Anonymous Referee #2

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Reviewer comment (RC)

Response to reviewer comments

Comments:

The study simulates the conversion of all forested areas north of 45N and compares the net biogeochemical plus biogeophysical climate effect – measured in terms of temperature change at the end of the 21st century – relative to a control simulation. The main results are in opposition to those reported in previous land cover change simulation studies (Bala et al. 2007; Claussen et al., 2001; Bathiany et al., 2010) to which the authors attribute to higher terrestrial C stock estimates in living biomass and the inclusion of substantial long-term C emissions from soil and litter pools on deforested areas.

The study overall is generally well-written and covers an important topic; however, it currently suffers from serious limitations concerning questionable assumptions in the experimental setup, transparency, and presentation of results which inhibit the potential value-added of the contribution.

Experimental: While although the simulated land cover change (LCC) is purported to be "academic" it is highly unrealistic as are the main methodological assumptions connected to it. The "deforestation" is more like land stripping in which 100% of aboveground C stocks are oxidized. While this may be theoretically possible as the authors claim, in reality it would not conform to any land management practice – even undesirable ones. In the case of forest conversion to cropland, for instance, some of the forest biomass would enter the soil pool from plowing/tillage; and in the case of removal by burning, some of the biomass would be converted to char with C immobilized.

Response#1: Rather than 'deforestation', we think the term 'forest clearing' is more appropriate in this study as the word 'forest' typically means woody vegetation. And in this study forest clearing meant complete removal of any form of natural vegetation. Thus the forest clearing method implemented represents the 'slash & burn' method. We do agree that the assumption of 100% C stocks being oxidized is a little extreme and not exactly what would happen in reality. But it is quite reasonable. See our Response#1 to Referee#1.

In reality some C connected to the former aboveground biomass stock would enter the soil compartment and would likely remain stable over the analytical time scale considered in this study, particularly if the conversion results in grasslands.

Response#2: The below ground carbon of the vegetation as well as the litter carbon does enter the soil compartment. We agree that this soil carbon would remain stable if the land conversion results in grasslands. This is the case with the studies by Bala et al. and Bathiany et al. However in the version of this study published in ESDD, the forested land was converted to either agricultural land or barren land. This is the reason why the soil carbon wasn't stable and was gradually decomposed and emitted. This was clearly a methodological discrepancy especially while comparing our study with that of Bala et al. and Bathiany et al. Thus we decided to redesign the experimental setup as described in Response#1 to Referee#1. With this new experimental setup, as shown in Fig. 1. of response to Referee#1, we do see that the soil carbon now becomes dependent on the land management scenario. For the more plausible scenarios the soil carbon does become more stable.

This is important because the entire value-added of the study hinges on the crucial finding – based on the LCC experiment – that biogeochemical impacts of LCC in northern latitudes dominate over biogeophysical impacts (with most of the impact stemming from long-term soil C emissions). I sincerely encourage the authors to consult the more specialized literature on C-dynamics of forest conversion to improve the precision of long-term C emission estimates, particularly those stemming from soil C pools
(See: http://onlinelibrary.wiley.com/doi/10.1111/j.1365-2486.2004.00738.x/abstract ;
http://onlinelibrary.wiley.com/doi/10.1046/j.1354-1013.2001.00459.x/abstract ;
http://www.sciencedirect.com/science/article/pii/S0016706111001303).
Response#3: Studies by Murty et al., (2002) and Karhu et al., (2011) show that the soil carbon does reduce after forest clearing followed by agriculture. But the magnitude of CO_2 emissions from soils could be overestimated if the change in bulk density of the soil isn't considered. Our study also shows a decrease of soil carbon after conversion of land cover from forest to agriculture. However as LPJmL, does not estimate the change in soil bulk density, it would overestimate the emissions from soils. This limitation of not computing the change in bulk density will be included in the 'discussion' section of the manuscript. With the new experimental setup, the results of this study also agrees with that of Grünzweig et al., (2004), as we show in Fig. 1 of response to Referee#1, that conversion of forests to agriculture leads to decomposition and emission of soil carbon stocks while conversion to natural grasslands would lead to sequestration of carbon in the long run.

Additionally, what is the rationale for only considering the worst case SRES scenario A2? The authors should better explain/justify why this scenario was chosen (rather than one with a more balanced energy supply like A1B, for example) and at least say something about how sensitive their long-term soil C emission estimates are to the assumed background climate and related DVM modeling assumptions driving respiration processes. The largest C-flux is the long term flux from litter & soil pools (233 GtC cumulative) and stems from respiration processes, so uncertainty and sensitivity aspects should absolutely be given more attention here.
Response#4: We can appreciate the effects of any mitigation option best if we consider the worst-case scenario as the reference scenario. SRES A1B is a scenario with a balanced energy supply and thus we do not consider it. Although the cumulative emissions of the SRES A1FI scenario is higher than the A2 scenario, we used the A2 scenario as it in the only scenario which has an increasing trend of CO_2 emissions even at the end of the 21^{st} century (Nakicenovic et al., 2000). In Fig. 1, we show the sensitivity of the soil carbon to changes in climate and CO_2. The redesigned experimental setup shows that the long term emissions from the litter and soil carbon pools are dependent on the land management scenarios (Fig. 1 of response to

Referee#1). We find that the emissions range from a cumulative of -28.6 GtC (carbon sequestration) for the most plausible, MAXL scenario to 218.5 GtC for the most idealistic, UNLIM scenario.

