Anonymous Referee #1

Reviewer comment (RC)

Response to reviewer comments

This is my review of the manuscript: Can bioenergy cropping compensate high carbon emissions from large-scale deforestation of mid to high latitudes? Submitted by Dass et al. for publication at Earth System Dynamics.

The manuscript deals with the pertinent issue of land cover change and land cover management in the context of a changing climate. The authors set out to determine the net impact on emissions expected under various scenarios involving the very large spatial scale conversion of mid- to high-latitude areas from their natural vegetation to bioenergy crops. The text is mostly clear but can sometimes be a bit convoluted. I made some comments on the attached pdf regarding some of these passages. Figures and captions are clear and elucidative.

From the point of view of framing the problem the manuscript is somewhat lacking in focus. They make it very clear from the start that "these studies are purely academic to understand the role of vegetation in energy balance and the earth system" but then go on to present results and discussion pretty much exclusively in terms of management options and mitigation efficiency. This is not such a big deal, but some clarity would be welcome.

My main concern though, and the reason why I believe the manuscript should be accepted only after major revisions, is that their main conclusion is somewhat weakened by factors that, in my view, should merit more attention.

Larger emissions: One of (the?) most important conclusion is that their deforestation simulations emit more carbon than those of Bathiany and Bala. Well, the way they change vegetation is very different from the way this was done in those earlier experiments. The type of land cover change simulated here is more akin to denudation than to deforestation.

Response#1: We agree that the setup of the deforestation simulation in this study was different from that of Bala et al. and Bathiany et al. In response to this comment and also to comments of the reviewer #2, we redesigned the experimental setup to make this study more comparable to the studies by Bala et al. and Bathiany et al. In the new experimental setup, after performing extensive deforestation of the high latitudes, we keep the land fallow for one year and then allow natural grassland to grow on areas not planted with bioenergy crops. We prevent the growth of any woody vegetation. Moreover, we also conduct an additional experiment with MPI-ESM-LR, the CMIP5 version for pre-industrial simulation (Giorgetta et al., submitted). We deforest all land north of 45°N and allow this land to be replaced by grassland, keeping the atmospheric CO_2 concentration fixed as the pre-industrial value. We conduct this simulation for 30 years. In these new experiments we assume 100% of the carbon of the above ground biomass to be emitted to the atmosphere as immediate emissions. We think that this is a reasonable assumption as it represents a 'slash & burn' type of deforestation. This assumption has also been used in earlier studies, e.g. by Grünzweig et al. (2004). Moreover, it has been observed through field studies that even in natural forest fires, as much as 90% of the carbon at the ground layer of a severely burnt forest is consumed (Michalek et al., 2000).

They remove more biomass than the other simulations and hence get larger emissions. How much of the difference they report is caused simply by this methodological disparity?

Response#2: We agree that there was a methodological disparity. Thus in order to remove this disparity, we have redesigned the experiments both with LPJmL and MPI-ESM, as described above. In the experimental simulation with LPJmL, we find the immediate emission of 182.3±0.7 GtC from the biomass burning. The long term changes in the soil and litter carbon pools range from a sequestration 28.6 GtC for the most plausible, MAXL scenario to and emission of 218.5 GtC for the most idealistic scenario, UNLIM, by the end of the 21st century (values are a mean of 19 GCM ensemble), as illustrated in Fig. 1. The new experiment with MPI-ESM simulating boreal deforestation showed that the immediate emissions are 61.3 GtC. Moreover, the equilibrium boreal carbon storage of 2.9 kg/m^2, averaged over all land north of 45°N computed by the version of MPI-ESM used in this study, is an underestimation compared to the observed value of 4 – 6 kg/m^2 (Prentice et al., 2001). Thus we show that there is still considerable difference in emissions though the methodical disparity has been minimized.

