per row.

P17L21 rewrite sentence

p19L11 not clear, rewrite sentence

p19L16 'low correlations', 'high standard deviations and RMSD'. Give the numbers.

P22L05 why compare LSUmax to scenario M and not default D as in all the rest of the manuscript ?

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