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Interactive comment

Interactive comment on "Impacts of land-use history on the recovery of ecosystems after agricultural abandonment" *by* A. Krause et al.

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We thank the two reviewers for their comments which certainly helped to improve the structure and readability of our manuscript. In the following we answer their comments.

Reviewer #2: This manuscript evaluates ecosystem recovery after disturbance from agriculture and pasture management using a dynamic vegetation model and idealized case studies. This is an interesting application of the LPJ-GUESS model, however the results are not particularly novel. The study was well-executed and the paper is clearly written. I suggest publication with some minor revisions mainly to improve clarity.

General comment: I find the inconsistency in terminology relative to "vegetation composition" to be somewhat confusing. First the term vegetation composition is used (abstract); later it is defined as the LAI of the dominant PFT (page 3, L 15), and then

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finally in section 2.4 the reader learns that it is both the dominant PFT and the dominant PFT LAI. Therefore, it's not clear which result is presented in the figures since it seems to flip from LAI in figure 2 and dominant PFT in subsequent figures. It should be obvious to the reader how dominant PFT is defined and kept consistent throughout the manuscript.

Reply: We assumed the dominant PFT to be representative for vegetation composition recovery. As declared in section 2.4, we tested two preconditions to check the recovery of the dominant PFT, the recovery of its LAI compared to the LAI in the reference simulation (analogue to the other variables) and the PFT's dominance compared to other PFTs (which was achieved instantly for grasses and usually earlier than the LAI recovery for woody PFTs). In Figure 2 we only show changes in the LAI of the dominant PFT (precondition 1) because there is no simple way to show dominance recovery (precondition 2) in this figure (which is most often not relevant anyway). We made adjustments in the text to achieve a uniform presentation of this definition and to avoid confusion.

Page 5, L 29-30: reads awkwardly, to what does lower limit refer? Assuming that 'lower limit' implies the time to return to pre-disturbance conditions, wouldn't it be more appropriate to say that the ecosystem might never return. It is possible those sites have reached new equilibriums; does a trend still exist or has the model reached a new steady state?

Reply: Yes, lower limit refers to the recovery time which could theoretically lie between 801 years and infinity (new equilibrium) in these cases. For soil C (the only variable for which significant areas do not recover within 800 years), there is often a small (P100) to very small (C100) trend, suggesting that most of these "black" areas would eventually recover as well within some more centuries. However, the C accumulation rate usually becomes much lower some decades/few centuries after agricultural abandonment. Given the relatively low amount of C stored in these regions and the fact that most of the originally lost C is recovered within one or two centuries the importance of the very long recovery times in these areas for the global C cycle should not be overrated. We made the following changes to the text for clarity:

“For all variables, the recovery time was capped at 800 years after reconversion to natural vegetation when recovery was not achieved at the end of the simulation period, implying a lower limit for the recovery time. However, the actual recovery time in these cases could theoretically lie between 801 years and infinity.” (p 6, line 3-6 in the revised manuscript with marked changes)

“It should be noted that even though some regions do not recover within 800 years, a large fraction of the original C loss is already replenished after few centuries, thereby limiting implications for the C cycle.” (p 9, line 8-10)

Page 7, L1-2: This is interesting, looking at Figure 3 – there is a line of grids in the boreal forest that has exceptionally long time to recover LAI right next to grid cells that only take a fraction of that time to recover. The authors mention this, but provide no reason for the behavior. Looking at figure 5, both P100 and C100 have similar levels of N limitation in this region, so why does C100 take so much longer than P100 to recover?