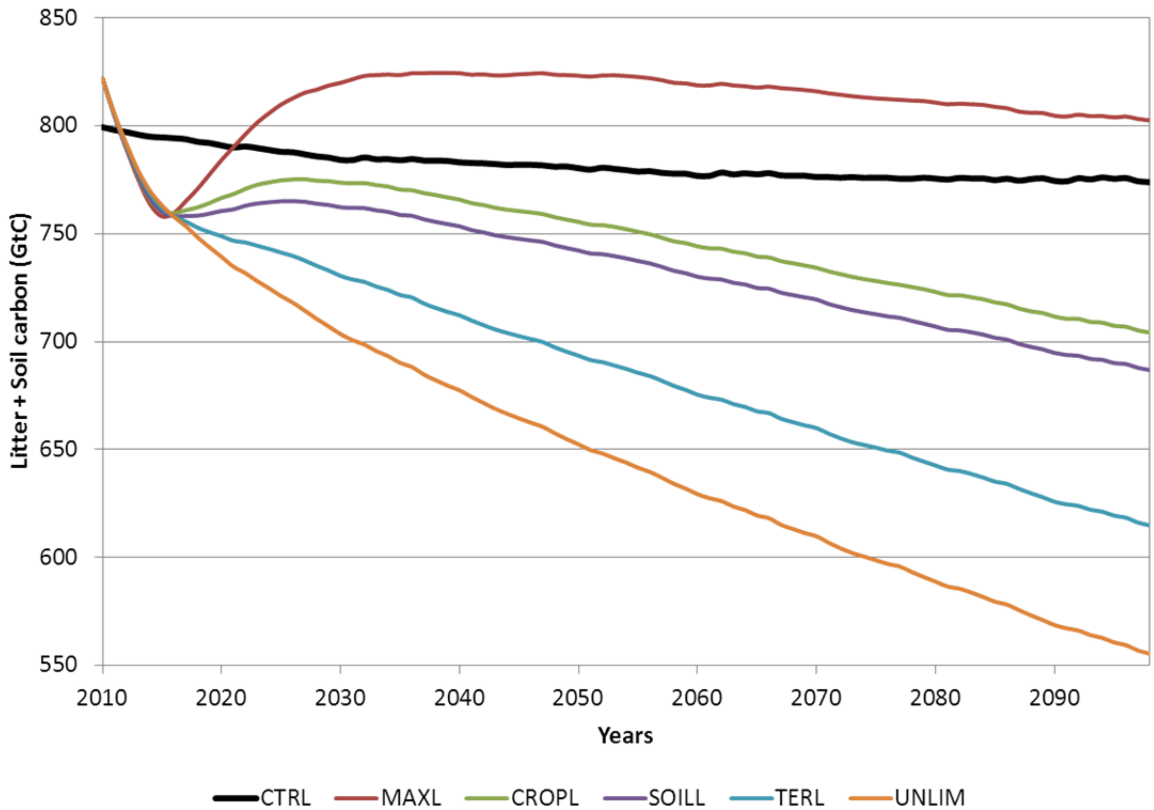

Fig. 1. The change in the soil and litter carbon pools for the different land management scenarios, after extensive boreal deforestation. After an initial decrease, the carbon pools of soil and litter start recovering as natural grassland is allowed to regrow on areas not used for bioenergy cropping. The difference in carbon of the different land management scenarios is because of the different extents of land under bioenergy cultivation. MAXL, the most plausible scenario has the least land under cultivation and thus has the most land under natural grassland. As grassland soils have more carbon than that of woody forests, the sum of litter and soil carbon of MAXL exceeds that of CTRL.

There is also the problem that comparing their offline simulation to results from fully coupled ESM, which is not very reasonable.

Response#3: The main deficiency of an offline model is that the feedback of the vegetation on the climate and vice versa is missing. While this would have an effect on future biophysical bioenergy potentials and the long term emissions, this would not have an effect on the immediate emissions, which depend on the current carbon stock. Another potential deficiency is that the climate simulated by the ESMs (used to drive carbon cycle models in ESMs) is different from the observed climatology used to drive the carbon cycle in LPJmL. However, climate biases in GCMs are relatively small. Besides, the carbon cycle models in ESMs are used to project the CO_2 concentrations in the future, therefore it is important to evaluate their performance. Thus it is reasonable to compare emissions of LPJmL with that of fully coupled GCMs.

The manuscript does not make it very clear why their higher value is better.

