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Geoscientific Model Development Discussions

# *Interactive comment on* "Simulating the effect of tillage practices with the global ecosystem model LPJmL (version 5.0-tillage)" *by* Femke Lutz et al.

**Femke Lutz et al.**

femke.lutz@pik-potsdam.de

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Dear Editor and Referees,

We would like to thank the two Referees for their valuable comments on our manuscript that will help to improve the manuscript and to rectify a major shortcoming of the original implementation (omission of tillage effects on infiltration).

In this letter we list the referees' comments, each point followed by our responses. We are able to address all points raised by the reviewers, have already extended the model implementation and will be able to provide a revised manuscript soon. The responses have been derived in consultation with all co-authors.

Best regards,



Tobias Herzfeld and Femke Lutz

Referee #RC1

The authors have developed a module in LPJmL, which simulates some biophysical effects of tillage on carbon, water and N2O fluxes. The work is very relevant to GMD and the wider scientific community, enabling a wide range of applied studies. However, I believe the manuscript should not be published. The main reason is that in the model development for water fluxes important processes were either neglected or misrepresented to the point that effects might be not only uncertain, but possibly wrong. In the following, I will explain this in more detail.

Referee comment 1: In the proposed model, soil moisture changes are affected by an increase in bulk density in no-tillage (NT), reducing infiltration rates. The problem is, that while this is fine in theory, hydraulic conductivity based on bulk density is not related to runoff production in any meaningful way. The evidence is that runoff generation is hardly affected at the local scale, and decreases with increasing field scale, sometimes dramatically (Shipitalo et al 2000). Reviews show that the effect is very variable, but on average, on plot or field scale, NT reduces runoff (Leys et al 2010, Armand, 2009), while the proposed model suggests an increase in runoff. The authors acknowledge that processes such as preventing crusting by residues and preferential flow might increase infiltration, but they do not implement these important processes, and as a consequence they get the effect wrong.

Answer 1: Thank you for pointing to this. Indeed, we had missed to address the positive effect of NT on infiltration as well as to clearly communicate our results and the underlying mechanisms. As suggested by the reviewer and literature, infiltration is enhanced under no-tillage as soil crusting is reduced and preferential pathways are affected. As a result, surface runoff decreases on average. We now further extended the model to include improved infiltration rates under residue cover. To do this, we follow the approach by Jägermeyr et al. (2016), equation 1, which has been developed for implementing in

situ water harvesting, e.g. by mulching, in LPJmL and is suitable for global-scale model applications. Given that a direct modeling of soil crusts depends on a lot of detailed information that is not available at the global scale (e.g. precipitation intensity, which is e.g. needed for the Green-Ampt infiltration routine) and modeling attempts are often found to be unsatisfactory even at field scale (Nciizah and Wakindiki, 2015), we believe that the simple approach described by Jägermeyr et al. (2016) is more suitable and also allows for more systematically addressing model uncertainties. In the now revised tillage implementation, the infiltration rate is dependent on the residue cover, so that infiltration is increased with increasing residue cover. With this revised implementation of tillage, we find a substantial reduction in surface runoff (-55.2%) under NT_R compared to T_R. We note that this modification of infiltration has moderate, but generally positive effects on the simulation of the other stocks and fluxes and simulation results are now closer to the reported literature values (e.g. CO2 and N2O emissions and crop yields). We will also revise the manuscript with respect to clarity, as we have only reported total runoff (surface runoff + lateral runoff + seepage) whereas only surface runoff is reported in the suggested literature.

Referee comment 2: Figure 1 suggests a link between residue cover and infiltration, which they do not model. Instead they model residue interception, which they term infiltration because they treat the residue layer as a soil layer. This is confusing, because interception is a very different process from infiltration. Residues are labelled as 'layer' into which infiltration can happen. This is contrary to the common definition of infiltration, which is water flux across the soil surface. This is not merely a semantic problem. Interception effects alone reduces infiltration into the actual soil, while their model scheme suggests an increase in infiltration. Residues do have a positive effect on infiltration, of course, through reducing sealing / crusting, but they do not model this.

Answer 2: We agree that the term infiltration was not used correctly here. In the manuscript, the term will be changed to residue interception as the water is infiltrating into the first soil layer after passing through the residues (if present) and part of the



rainfall will thus be intercepted. The residue cover is not treated as a soil layer. We will correct and clarify this in the manuscript, including Figure 1. For the comment related to the positive effect of residues on infiltration, we would like to refer to our response on the first issue raised by reviewer 1. The model now intercepts part of the rainfall in the residue layer but has increased infiltration capacities under residue cover. These changes to the model implementation will be reflected in the updated version of Figure 1.

