Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-255-RC1, 2018

Geoscientific Model Development Discussions

Open Access

GMDD

Interactive comment

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

Anonymous Referee #1

Received and published: 3 December 2018

General comments:

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 However, I believe the manuscript should not be published.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.

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 pro-

Printer-friendly version

Discussion paper

duction 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.

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.

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.

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.

In summary, in the current form, some of the process representations are insufficient

Printer-friendly version

Discussion paper

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.

5. 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.

6. The manuscript is misleading at parts. The start of section 2 suggest 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).

7. 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.

8. 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.

9. 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.

Specific comments: See annotated pdf in supplement to comments.

References:

Armand R, Bockstaller C, Auzet AV, Van Dijk P. 2009. Runoff generation related to intra-field soil surface characteristics variability: Application to conservation tillage context. Soil and Tillage Research, 102: 27-37. DOI: https://doi.org/10.1016/j.still.2008.07.009.

Balland V, Pollacco JAP, Arp PA. 2008. Modeling soil hydraulic properties for a wide range of soil conditions. Ecological Modelling, 219: 300-316. DOI: https://doi.org/10.1016/j.ecolmodel.2008.07.009.

Haddaway, N.R., Hedlund, K., Jackson, L.E. et al. Environ Evid (2017) 6: 30. https://doi.org/10.1186/s13750-017-0108-9

Leys A, Govers G, Gillijns K, Berckmoes E, Takken I. 2010. Scale effects on runoff and erosion losses from arable land under conservation and conventional tillage: The role of residue cover. Journal of Hydrology, 390: 143-154. DOI: https://doi.org/10.1016/j.jhydrol.2010.06.034.

Risse LM, Nearing MA, Zhang XC. 1995. Variability in Green-Ampt effective hydraulic conductivity under fallow conditions. Journal of Hydrology, 169: 1-24. DOI: https://doi.org/10.1016/0022-1694(94)02676-3.

Shipitalo MJ, Dick WA, Edwards WM. 2000. Conservation tillage and macropore factors that affect water movement and the fate of chemicals. Soil and Tillage Research, 53: 167-183. DOI: https://doi.org/10.1016/S0167-1987(99)00104-X.

Zhang X, Nearing M, Risse L. 1995. Estimation of Green-Ampt conductivity parameters: part II. perennial crops. Transactions of the ASAE, 38: 1079-1087.

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2018-255/gmd-2018-255-RC1-supplement.pdf

---

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-255, 2018.