Response#4: In page 319, lines 23 – 27 and page 320, lines 3 – 6, we cite the observed estimates of the carbon stocks of the boreal and temperate regions. We do agree that while the immediate emission computed by LPJmL is an overestimation, the long term emissions are reasonable. This also shows that the total emissions computed in studies by Bala et al. Bathiany et al. and the additional simulation which we did, with MPI-ESM, are an underestimation.

In fact on page 330, lines 11-14 they note that their immediate emissions are more than twice the value of what the latest observational numbers would suggest. I guess it was also not very clear to me what was the reason for discussing the differences between what is considered mid and what is considered high latitude.

Response#5: We do agree that the value of immediate emissions is higher than the observational values. However, in lines 13 – 21 of the same page, we also provide a possible explanation for this overestimation. We also agree that instead of calling the area of interest ‘mid to high latitudes’ we should call it ‘high latitudes’.

So, in a sense the “larger emissions due to deforestation than previous efforts” result is a little weakened due in part to an “apples to oranges” type of comparison.

Response#6: We agree that there was a difference in experimental setup between this study and that of Bala et al. and Bathiany et al. Thus we redesigned our experimental simulations, see our response above. However, we still find that the emissions computed by this study are larger compared to previous studies. Moreover the statement that the emissions computed by Bala et al. and Bathiany et al. are an underestimation compared to observed values still holds.

Warming: The authors use Matthew et al.’s metric to estimate what their extra CO2

emissiosn would mean in terms of temperature and than add this warming to the temperature estimates of Bala and Bathiany. Ok, but the caveats of comparing offline simulations to fully coupled ones should be better presented. In fact, so should the utilization of the Matthew’s metric, which is not based on a climate system with such rapid and large land cover change.

Response#7: The caveat of using an offline model for computation of future biophysical bioenergy potentials have been stated in Page 332, lines 15 – 20. However while comparing the results of an offline model with that of a fully coupled one, as stated above, there should not be any significant difference in the immediate emissions but there could be differences in the long term emissions as the decomposition of soil and litter carbon is dependent on the climate which is again affected by the feedback from the land use change. The metric to calculate the temperature response to extra CO_2 emissions based on studies by Matthews et al. and Gillett et al. takes into account the response of the ocean carbon system on multi-decadal timescale. The same is valid for studies by Bala et al. and Bathiany et al. Thus it is reasonable to add these two temperature values as an approximation for the net temperature change.

Also, while the manuscript mentions many times the importance of the albedo change the authors provide no estimate (other than saying it should be similar to what was seen by Bala and Bathiany) of what this would be in their case. Once again, offline to fully coupled comparisons are tricky. For example, their extra atmospheric CO2 could impact snow cover in significant ways, this would certainly change albedo. At least the authors could present the change in albedo caused by the different land management scenarios, and it would not be very difficult to go from that to an estimate of change in radiative forcing (following the steps takeb by Betts 2000) and eventually temperature or at least some CO2 emission equivalent.

Response#8: We did not provide any values for albedo change as LPJmL is unable to compute so. However, in the additional simulation using MPI-ESM, we find that boreal deforestation leads to an increase of albedo, leading to an increase of surface upwelling short wave radiation by 11.9 W/m^2, averaged over 30 years after the deforestation event and over all land area north of 45°N, compared to equilibrium conditions. In this experiment, the atmospheric CO_2 concentration is prescribed to the pre-industrial value and therefore only biogeophysical effect of boreal deforestation is considered. A decrease in the global near surface air temperature averaged over the 30 years of simulation is 0.3°C. Using the metric of transient climate sensitivity to cumulative emissions (Gillett et al., 2013; Matthews et al., 2009), we find that the immediate emissions of ~182GtC would lead to an increase of global temperature by 0.15 to 0.38°C. For the sake of harmonization of experimental setup, we implement deforestation on all available land north of 45°, irrespective of the land management scenarios. The deforestation is followed by either grassland or bioenergy plantations, which are both herbaceous and have similar albedo. Thus changes in land surface albedo would remain the same irrespective of the land management scenario.

