

## ***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 #1**

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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

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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. 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?

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

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

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.

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Warming: The authors use Matthew et al.'s metric to estimate what their extra CO<sub>2</sub> emissions would mean in terms of temperature and then 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.

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 CO<sub>2</sub> 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 taken by Betts 2000) and eventually temperature or at least some CO<sub>2</sub> emission equivalent.

Please also note the supplement to this comment:

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

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