

**Author Response to Referee Comments on Carbon density and anthropogenic land use influences on net land-use change emissions**

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We thank the referees for their helpful comments. The original referee comments are in blue below, with our response in black.

*Anonymous Referee #1*

*Smith and Rothwell have used a conceptually simple model of the terrestrial carbon cycle to estimate the size of carbon emissions due to land-use change, and the sensitivity of this estimate to its underlying assumptions. The method allows to clearly and consistently differentiate the potential magnitude of key uncertainties in the assessment of land-use change emissions, both under the deforestation dominated historical period and a future afforestation scenario. Given the difficulty of comparing published studies of land-use change emissions due to the different assumptions and definitions used in those studies, a carefully constrained analysis such as presented here presents a useful step forward in understanding.*

*The article is well written and generally easy to follow although there are some areas that require or would benefit from further discussion/explanation, as detailed below.*

*We appreciate the very helpful comments.*

*The model conceptualization and reasoning appears robust, and I have few quibbles with the science so long as the points below can be adequately addressed. The authors should, however, double check all values in the tables, main text, and SI, as there are several places where there appear to be inconsistencies. For instance, the total 1700- 2000 LUC emission for the VEGAS and CESM sensitivity studies are +16.4 and +20.4 Gt C respectively in the SI, but -20, and +32 Gt C in Table 5. Line 24 on pg. 4170 then goes on to describe decreases of 6 and 18% for these sensitivities relative to the central scenario, which is consistent with neither set of results.*

*Thank you for the careful reading. We will check the values in the tables and figures.*

*Overall I would recommend the paper for publication subject to minor revisions.*

*Main comments*

*The results are clearly dependent on the values selected in Table 1 and 2 of the supplementary information (SI). There need to be some justification of why the numbers used were chosen.*

*Additional discussion of the values chosen will be added to the SI (expanding on the discussion in the main text).*

*The analysis is restricted to emissions from land-use change, and the effects of changing climate and atmospheric CO<sub>2</sub> concentration are not considered. This usefully simplifies*

the interpretation of the results. Possibly it does not add a substantial bias to the historical emissions calculation (although it most likely does to the future scenario calculations), however there should be more discussion of how changing climate and [CO<sub>2</sub>] are likely to influence the results. If the study was just restricted to a sensitivity analysis of land-use change assumptions in the absence of a changing environment then this discussion would not be required, and could be left to a separate paper (as the authors indicate that they plan to do). However, as the results are also presented as an estimate for the historical period, and directly compared (in section 4.2) to studies that do take a changing environment into account, such discussion is necessary, perhaps also with a couple of additional sensitivity studies to back it up.

We will add a sensitivity test for several values of CO<sub>2</sub> beta feedback and Q10 temperature sensitivity to the SI to give some context (and add a brief discussion of these to the main text). These were actually already done (and were inadvertently left in the supplement table). The assumptions for these values, at least in a parameterized model such as this, do have a significant impact on historical results. We agree with the referee that this will provide some useful context for the results without feedbacks that we focus on in the paper.

It took me quite a long time to work out why cropland should become a carbon sink in the 21st century, and for pastureland it still isn't clear to me. The particularly confusing line is on pg. 4168, line 24, "Cropland also becomes a net sink, and both cropland and pastureland take up carbon as the total areas of each decrease." This implies that the sink behaviour of the crop and pasture lines on Fig. 1 in the 21st century is due to the total areas of each decreasing. Following the authors' method, a reduction in cropland area would cause cropland to be a carbon source (as LU emissions are allocated to the ecosystem type that loses area). I think the authors perhaps meant something like "both cropland and pastureland take up carbon due to productivity increases and XXXX respectively, despite the total areas of each decreasing"? In any case, it would be helpful if the authors could clarify this, and make the reasoning for this behaviour more explicit at its first mention.

We will clarify the text in these instances. There are two factors at work here. First, we will clarify in this section of the text that no carbon (e.g., soil carbon) is lost on reversion of cropland or pastureland to natural ecosystems. While there may be instances where there might be some carbon loss, in most cases, there is, in generally, little soil disturbance when cropland or pasture is allowed to revert to natural vegetation. (Although it might be possible that soil carbon could decrease during the change in ecosystem structure from cropland to natural vegetation). Second, for cropland, productivity increases are the driver of increased uptake into the future. There is no productivity increases assumed for pasture, however. The reasons for the pasture uptake will also be discussed (largely making up for previous losses during conversion, which as we note, in part, appears to be due to an artifact in the land-use data).