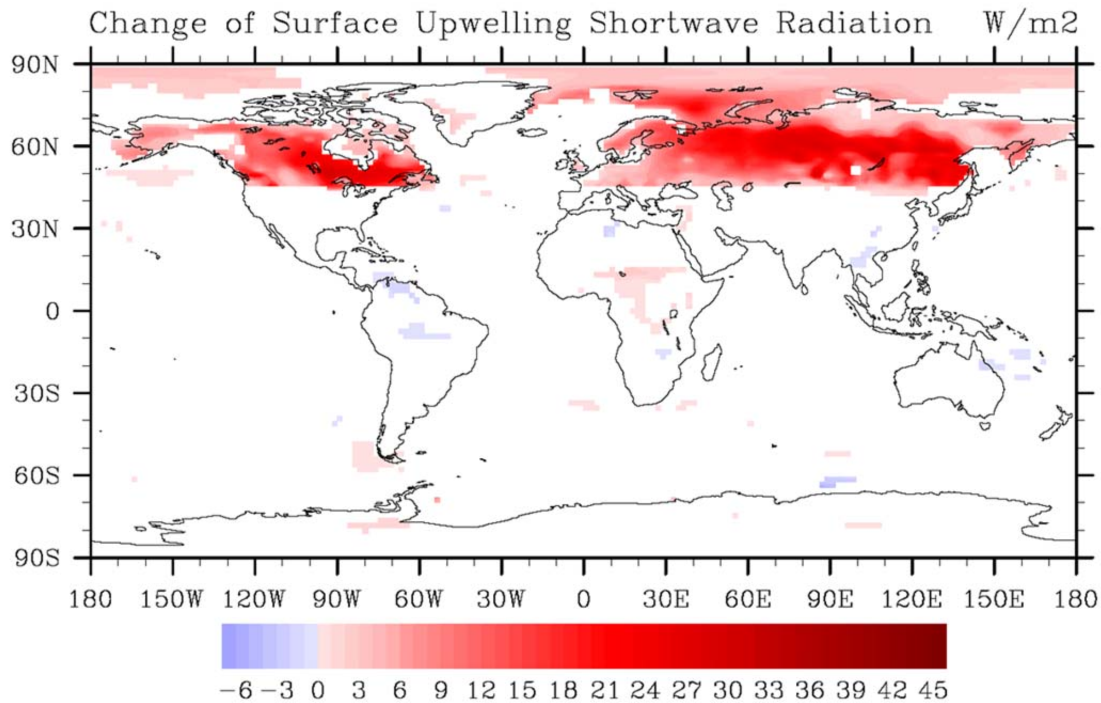

Fig. 2, The change of surface upwelling shortwave radiation (W/m²) caused by an increase of albedo, due to deforestation. Shown is a difference between deforestation experiment of regions above 45°N and pre-industrial control simulation from the Coupled Model Intercomparison, Phase 5 of MPI-ESM (Giorgetta et al., submitted). Shown are only statistically significant changes ($p<0.05$).

