2nd review

The reviewers have made substantial and important adjustments to the manuscript, addressing the reviewers comments thoroughly. I especially commend them on including a residue-infiltration relationship. Overall, I can now recommend the publication after the following minor to moderate revisions:

1. I agree that a simple approach to infiltration is suitable for this study. Yet the exponents used in Jägermayer et al seem to be hypothetical 'what – if' type assumption about the relationship between management and infiltration. Implementing this approach in the core of LPJmL (actual, not what-if relationship) will require some more justification/ anchoring in empirical data on the effect of residues on infiltration. Figure 4 shows that the model results are within measured range of one review study. But the review study by Ravainoson simply lumps all data, without considering important factors such as slope, scale (plot size), rainfall intensity. The paper should specify the relation of their general equation with these factors. For example LPJmL is a point model, yet effectively applies the equations to very large grid cells, how does this relate to the various plot sizes in the reviewed plot data? Should the equation therefore be at the upper or lower range? For studies on scale effects of surface runoff and tillage (though not explicitly residues) see for example Langhans et al 2019 or Leys et al 2010. This exercise needs to be repeated with all important factors, and then used to justify equation 10 and 11, or else equation 11 for the exponent p should be adapted accordingly. Given the sensitivity of the outcomes (witness relatively large changes from previous version) to this relationship I believe this to be an important effort.

2. I support the authors' decision to introduce a section on crop productivity. In itself this is an important outcome, fitting with the motivation of the study in the first place. I also believe that the aridity bar chart in Appendix Figure 2 helps to clarifying the pattern of yield changes (BUT: vertical bars better convey the causality between aridity and yield effects than horizontal bars, please consider changing).
It is surprising that the bulk of the section is devoted to the reasons why yields improve in dry regions, while the striking decreases in yields with NT in most of the humid tropics is only mentioned in one sentence, and no explanation is given. Given that yield increases in the humid tropics are of particular interest (e.g. for the SDGs) this result must be thoroughly discussed and explained.
Trying to understand myself what the reason for yield decrease in the tropics with NT is, I looked at Figure 1: if NT increases soil moisture nearly everywhere (Figure 5B), the only direct explanation for decreased Maize yields in the tropics (Figure Appendix 2B) according to the scheme is increased NO_3 leaching (less nutrient availability). Yet looking for example at India, NO_3 leaching actually decreases, yet yields decrease too. How is that possible? Are there important indirect effects of NT on yields that are in the base model, but not in the present extension? Please analyse this problem and give an explanation in the manuscript.

3. Now, the manuscript more clearly shows that C-input is higher in NT, which is a direct cause of increased long-term CO_2 emissions

4. Appendix 4: strange combination of sub-plots (evaporation, surface runoff, bare soil effect). Consider re-ordering in more straightforward combinations, or separate plots

5. Please add references to all processes and effects mentioned in section 2. It is an important convention in science to reference one-sentence assertions. It is even more important here, because it is claimed in the introduction that the most important processes are addressed. This needs to be supported in section 2, at least by giving meaningful references (that actually show a significant effect).

6. RD is defined in equation 34 but not further used in the figures. Either use RD in the results section and figures, or remove the equation. Also, while the comparison of MS/T_R is consistent for the figures, in Table 3 other comparisons are made. That is OK, but how and why need to be mentioned in the methods section. I suspect it is for comparability with available literature?

References

Langhans, C., J. Diels, W. Clymans, A. Van den Putte, and G. Govers (2019), Scale effects of runoff generation under reduced and conventional tillage, *CATENA*, *176*, 1-13, doi:https://doi.org/10.1016/j.catena.2018.12.031.

Leys, A., G. Govers, K. Gillijns, E. Berckmoes, and I. Takken (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*(3), 143-154, doi:https://doi.org/10.1016/j.jhydrol.2010.06.034.