Geoscientific
Model Development
Discussions

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

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Overview

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 maximum livestock grazing densities.

Thank you for the concise overview. The last item of deriving potentials was misleading also for another reviewer so that we changed and extended the approach (description under specific comments). Our responses are inserted below, following their original comments.

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.

We acknowledge that a storyline from methods over results to the discussion is only weak and should be much clearer linking underlying model assumptions to resulting trade-offs between productivity, harvest and carbon sequestration in the soil. Therefore, we include

1. more precise description of the methods including formulae. Therefore, P6L13 to P6L15 is exchanged with
   'Partitioning of assimilated carbon B_I to leaves and roots is calculated in a way that a given leaf to root mass ratio is approximated. The PFT-specific parameter lr_p is 0.75 for both grasses (Sitch et al., 2003), i.e. that 0.75 times leaf carbon equals root carbon under optimal conditions. lr_p is scaled to the actual ratio lr (Eq. 1) with a measure of water stress (actual ratio of plant water supply W_{supply} to atmospheric water demand W_{demand}) (Eq. 2 in Sitch et al., 2003). Under dry

conditions, this scaling results in a lower lr so that the allocation of assimilated carbon is shifted towards the roots.

$$lr = \max(0.25, lr_p \cdot W_{supply}/W_{demand})' \tag{1}$$

and more detail is given instead of P6L24 to P6L25:

'After a harvest event, leaf carbon and thereby lr is reduced. Carbon allocation in the following period will try to reestablish the actual leaf to root mass ratio lr. Depending on the water supply to demand ratio, the assimilated carbon is incorporated more or less to the leaves so that the actual water conditions and NPP determine the recovery time of the leaves. Without a feedback to primary productivity, a 10 % reduction of the water supply alone would result in a slower recovery time of several days and leaves would have less carbon when the new lr is established. Hence, even more important is the feedback on primary productivity connected to the leaf carbon content.'

Also the source of the parameters is given explicitly after P7L1:

'Some of the parameters have standard values such as $\alpha_{leaf} = 1$ for grasses, i.e. grass leaves are assumed to be photosynthetically active for 1 year (Eq. (6) in Sitch et al., 2003), $N_{ind} = 1$, i.e. that one average individual is considered (as set in Sitch et al., 2003), and $k_b = 0.5$, as established in the literature and recently confirmed by Saitoh et al. (2012) (Eq. (7) in Sitch et al., 2003).'

The section now ends with an estimate for the effects of carbon removal from the leaves:

'This dependency of light absorption and photosynthesis on leaf carbon content leads to a negative feedback of harvest on absorbed radiation. When leaf carbon is reduced to 50 %, the reduction of fAPAR is about 30 % for $L = 100$ gC m^{-2} and is diminished to 2 % for $L = 500$ gC m^{-2}.'

2. We rewrite most of the results section and end each subsection with an assessment of the processes leading to the simulated results. For the default option,

this would read like:

'The frequent mowing option D generates nearly optimum grass yield in all regions with a minimum productivity. As soon as regrowth occurs, leaf biomass is removed without an additional or residual flux into the soil. Productivity under high temperatures and precipitation is even enhanced by the harvesting because of the comparatively high residuals (Fig. 1) for high leaf biomass. The feedback of leaf carbon content to photosynthesis favours plant regrowth in these regions because leaves after the harvest are growing exponentially. Cold and low productive regions with low respiration and turnover provide the only environment in which high values of soil carbon are reached under option D.'

3. We include a better reference to the discussion of strengths and weaknesses of process formulations in the beginning of the results section:

'The effect of the harvest options are described for grass yield, NPP, and total soil carbon of the 3 m soil column and analyzed with respect to the underlying processes (see also discussion on strengths and weaknesses of the chosen approach in section 4.2). '

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.

We agree that although the lack of global benchmark data is a drawback, the parameter choices should be better motivated and the discussion on our parameters in relation to other approaches should be better visible. We will extend the text accordingly, see below for specific comments and section 4.2.

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.

