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Interactive comment on "Can bioenergy cropping compensate high carbon emissions from large-scale deforestation of mid to high latitudes?" *by* P. Dass et al.

Anonymous Referee #2

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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. 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. 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).

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.

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.

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.

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?

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).

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.

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.

Interactive comment on Earth Syst. Dynam. Discuss., 4, 317, 2013.