Referee comment 3: CO2 emissions decrease in the short term with NT, and increase in the long-term. Unless there is more C-input with NT, this is not a reasonable outcome, and also contrary to literature, as the authors suggest themselves. If there is more C-input for NT, the authors need to be clear about it. If there is more C-input (from increased NPP), that would also be inconsistent with meta-analyses of yields, which generally find no significant difference with tillage.

Answer 3: CO2 emissions are variable in space and time and indeed subject to different drivers, which are covered by our model. For the majority of cropland areas (median reported in Table 4 of the manuscript), CO2 emissions initially decrease after introducing no tillage (NT_R vs. T_R), which is consistent with literature and theory, as pointed out by the reviewer. In table 4 of the original submission, we had accidentally reported the values after 10 years not after 2 (year 1-3 average). There are two explanations for CO2 to increase in the long term under no-till: 1) there is more C input for NT from increased NPP or 2) the decomposition rate is higher under NT over time, due to changes in e.g. soil moisture or temperature. When looking at the initial response, CO2 emissions indeed decrease almost everywhere. After 10 years, CO2 emissions continue to decrease in most humid regions, whereas they start increasing in drier regions. These are also the regions where we observe a positive effect of reduced evaporation and increased infiltration on plant growth, i.e. in these regions the C-input is substantially increased under NT-R compared to T-R. The relative differences of residence time of soil carbon for NT-R compared to T-R are relatively small

(+1,5% after 10 years), but show similar patterns, i.e. the residence time decreases in drier areas but increases in more humid areas. As such, both mechanisms that affect CO2 emissions are reinforcing each other in many regions. This is in agreement with the meta-analyses conducted by Pittelkow et al. (2015), who report yields (and thus general productivity and thus C input) often to be equal or higher in NT than in CT in dry climates after 5-10 years. Their results show that in general, NT performs best relative to CT under water-limited conditions, due to enhanced water use efficiencies when residues are retained. We will modify the text to clarify the effects of NT on the processes that affect CO2 emissions and to describe in detail how different responses in CO2 emissions in space and time can be explained.

Referee comment 4: For N2O, the authors acknowledge the uncertainty of the equations they use. However, the problem is compounded by the uncertainty of (and possibly wrong) effects calculated for soil moisture.

Answer 4: Simulating N2O emissions is very challenging, as there is a high spatial and temporal variability of the flux. Moreover, it is very sensitive to soil moisture and it is therefore indeed important to correctly simulate this. The modification of the infiltration function as described in the response to account for residue effects on crusting (see the response on comment 1) has an effect on soil moisture, and therefore on N2O emissions; the N2O emissions increased from +7.5% to +18.3% under NT which is closer to the value reported by Mei et al. (2018), who reports an increase of 36.1%. When looking at climate regions, we find that the simulation of N2O emissions are closer to the observed value of Mei et al. (2018) for all regions, except for the cool- and humid zones, where Mei et al. (2018) report a small decrease, whereas the new infiltration scheme further increases N2O emission in our model. The uncertainty of simulating N2O emission by the model will have to be further investigated, by conducting e.g. pixel analysis / sensitivity studies, but lies beyond the scope of this study. The mechanisms leading to the increased N2O emissions under NT as well as the spatial patterns and uncertainty will be better explained in the revised manuscript.



Referee comment 5: In summary, in the current form, some of the process representations are insufficient leading to wrong or very uncertain effects. The authors do mention some of these processes in the discussion, but there is no justification that despite these omissions we can trust the modelled effects. There are some more general remarks about the manuscript.

Answer 5: We believe that, with the modified infiltration function (see response to the first comment), the representation of processes is improved. In the manuscript, we will more explicitly point out uncertainties and where correct responses to tillage practices are found. We are convinced that the now updated version of the model is a substantial improvement compared to previous model version of LPJmL (where tillage was not addressed at all), which is our main aim in this model description paper. We also think that a detailed representation of tillage effects in a global crop, hydrology and dynamic vegetation model is a general advancement of modeling capacities. We can demonstrate that many modeled effects of tillage are within expected ranges and we explicitly address uncertainties and lack of agreement with reference data in the revised manuscript. We think that the ability of the model to reproduce diverse responses in space and time are an asset of the model, reflecting the diversity in outcomes also in experimental data and the importance of climatic, soil and management conditions.

Referee comment 6: Cursory reading of important literature: this is evident in the two first points made above. Also, for water fluxes, only the aspect of reduced soil evaporation is compared to a single study. Also they claim that there are no models for crusting effects and no other PTFs with SOC, which is not true (Zhang et al. 1995, Risse et al 1995, Balland et al 2008). For SOC change very few of the more recent SOC meta-analyses were referenced. I suggest to use the citations of Ogle et al 2005 as starting point, see also Haddaway et al 2017.