We see that the combination of result description with a reference to the processes causing these results will enhance the readability and value of this section. We decided to improve the presentation of the results by rewriting the section and including a paragraph in each subsection about the underlying processes that lead to the simulated results (see item 2 above).

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.

We see that the abstract is not adjusted to the main results and the reference to global meat production a bit far-fetched. We will adjust the abstract accordingly.

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

Thank you for this overall assessment. In the following, we will try to meet your recommendations.

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?*
  The concept is defined in section 2.2 (P4L29) and explained in more detail in section 2.2.2 (P5L27 to P5L31) and 2.3.1. We include references to the first description.

- *In equation (1), what are exactly the variables L, R and lr. If they are as described in the text above, then $L/r = R$ so $R - L/lr = 0$. A deeper explanation of this equation is needed. Also it is not clear how the optimal leaf to root ratio plays a*

C5

role.
  We see that the description was not informative and adjust the definition of lr also with an equation (see item 1 on page 2). The parameter lr is the actual and changing target for carbon allocation between leaves and roots so that usually $L/R \neq lr$ either because the water supply to demand ratio changed or the plant compartments changed after a harvest event. We include more explanation of the effect of the variability of lr when describing the effect of a harvest event and illustrate the feedback of leaf carbon reduction to photosynthesis at the end of the paragraph.

- *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 $Nind = 1$, and explain in the text what is the meaning of setting these values to 1. Also, for lrp, on line 14p6, it is said that it is PFT-specific and set to 0.75. Is it 0.75 for all grassland ecosystem?*
  We remove the setting from the text, include the values and their consequences into the equations and add one sentence before the equations (see item 1 on page 2) to give the source of the chosen values. We hope that this was intended by the remark.

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).*
  We acknowledge that the background of the parameter choices has to be made clearer. Most of the parameter values are already described in the literature on the implementation of processes in LPJmL which is stated now clearly in section 2.3.1.The mentioned equations (now 4 to 7) are reported as published in Sitch et al. (2003) and we clarify now which equations and parameters are taken from

C6

this publication.

For those parameters which are introduced for accounting for grassland management, values are estimated from literature. These are discussed in section 4.2 which is referred to in P7L17. We did not apply a calibration technique such as Bayesian parameter estimation or another fitting procedure because we did not find a reliable global dataset to be used in such an exercise. We admit that the chosen parameters may not be ideal but with this model development presented here, we aimed primarily at the representation of grassland management which in future can be made use of to derive spatial distributions of management options and intensities. One example of the potential combination of modeling and data analysis was presented by Chang et al. (2016) but goes beyond the scope of this paper. We hope to clarify this point when stating the aims and objectives of the implementation in exchange with P3L27 to P3L32 and explicitly point out that parameters are not calibrated here, which could be included in future work with appropriate reference data (e.g. in regional studies):

'Without being able to represent actual grassland management at this stage, we are aiming with this implementation at the following objectives:

- comprehensive representation of the diversity in grassland management and in related feedbacks between biomass removal and primary productivity,

- demonstration of the role of grassland management for biogeochemical simulations by analyzing the effects on grass yield, Net Primary Productivity (NPP) and soil carbon stocks,

- assessment of potentials of agricultural productivity by determining maximum harvest and the associated livestock densities with and without the condition of maintaining soil carbon stocks.

- evaluation of model performance by comparing simulated harvest with an European data set (Smit et al., 2008) and potential livestock densities with

data from the Gridded Livestock of the World v2.0 (Robinson et al., 2014).'

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.*
We take the point that with spatially homogeneous assumptions on management the resulting effects can be investigated best in relation to their climatic drivers as in the figures according to Fig. 2. Thus, we move the maps (formerly figs. 3, 5, 7, 10) to the appendix and concentrate on the T,P plots.

- *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.*
We appreciate this comment and will include black lines denoting the regions into the T,P plots.

- *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.

We acknowledge this comment and use the color scale of Fig. 4 also for those maps which show actual values and not differences (formerly figs. 3a, b, c, 5a, 7a, 10a).