Reply: In the C100 simulation, other PFTs than the dominating one (which in this case is IBS) are more competitive once tree establishment occurs. Thus, while total LAI is similar to the other simulations, the LAI of the dominant PFT is substantially reduced, thereby remaining below the threshold in some regions. We interpret this behavior as the PFT’s response to varying soil available N levels. Total N limitation seems indeed similar for P100 and C100 in this region, however, available N is still higher for P100 and even small differences can be important as IBS generally suffers more from N limitation than other PFTs. We added this explanation (p7, line 9-11):

“Furthermore, in parts of the boreal zone recovery takes several hundred years for C100 instead of a few decades for the other simulations because lower available N levels relatively reduce the growth of IBS (the dominant PFT in this region) compared to other woody PFTs."

Page 7, L14-16: This sentence should be rewritten for clarity.

Reply: We shortened the sentenced to (p7, line 26-28):

"Compared to dominant PFT, recovery occurs slightly later for vegetation C (Fig. 2, Table 1)."

Page 7, L17-18: The fact that grasslands return so quickly isn't surprising given the model setup. Although the authors don't specify how crops are modeled (other than some C and N modifications), I suspect that they are otherwise modeled exactly the same as grasses. If this is the case, grassland recovery would be almost immediate relative to dominant PFT and vegetation carbon.

Reply: Croplands are modeled as pastures (grasslands) apart from the differences described in section 2.1. Quick vegetation and LAI recovery is indeed not surprising, however, if productivity is reduced e.g. due to nitrogen limitation, LAI or vegetation C recovery will sometimes take several decades or more (Figure 3).

Page 7, L21: Boreal forests seem to have a higher standard deviation than tropical in figure 6.

Reply: That's true, but here we compared the standard deviations of dominant PFT (e.g. the black bar with the green background in the PFT figure) to the standard deviations of vegetation C (e.g. the black bar with the green background in the Veg C figure), not standard deviations of one variable for different biomes (which would be e.g. the black bar with the green background in the PFT figure to the black bar with the blue background in the PFT figure).

Page 8, L 6: The soil C loss of 0-11% seems very low, especially for agriculture lands. I would expect a minimum of 20% loss (even for a short 20 year period). This seems to be confirmed in the discussion section.

Reply: The relatively low values in our study could have a number of reasons. While observations are often restricted to the top soil C loss (where changes tend to be more pronounced), the model has no soil depth, meaning that our changes are implicitly averaged over the whole soil profile. Moreover, we excluded litter C from the analysis (mentioned in section 2.4). While soil C is usually larger than litter C, relative depletions are larger for the litter. In addition, our assumed fertilizer rate might be higher than historic fertilizer application, thereby overestimating C input to the soil. We added the following sentences to the discussion (p 10, line 22-25):

"LPJ-GUESS tends to simulate lower C loss in croplands than commonly reported in observations. We attribute this to a combination of the observation's focus on the top soil (while in LPJ-GUESS soil C is implicitly averaged over the whole soil column), the exclusion of litter in our analysis, and our relatively high fertilizer rates."

Page 8-9, L29-31-L1-3: This is unexpected and deserves some explanation.

Reply: We added a sentence about the cause (see also comments to reviewer 1) (p 9, line 18-20):

"This occurs because tillage-driven C losses in more labile soil pools, which dominate the system's response during the first decades, are eventually supplanted as the dominant process by accumulation in more stable pools."

I'm also curious, how is soil C recovering so quickly for P100 and C100 (for example in central Africa) when the other components (LAI, Veg C, NBP) are taking much longer (a hundred years or more).

Reply: It's important to keep in mind that while vegetation C and dominant PFT LAI are generally higher in the tropics than in other forest biomes, soil C is actually lower (see Figure 1), which means more vegetation C and less soil C has to be accumulated to reach background conditions (even though relative depletions are similar). Also, while tropical productivity is much higher than in temperate and boreal forests, biomass accumulation rates are similar, pointing towards higher turnover rates in tropical forests and possibly also increased mortality. The C input to the soil is thus enhanced and the high soil turnover rates allow soil C to response quickly to the new conditions.

Page 10-13, Section 4.1: This section tends to ramble and focus on elements that are not related to this study. At times, it is difficult to read and should be shortened and focused.