Answer 6: We agree that the existing literature was not sufficiently reflected in the original submission of the manuscript and we will expand that in the revised version. From the various sources who reported the effect of residues on soil evaporation

(Balwinder-Singh et al., 2011; Gava et al., 2013; Ranaivoson et al., 2017; Steiner, 1989), only Steiner reported a suitable function of the reduction of evaporation depended on residue amount. Soil crust formation and adjustments of K due to crusting as proposed by Risse et al. (1995) and Zhang et al. (1995) are calculated from cumulative kinetic rainfall energy, which is currently not available at the global scale. Please also see refer to issues 1 and 2. We will also update the description of PTFs and better justify our choice.

Referee comment 7: The manuscript is misleading at parts. The start of section 2 suggests model development which was not implemented in the way described. Please be very clear in all parts of the manuscript what was implemented and what not. Also some parts of the discussion gloss over the problems with the partiality of model development, and the problems with the literature comparisons they make (please refer to the specific comments). Moreover the section on tillage effects on bulk density was nearly literally taken over from APEX/EPIC model, this should be acknowledged more clearly (not just with a reference to the model documentation).

Answer 7: Thank you for the specific comments. We will address all of them in the revisions and update the manuscript in the parts that are misleading. We see that clarification is especially needed in regard to the water fluxes, as already discussed in comment 1 and 2 as well as in the description of model implementation and model evaluation against reference data. Additionally we will also be more specific on what we define as tillage and no-till and if residues are retained. We will also refer in the text to the APEX/EPIC model and that the equation we used is adapted from the APEX/EPIC model.

Referee comment 8: The authors should reformulate the last paragraph of the introduction as (a set of) objectives. Is the objective just to describe the new module? I think the evaluation of the module is an objective, too.

Answer 8: We will add the objectives to the introduction. The objective of this study



is to 1) describe the new tillage module in LPJmL in full detail and 2) evaluate the new tillage module against literature values reported in meta-analyses using a set of stylized management scenarios.

Referee comment 9: Although not specified, the objective of the paper is to describe a new tillage module in LPJmL. They state that this has been done so in other models, but (presumably) to an unsatisfying level. The authors need to be very specific about the state-of-the-art tillage modelling in global dynamic vegetation models / gridded crop growth models, and how (if at all) they improve on existing formulations.

Answer 9: One of the objectives of the paper is indeed to describe a new tillage module in LPJmL. The objective is not to propose a new approach to represent tillage in crop models, but rather to describe the extension of LPJmL so that management effects on biophysical processes and biogeochemistry can be better represented in LPJmL. The implementation is guided by existing modeling approaches and we have extended LPJmL in a way that is more process-based than other approaches (e.g. Pugh et al., 2015), but still suitable for global-scale applications, in which calibration is strongly impeded by the lack of reference data and several driving data are not available (e.g. rainfall intensity, management practices). The choice for representing tillage at a process level rather than simple scaling factors introduces additional uncertainty, which we acknowledge and discuss. However, it also allows for improved understanding, e.g. by comparing different soil properties to reference data, and for accounting for the spatial heterogeneity in soil, climate and management conditions. We do not intend to predict the effects of tillage with high certainty, which is not possible at the global scale with the associated lack of detailed data. Instead, the main purpose is to enhance the understanding of the complexity of tillage effects at the global scale and to upscale findings from field experiments. This will be better described and discussed in the revised manuscript. We will also expand the introduction by discussing the state-of-art of tillage modelling in DGVMs and crop growth models.

Referee comment 10: The methodological explanation on the comparisons between

NT and T results (4.2) and the subsequent comparisons can be improved, currently this requires 2 or 3 re-readings.

Answer 10: Thank you. We will rephrase section 4.2 for clarity.

Referee comment 11: Specific comments: See annotated pdf in supplement to comments. Answer 11: Thank you for the specific comments. We will revise the manuscript accordingly.

Referee #RC2

Referee comment 12: General comments: The work presented in this paper putting forward a 'tillage' module for LPJmL model version 5.0 is the perfect fit for GMD as a journal enabling outreach to a wider community of ecosystem, earth system and atmospheric modelers. It is definitely a crucial addition in the suit of tools that enable evaluation of soil N and C dynamics resulting in CO2 and N2O emissions and how they are impacted by agricultural management practices like, tillage in conjunction with other practices like use of residue cover. However, after going through the GMD Discussions draft submitted, it seems to require some major revisions in terms to addressing the scientific assumptions to modeling approach used in this proposed module, before it can go ahead for final publication.