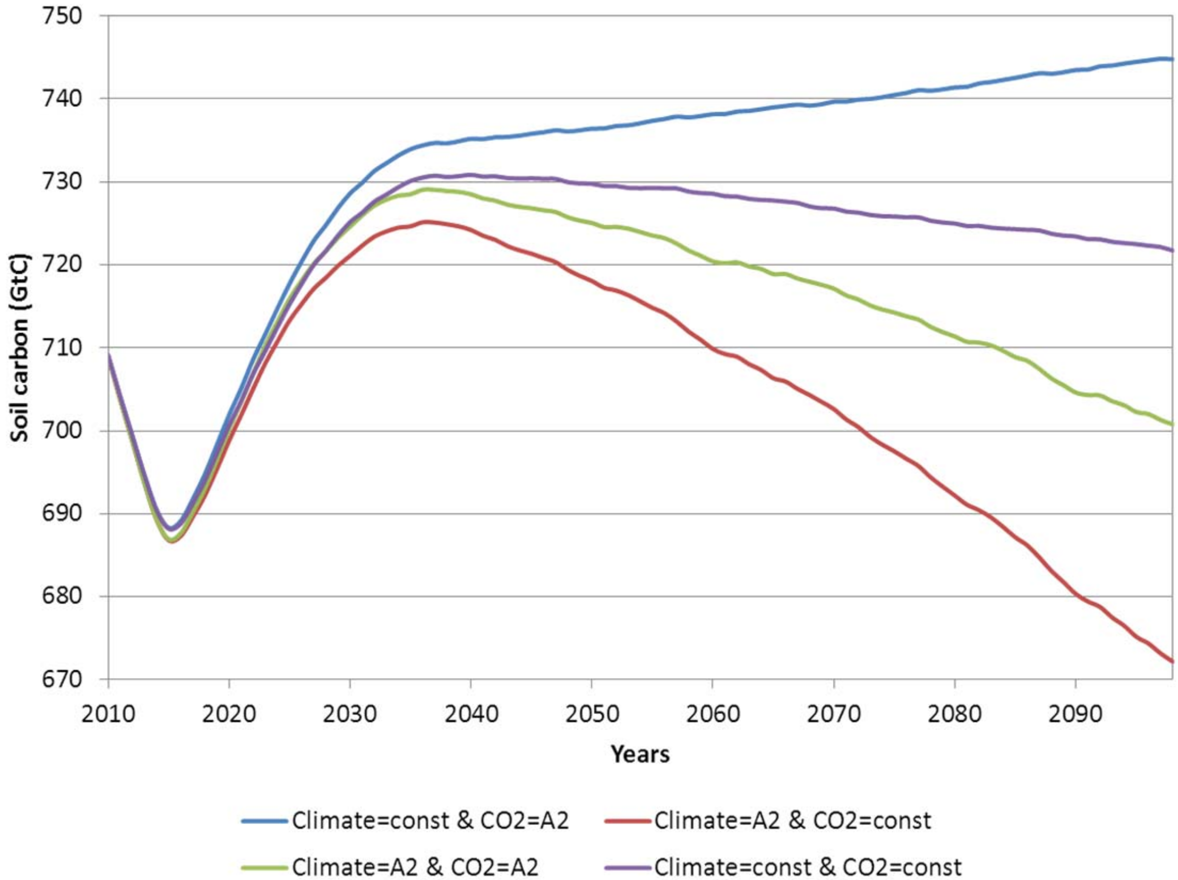

Fig. 1: The sensitivity of the soil carbon pools of the MAXL scenario to changes in climate and CO_2. When the climate changes according to the SRES A2 scenario, the temperature increases. This increases the soil respiration, thereby increasing emissions and decreasing the soil carbon. On the other hand CO_2 keeps increasing according to the SRES A2 scenario. This means that vegetation productivity increases due to CO_2 fertilization leading to an increased nutrient supply via litter to the soil, and thus increasing the soil carbon.

Transparency: The authors seek to compare their biogeochemical results to those reported in Bala et al. (2007); however, it is difficult to discern how they are able to do this using the information provided in the manuscript. There are layers of embedded assumptions surrounding the vegetation compartments and general modeling of the terrestrial C-cycle with LPJmL which are not disclosed, thus it is impossible to know which parameters are harmonized for a fair comparison across studies. Since the entire value added of this study is contingent on the updated C-stocks estimates in the northern hemisphere, it is absolutely crucial that the authors better isolate differences across studies and explain them.

Response#5: We agree that the experimental design of this study was not harmonized properly with the studies of Bala et al. and Bathiany et al. So we redesigned the experimental setup so as to make this study more similar to that of Bala et al. and Bathiany et al. See Response#2 to Referee#1.