Reply: We added a table (Table 2) with the relevant information while removing large parts of the text (see also comments to reviewer 1).

Page 10, P1: The authors did a poor job of comparing the model output with observations. First, the authors never mention the soil depth of the model, so I don't think any of the comparisons with observations in this section are useful. Second, comparing against other versions of the model that didn't have the N cycle doesn't add any useful information regarding model performance capturing soil C loss after disturbance. I understand that soil C loss isn't the focus of the paper, but if the reader can't trust the soil c loss from management (which I suspect is underestimated), how can they trust the recovery estimates.

Reply: The primary aim of our study was not to reproduce site-specific soil C losses quantitatively but to estimate the legacy effects of different LU histories on the modelled carbon cycle and vegetation regrowth. We did not report soil depth simply because the model has none (we have now made this explicit in the text in section 4.1). Whilst this, and the fact that we did not use site-specific characteristics, indeed makes quantitative comparisons to observations difficult (which we have attempted to state more clearly in the revised manuscript), we still think the model should nevertheless capture the general tendencies. The fact that the model successfully reproduced a range of observations (see later in the discussion) and also captures most of the patterns observed after agricultural abandonments strengthens the reliability of our results. We mentioned the Pugh et al. (2015) study to give an idea how a different model setup can modify long-term C fluxes in agricultural soils. We extended one sentence in the discussion (p 11, line 1-4):

“By contrast to studies of LU effects compared to previously natural ecosystems, the regeneration of ecosystems after agricultural abandonment has been studied less, and a direct comparison to our simulations is challenging, either because limited information about former LU or reference conditions was provided in these studies, or because there are important differences from our setup in terms of management and LU duration or other site-specific characteristics.”

Page 10, L 28-32: Reword for clarity.

Reply: We removed the sentence and put the information into Table 2.

Page 11, L 29-30: This sentence is not clear.

Reply: We removed the sentence and put the information into Table 2.

Conclusion: I think one conclusion that wasn’t made (that could be based on the alternate recovery results) is that recovery for some variables doesn’t seem to be ever reached, only a new equilibrium (particularly for soil carbon).

Reply: As mentioned before, even though for soil C significant areas do not recover within the simulation period of 800 years, we think that finding should not be over-interpreted. It might take some more centuries or millennia until soil C in these areas would actually be “recovered” according to our definition, but the actual amount of C “missing” to reach reference conditions is usually very small, thereby limiting impacts on the global C cycle. We added the following point to conclusions #5 (p 16, line 21-23):

“5. In terms of soil C, our results suggest that some subtropical regions might not recover at all on timescales relevant for humans. However, given the low absolute amounts of C “missing” in these soils, implications on the global C cycle are expected to be small.”

Table 1: caption reads “Recovery times are depicted in Figure 4” – should be Figure 3.

Reply: Corrected.

Table 1: For the dominant PFT LAI recovery, I find it interesting that for a temperate forest, the P100 and C100 take less time to recover than the P20 and C20. Although all times are within the error bars, it still is not consistent with the other biomes. The authors don’t mention this behavior in Section 3.2, but it would be nice to have an explanation.

Reply: This pattern is generally found in regions where the TeBS PFT dominates, while regions where TeBE or IBS dominate recover typically faster for P20 and C20. We interpret this behavior as that reduced soil N favors TeBS in the competition with other tree PFTs, thereby reaching its background LAI levels earlier. We added this explanation to the results (p 7, line 22-24):

“Interestingly, temperate forests recover faster for P100 and C100 then for P20 and C20. This pattern is generally restricted to areas where the TeBS PFT dominates. We interpret this behaviour as that reduced soil N favors TeBS in the competition with other tree PFTs, thereby reaching its background LAI levels earlier.”

Figure 1 has a lot of acronyms that aren’t defined, please define each PFT.

Reply: We added a table (Table A1) for the PFTs.

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