Answer 12: Thank you for the positive general assessment. We will modify the manuscript for better clarity in scientific assumptions and in the overall presentation of results.

Referee comment 13: There are some structural discrepancies in how some processes that are not actually modeled as listed in Fig 1 are still listed in the text as explanation for model performance. Authors need to address those discrepancies.

Answer 13: Thank you for your comment. We will strictly focus only on processes which have been implemented into the model and clarify the specific sections. We now further included a mechanism of improved infiltration in relation to residue cover. In



the revised manuscript we will be more specific on the processes which are actually modelled and update Figure 1 in this regard. We will make sure that a process that is not included in our implementation, but is referenced as an explanation of missing model skill, is clearly described as a missing process.

Referee comment 14: More discussion on soil Nitrogen pools and their dynamics with different management strategies along with soil Carbon pools is needed both in terms of explaining the N2O emissions better, as well as adding differentiation between C and N parts of SOM in equations by incorporating C:N ratios.

Answer 14: Thank you for this comment. We realize that we have not sufficiently described which functionality refers to carbon and/or nitrogen pools and fluxes and will update the equations and the description accordingly for better clarity.

Referee comment 15: It would be helpful to list in a tabular form or in form of figures, as to how the proposed tillage module improves upon the already available modeling approaches for effects of Tillage on SOM dynamics, soil properties, crop yield, CO2 and N2O emissions.

Answer 15: We will update this section and discuss in more detail on how our implementation is different from already existing model approaches. The main objective of this paper is to present the updated version of the process based LPJmL5 model, which has now been improved by including tillage management and a detailed interaction of processes of residues effecting soil water content. We will explain this in more depth in the text including the difference between our approach and that of other global models (see also answer 9). A comparison of how tillage is modeled in other biophysical models can also be found in Lutz et al. (2019), which we will refer to in the revised manuscript.

Referee comment 16: Effects of tillage on Bulk density is adapted from APEX v0806 model (http://epicapex.tamu.edu/files/2014/10/APEX0806-theoretical-documentation.pdf), however it seems it has been assumed that Bulk density after

tillage = Bulk density after soil completely settles, which is not necessarily accurate all the time after Tillage. Moreover, this assumption is really not highlighted in the text.

Answer 16: We indeed assume that the bulk density after tillage can reconsolidate over time to its original bulk density prior to tillage. The rate of reconsolidation depends on the infiltration rate and sand content of the soil. We are aware of the problem of the so called fluffy soil syndrome, which is the results of tillage without a wetting cycle under dry condition (Daigh and DeJong-Hughes, 2017). However, this topic is not yet very well researched. Since in our model soil consolidation is dependent on water infiltration, in dry regions the soil possibly does not consolidate back to its original state in all cases. This effect can also vary from year to year. Any negative implications from this effect are not yet part this tillage model implementation. We will revise the manuscript to more clearly describe the dynamics of changes in bulk density after tillage and the gradual reconsolidation over time.

Referee comment 17: There can be huge uncertainty in terms of how CO2 and N2O emissions are effected by additional management practices adopted with Tillage or No tillage, under short or long term analysis for different crops and climate with varied soil properties. This needs to be explained more as to how such kind of uncertain behavior can be explained by proposed LPJmL-Tillage version 5.0 currently and what aspects need more work in the future.

Answer 17: Indeed, different environmental conditions (soils and climate) and management practices other than tillage strongly determine the effects of tillage on soil properties and thus also CO2 and N2O emissions. The process-based representation of tillage effects in the LPJmL model allows for representing the complex and diverse effects of tillage across environmental and management gradients, which are reflected in the broad variability in N2O and CO2 responses under different tillage experiments. Indeed, we also find that the modeled impacts of tillage are very diverse in space as a result of different framing conditions (soil, climate, management) and feedback mechanisms, such as improved productivity in dry areas if residue cover increases plant



available water. It is indeed important to understand which processes lead to uncertainties in those fluxes. Therefore, we evaluated model performances by using results of meta-analyses. Even though there are not enough reference data to verify the simulated spatial patterns in e.g. CO2 and N2O emissions, we think that the ability of the model to reproduce a broad diversity of tillage effects in response to environmental conditions is an asset of the model that can be employed to explicitly study soil management and associated biogeochemical effects, including uncertainty. We will extend the general discussion on these aspects and will point out future research needs to better address those uncertainties.

Referee comment 18: "Specific comments" addressing the scientific issues including but not limited to the above general overarching comments with the modeling approach and analysis to be addressed, are all listed in detail in the attached Supplement (gmd-2018-255_RC2.pdf).

Answer 18: Thank you for the specific comments. We will revise the manuscript accordingly.

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