- *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 and relationships described.*

The trade-off between the harvesting or sequestration in the soil of assimilated carbon is not very simple to illustrate. We tried to find a way for the visualization (Fig. 1) and came up with a possibility to distinguish average harvest in relation to soil carbon and NPP in 2 ways. For a low livestock density (upper row), the ratio of harvest to the feed demand is only lower than 97% (or 0.97 in the figure) when NPP is below 50 gC m^{-2} a^{-1}. At medium NPP values, very high soil carbon values occur but for most of the grid cells (dark color in upper right plot) soil carbon values are below 20 kgC m^{-2}. At high grazing pressure (lower row), the demand cannot be fulfilled when NPP is below 400 gC m^{-2} a^{-1} and the occurrence of soil carbon values above 40 kgC m^{-2} is less often. In case, this diagram is helpful, we will include this representation in the results section.

- *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.*

We appreciate the attention paid to the presentation of the results but in this case, we keep the chosen view. When temperatures are displayed, the connection to 'cold' and 'warm' colors is fair but usually blueish colors are perceived as positive and reddish as negative. Thus, we have chosen this (color-blind safe) combi-

C9

nation for the presentation of the differences. And we think that the same color scale for the actual values match very well with the positive values in the difference plots which can be distinguished easily by the color legend beside each figure.

- *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.*

Thus, harvest maps for options M, G_D and G_R (formerly figs. 5a, 7a, 10a) are exchanged by difference maps and included in the appendix.

- *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.*

We take this comment as a reason to revise the description of the comparison and to include a new figure. Section 3.2 is now following a reasoning including 3 hypotheses and concentrates on 3 selections from the simulation results to test these . The section heading will be 'Comparison to harvest data' and the motivation will be extended to:

'Since management assumptions for the simulations were spatially homogeneous and management in Europe is known to vary spatially as well as temporally, we use the comparison to find out whether climate- and management-induced variations in grass harvest can be captured by the applied options. Therefore, we formulate and test the hypotheses that European grass harvest

C10

1. can be achieved by grazing animals only,

2. is determined by management and only to a minor degree by climate and

3. per geographical entity, a dominant management option can be identified that results in similar harvest values as reported.'

The text for hypothesis 1 will use Fig. 14 b with

'For testing hypothesis 1, we choose for each regional value from the simulation results for option G_D with varying livestock densities the maximum harvest value (see section ??, Fig. ?? b) which resulted mostly from simulations with medium stocking densities. Clearly, the pattern differs from the reported yield estimates (Fig. 14 a). The gradient of reported yields from northeast to southwest is underestimated and yields are higher in southern Europe and lower in the western parts of the continent. Thus, a continuous withdrawal of leaf biomass could achieve higher grass harvest in eastern Europe and the Mediterranean whereas for yields in western Europe (esp. Great Britain, The Netherlands and Norway) much higher values are reported than simulated. Therefore, we can reject hypothesis 1 and, thus, support the assumption that grassland management in Europe is not homogeneous concerning the presence of animals on the pasture or the harvesting intensity.' For hypothesis 2, Fig. 14 c is used:

'Hypothesis 2 cannot entirely be resolved with our simulation results but we test whether the reported gradient from northwest to southeast can be reproduced. From simulations with option G_D, we select per region the harvest flux closest to the reported values (Fig. 14 c) so that the livestock densities can be inferred that lead to the observed harvest values. The resulting pattern matches the reported values below 260 gC m^{-2} a^{-1} which are occurring in most of southern and eastern Europe as well as Scandinavia apart from Norway. Values are only underestimated in Great Britain, The Netherlands, Ireland and Norway on highly managed grasslands, e.g. which are fertilized and irrigated. Comparing those regions in which maximum values (Fig. 14 b) are the closest to reported values

(Fig. 14 c) but still more than 10 % too low, apart from the 4 countries mentioned before, German and French provinces appear. We interpret this as strong indicators for intensively managed systems. On the other hand, regions in which maximum harvest by grazing overestimates reported values by 50 % are located mainly in East European countries (Slovakia, Montenegro, Macedonia, Lithuania, Hungary, Croatia, Estonia, Belarus, Bulgaria, Albania). There, the potential of grass production is by far not utilized and climatic conditions are not limiting. Hypothesis 2 can be confirmed by these findings.'