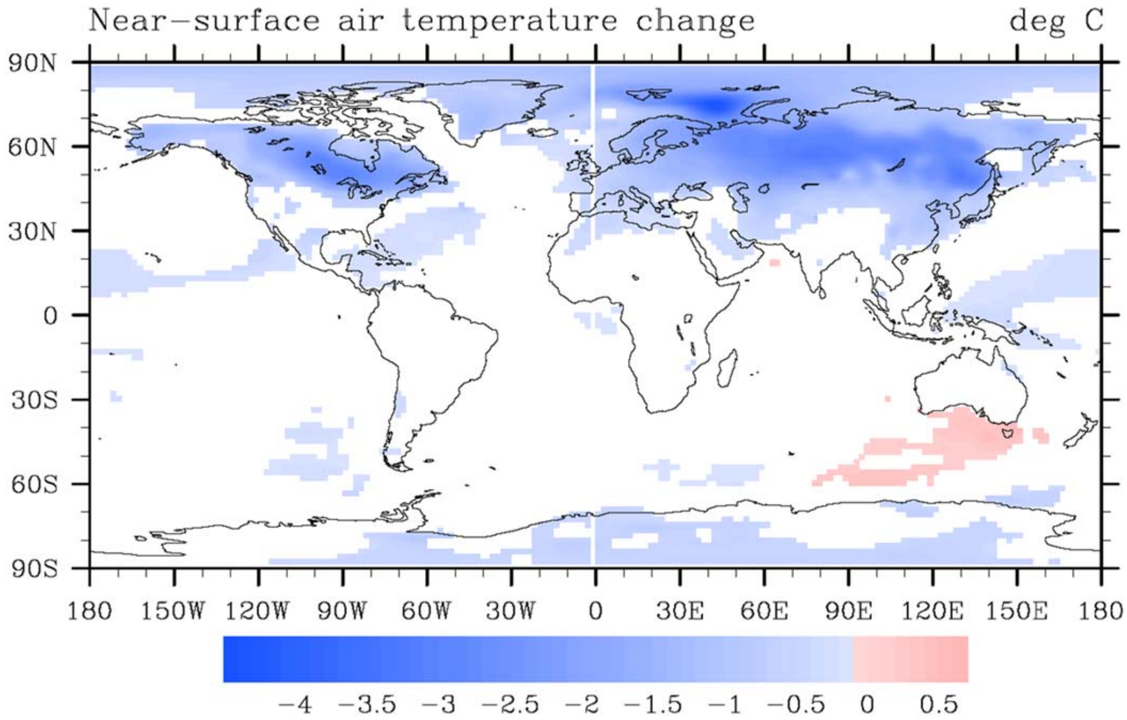

Fig. 3. The same as Fig. 2 but for near surface air temperature (°C)

Please also note the supplement to this comment:

http://www.earth-syst-dynam-discuss.net/4/C154/2013/esdd-4-C154-2013-supplement.pdf

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

<u>Manuscript comments of published 'discussions' paper:</u>

Page 319, Line 21: "The dominance of the biogeophysical effect of global boreal deforestation (Bala et al., 2007; Bathiany et al., 2010) could be due to an underestimation of the biogeochemical response."

RC: To simplify the reader's life you should make your point at the end of this first sentence. Something like: "Previous works assume X amount of carbon and we believe this value is closer to Y (larger)". Then go on to explain. Also, from the start make it clear that the difference arises from including mid-latitude forests, not from use of different land carbon densities.

Response#9: We agree with comment. Changes are to be made in the revised manuscript.

Page 320, Line 16: “Thus it is evident that the total carbon emissions in these studies are at the lower end of observational estimates.”

RC: I'm not sure this is so evident. This is what the models got and it has a lot to do with what happened to modelled soil carbon. If I follow you correctly, part of your argument is based in the absence of modelled soil emissions after deforestation. I'm not so sure observations paint an unequivocal large loss of soil carbon after mid- to high-latitude deforestation… If they do, you should support it here.

Response#10: When land is converted from any form of natural state to crop land, then the supply of nutrients through litter stops immediately. Thus the carbon which is already stored in the litter and soil is decomposed and emitted due to soil respiration. Numerous studies unequivocally show that conversion of land from forest to crop-land leads to degradation of soil carbon stocks (Davidson and Ackerman, 1993; Ellert and Gregorich, 1996; Guo and Gifford, 2002; Post and Kwon, 2000).

Page 320, Line 18: “While we are not proposing large scale deforestation as a mitigation option, we carry out a purely academic study to make a better estimation of the carbon cycle changes under such large-scale deforestation.”

RC: How large a scale? What are you proposing, total deforestation above 45N? Even if this is dealt with by methods, you should give the reader a better idea of what you are talking about.

Response#11: We agree and will be more explicit in the revised manuscript.

Page 322, Line 15: “Energy trees have been excluded here as they would not yield the albedo driven cooling effect, while energy grasses are harvested annually (Beringer et al., 2011)”

RC: Could this be explained a bit better?

Response#12: Deforestation of the high latitudes results in an increase of albedo which reduces the radiative forcing. Thus if bioenergy trees were to be planted in the deforested areas, then this reduction in radiative forcing would be lost. Thus for bioenergy plantations, we use all types of herbaceous bioenergy crops and bioenergy grass. We will add this in the revised manuscript.