This leads me to another major transparency issue which is in the actual derivation of the "updated" C-stock estimates. For example, text page 330, lines 9-11: "According to 2007 estimates, the carbon stock in the living biomass in the boreal forest and half of the temperate forests of the Northern Hemisphere amounts to 73 GtC (Pan et al., 2011) and this study computes immediate emissions, or the carbon emitted when the living biomass is burnt completely 182 GtC."

There seems to be a major mass balance issue unaccounted for or unexplained. In general the text here and in the Introduction describing where the numbers come from and why they are so much different is difficult to follow/comprehend. Since it deals with one of the most critical assumptions in the whole manuscript it should be clarified. As it reads it appears that 73 GtC in living biomass results in 182 GtC from the same biomass.

Response#6: We believe that there is a misunderstanding here. The value of 73 GtC stock is according to the paper (Pan et al 2011) and this value is based on observation. However the emission value of 182 GtC has been computed by us in this study by the LPJmL model. We mean that the immediate emission calculated by LPJmL appears to be an overestimation compared to observational values. We also provide explanations for this overestimation in the same page.

A third major transparency issue surrounds the biogeophysical modeling. Absolutely zero space is given to describing the modeling, the main assumptions and data sources, and the contribution from biophysical impacts in the presentation of results. A study that has – as the main research objective – the goal of comparing biogeophysical vs. biogeochemical impacts from an LCC simulation cannot be published without any attention here! Some information concerning the methodology surrounding the biogeophysical impact analysis is needed. Additionally, what is the contribution from biogeophysical impacts, their spatial distribution, and the main parametric and modeling uncertainties? And what are the relative contributions between evapotranspiration, roughness, albedo changes? Some results here are needed, particularly with regards to the spatial distribution of the various perturbations and their impacts. Which regions do albedo changes (or roughness changes or ET changes) dominate and do the resulting impacts follow the same spatial distribution?

Response#7: Biogeophysical effects cannot be studied with LPJmL. Thus we had attempted to compare the biogeochemical forcing computed in this study with the biogeophysical forcing computed in the studies by Bala et al. and Bathiany et al. However since an assessment of the biogeophysical effects is important, we conduct an additional experiment with MPI-ESM to study the same. See Response#1 & Response#8 to Referee#1.

Presentation: Another major criticism is the potential for this study to be misinterpreted for one that investigates the mitigation potential of bioenergy, evident in the study's title ("Can bioenergy compensate deforestation emissions?") and based on presentation of results. Due to the experimental design the net combined geochemical-geophysical climate impact from the land cover change simulation cannot be attributed to bioenergy in all but the “UNLIM” land management scenario – an unrealistic scenario. To do so for the other land management scenarios would require restricting the analysis to only those grid cells in which 100% of the deforested area (green pixels, Fig. 6) is suitable for bioenergy crop production, i.e., a 1:1 area conversion. Figure 10 is therefore misleading as the C-sink capacity on land areas suitable for bioenergy is left to accommodate the full “carbon debt” due to deforestation on all land areas north of 45N (“Total emissions”, black).

Response#8: We implement forest clearing on all available land north of 45°N. Thus the immediate emissions for all the land management scenarios are the same. However, in the redesigned experimental setup with LPJmL, natural grassland is allowed to regrow if the land is not cultivated. As the extent of land under bioenergy cropping differs among the land management scenarios, the long term emissions would also be different for the different land management scenarios (Fig. 1 of response to Referee#1).

If the authors could attribute the combined climate impact associated with “deforestation” and biomass production on the 1:1 conversion pixels in the MAXL land management scenario – which is the more “plausible” bioenergy scenario with areas more realistically suitable for biomass crops (for bioenergy) – the value added of the study due to increasing the policy-relevancy of the results would be significantly improved.

Response#9: We do agree that forest clearance on all available land north of 45°N, even if we do not plant crops there is unrealistic. However, we cannot change this if we are to harmonize our study with that of Bala et al. and Bathiany et al.

A final comment related to methodology surrounds the authors’ choice to introduce a counterfactual C flux scenario into their analysis whereby they have included the reduced terrestrial C-sink capacity. Page 327 lines 2-3: “...followed by a loss of C sink that forests would accumulate if not removed of 34.8 GtC”. This lost C-sink flux represents a substantial “emission” which is based on counterfactual considerations (one of the many theoretically possible) and its inclusion in the results questions the fundamental physical basis of the study, as the direct effects of human induced perturbations should be assessed first and independently of alternative speculative aspects. If you introduce one counterfactual then you could just as easily introduce several additional valid counterfactual scenarios thereby greatly expanding the level of uncertainty and undermining the integrity of the analysis… for example, one could just as well have included the foregone C sequestration that would have occurred in the forests if the land would not have been cleared (and you can see how many hypothetical terms are present in this sentence). These counterfactuals are grounded in non-casual relationships and partial assumptions (i.e., not considering specific saturation thresholds, fires, pests, human disturbances, etc.) and should not be included.

Response#10: We agree to remove these lines from the manuscript.

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