The inclusion of Fig. 2 (as Fig. 14 d) allows to relate to hypothesis 3 with:

'For testing hypothesis 3, for each geographical region the closest value from all available simulation results is chosen (Fig. 14 d). The derived values deviate from those in Fig. 14 c in the highly managed countries identified before (Great Britain, The Netherlands and Ireland) and some regions in Finland, Germany and France. The options that result in closer values are the default option D and mowing M both describing pasture regimes with additional harvesting to increase yields. Regions in which reported yields are higher than in Fig. 14 d can thus be identified as definitely under regimes including other yield increasing measures such as fertilization and irrigation. Therefore, also hypothesis 3 can be justified by analysing the simulation results.'

- *About the results in general, the article lacks an analysis of the results.*
  We hope that with the inclusion of paragraphs on the underlying processes and their implementation in the model as well as with the changes in the comparison to harvest data (section 3.2) and in the derivation of potentials (section 3.3), the necessary analysis of the results is improved.

Discussion

- *The discussion section is about effects on soil carbon, uncertainties and further developments in the modelling approach. If all these discussion points are in-*

teresting, 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?

The called for analysis of the results will be included in the results section in order to give insight into reasons for the patterns in the mentioned figures. We hope that these changes are satisfying.

0.1 Editorial comments

- *In introduction the abbreviation Mio for Million is not the conventional one.*
  'Mio' is changed to 'm'.

- *Fig. 1 : axis labels need to be more explicit.*
  Labels of figure 1 are changed at the x-axis to 'Pre harvest leaf carbon' and at the y-axis to 'Post harvest carbon partition'.

- *P4l23 typo : "1 and 1"*
  This is no typo but a misleading sentence that we change to
  '5 soil layers with a thickness of 0.2, 0.3, 0.5, 1.0 and 1.0 m, respectively.'.

- *P6 text in 2.3.1 introduces 2.3 but not 2.3.2*
  We introduce
  'a daily allocation routine for the assimilated carbon which is a prerequisite for the'
  into the last sentence to overcome this omission.

- *P6L3 sentence not clear*
  We assume that the lack of clarity refers to the 'stand' and include a reference to the definition of the stand concept at the end of the sentence with '(concept see 2.2)'.

- *P6l28 'are used' instead of 'is used'*
  Plural form is corrected.

- *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'.*
  We delete the sentence within the new description of the results.

- *Fig. 9 please make the figure visually lighter by using only one color bar per row.*
  We take this point and will adjust the figures.

- *P17L21 rewrite sentence*
  The sentence is changed and split into two:
  'Particularly high losses of soil carbon are simulated in cold regions (annual mean temperatures below $0\,^\circ$C) (Fig. 9 c). There, soil carbon for low stocking densities (0.4 LSU ha^{-1}) decreases by 2.5 $\pm$ 2.8 kgC m^{-2} and for higher densities (2 LSU ha^{-1}) even by 19.9 $\pm$ 8.2 kgC m^{-2}.'

- *p19L11 not clear, rewrite sentence*
  This sentence was deleted when section 3.2 was completely rewritten.

- *p19L16 'low correlations', 'high standard deviations and RMSD'. Give the numbers.*
  Also this sentence will be deleted when section 3.2 is completely rewritten. Numbers for standard deviations and correlation coefficients from the Taylor diagram are then included in the description of Fig. 13.

- *P22L05 why compare LSUmax to scenario M and not default D as in all the rest of the manuscript?*
  The reasoning for this comparison is given in section 2.6.2 which is extended to: 'This comparison was chosen because under the mowing option neither biomass removal is maximized nor is harvested carbon added to the soil so that a rather moderate impact on soil carbon stocks is expected compared to grassland without harvest.'

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