Page 324, Line 5: “with crops such that those crops return maximum primary bioenergy per pixel per year”

RC: What are these? Are they standard LPJ pfts? You say above that you don't use "energy trees", which trees to you use? Bioenergy per pixel seems like a horrible unit :)

Response#13: These crops are standard LPJmL crop functional types (CFTs) and bioenergy functional types (BFTs), except bioenergy trees (Beringer et al., 2011; Bondeau et al., 2007). We agree to replace the unit “maximum primary bioenergy per pixel per year” with “maximum primary bioenergy yield (MJ/ha)”

Page 324, Line 7: “2/3 of the sap wood”

RC: forest litter? Explain the 2/3.

Response#14: It is the assumption that 1/3 of the sap wood is in the roots and thus belongs to below ground carbon. The forest litter enters the soil carbon pool and is then decomposed.

Page 324, Line 13: “The land use of the area deforested in this experiment is dynamic and could potentially change from year to year depending on which crop would provide maximum energy yield for that particular year”

RC: maybe explain this a bit better, loose the "potentially". What happens to the carbon when a crop is substituted?

Response#15: We agree and will remove the word ‘potentially’ from the revised manuscript. After annual harvest, all parts of the plant other than the storage organs are left on the field and as a result enter the litter and then the soil carbon pool.

Page 327, Line 3: RC: This would be a good place to reminde the reader that these emissions are not influencing the climate forcing your land cover simulation.

Response#16: We agree and will add a sentence in the revised manuscript saying so.

Page 328, Line 11: RC: Ok, but here you should say that savings coming from land use change must also take into account other climate forcings associated wtih these changes. Actually, you could use LPJ to estimate the change in shortwave absorption between control and managed simulations to come up with a number. At least provide what was the change in albedo and say something like : this biogeochemical cooling would be augmented/reduced by the increase/decrease in albedo caused by the land management.

Response#17: The version of LPJmL used in this study does not calculate albedo or shortwave radiation. However, in the additional simulation with MPI-ESM, we have calculated the change in upwelling short wave radiation, see our response above. These changes and figures will be added in the revised manuscript.

Page 329, Line 14: "They did not estimate net long term emissions"

RC: This is a bit confusing, the models have long term carbon dynamics, but these do not show large net emissions.

Response#18: We agree and will rephrase this in the revised manuscript.

Page 329, Line 28: "it meant complete removal of any kind of natural vegetation, leaving behind bare ground"

RC: so at least from a linguistic sense, you did not perform deforestation…

Response#19: The deforestation simulated in this study is more similar to the 'slash & burn' type of forest clearing. Thus it removes all natural vegetation. However, in order to make the experimental design similar to that of Bala et al. and Bathiany et al., we plant herbaceous crops or let natural grassland grow back in areas not planted with crops.

Page 331, Line 5: "The dominance of the biogeochemical effects (carbon cycle) over the biogeophysical (albedo) is however robust in our analysis"

RC: I'm not sure how you can state this without an estimate of your biogeophysical forcing…

Response#20: After carrying out the additional boreal deforestation experiment with MPI-ESM we found that the global temperature reduced by 0.3°C due to biogeophysical effects only. On the other hand, the immediate emissions of ~182 GtC lead to an increase of global temperatures by 0.15 to 0.38°C. According to the new experimental design, the long term emissions depend on the land management scenarios, varying from a sequestration of 28.6 GtC for the MAXL scenario to emission of 218.5 GtC for the UNLIM scenario. Thus we can say that for the most plausible scenario, the biogeophysical forcing is more dominant or gets just about neutralized. But as the scenarios get more idealistic, the long term emissions increase and biogeochemical forcing becomes more dominant. We will discuss this in the revised manuscript.

Page 332, Line 18: "The mitigating effect of large-scale bioenergy production on climate is not considered here. To include these feedbacks, a full coupling of the carbon cycle and the climate system would be necessary"

RC: I believe some statement like this should be present in the introduction.

Response#21: This statement discusses one of the deficiencies of this study and is not the main focus of this study. Therefore we prefer to keep it in the 'discussion' section.

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