

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

Anonymous Referee #2

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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 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

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the manuscript.

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?

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?

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

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.

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

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.

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

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hundred years or more).

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.

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.

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

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

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).

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

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.

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

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