On pg. 4170, line 7, the authors state that the results from their work are similar to those of Hayes et al. However, I would say that one really needs the eye-of-the-believer to agree with that at first glance. Many of the results displayed in the SI actually appear very different. This statement should be removed or further justified.

We will expand the discussion on this point. We are, of course, comparing slightly different results, since the Hayes et al. data include climate and CO2 feedbacks while our results explicitly exclude these (except, at least in part, for cropland, where we use actual crop productivity.)

The land-use types in Table 6 appear to encompass the major global land uses. Why then are the total land areas so different between G-Carbon (9831 MHa) and GCAM (11390 MHa)?

We thank the reviewer for the careful examination of the table. The table has been corrected and an “other” land category (equal to tundra, rockicedesert, and wetlands) added so that totals add to a consistent total across time.

Minor/typographical comments

It would be helpful to number and properly caption the tables and figures in the SI, e.g. Table S1, S2, etc. It would also help the reader if the main text referred to specific section/figures/tables in the SI.

This is a good idea, we will add these.

Pg. 4162, line 12. Please define harvest index to assist the reader.

Good idea, done.

Pg. 4166, l. 11. I believe the reference to Figure 3 should actually be to Figure 2. Also Table 3 should be referenced here.

Corrected, thank you.

Pg. 4168, l. 21. ‘Significant’ is mistyped.

That’s bizarre, as far as I can tell it was correct in my submitted version! We will try to look for that in the next journal version.

Pg. 4168, footnote. “. . .to a regime in which global policies. . .” C1313

Corrected, thank you.

Pg. 4169, l. 8. I believe the reference to Table 5 should in fact be to Table 4? Pg. 4169, l. 19. “. . .for the lower estimate of DeFries. . .” Pg. 4170, l. 4. I believe the reference to Table 5 should in fact be to Table 4? Pg. 4170, l. 4. “. . .values here are within. . .”

Corrected, thank you.

Pg. 4170, l 23. Please give references for these models.

We have corrected this major oversight. Thank you.

Pg. 4171, l. 17. Please add “in our simulations” after “21st century”.

Done.

Pg. 4171, l. 20. “Densities” is mistyped. Also please rephrase this whole sentence without the use of the brackets. This type of sentence is difficult to read, and, as space is not an issue, would be much better as two separate sentences.

That you, our mistake, corrected. Sentence also rewritten as suggested.

Pg. 4172, l. 17. The second clause of the first sentence does not make sense. The text in section 5.3 of the main text and section 3.3 of the SI are very similar and repeat each other. I suggest taking the salient bits out of the SI, adding them to the main text, and then deleting this SI section, in order to tell the story more smoothly.  
Sentence corrected and duplicate material removed as suggested.

Pg. 4173, l. 18. “LUC results are most sensitive”. To what does “most” refer? Bigger sensitivities than these have already been described in the previous sections.  
Phrase reworded to clarify.

Pg. 4171, l. 22. Replace “areas as” with “the areas”. Pg. 4175, l. 4. Missing full stop after “scenario” Pg. 4175, l. 10. “GCM” or “GCAM”? Pg. 4175, l. 14. “. . .we find net land-use. . .”

Pg. 4176, l. 7. “emissions” should be “emission”.

Corrections made. GCM now spelled out as global climate model to clarify (and reference moved to end of sentence).

Pg. 4176, l. 8. This statement is true for a non-spatially explicit model such as G-CARBON, but a spatially explicit ecosystem model should (in theory) capture the productivity variation. Therefore please caveat this statement.

The paragraph noted by the referee makes two statements, one about cropland and one about pasture. Actually we don't believe this is necessarily the entirely case, but is likely less of an issue for pasture at least. The issue is how anthropogenic land-use categories are represented in a spatial model. The issues identified here could occur in a spatially detailed model just as it does in G-Carbon (whose inputs were derived from spatially detailed data), depending on how anthropogenic land-use categories were treated in the ecosystem model. We have expanded this discussion with additional text as follows:

The lowest value for LUC emissions was found in a scenario where croplands are represented as grasslands, instead of reported crop productivity over time. In most cases, conversion to cropland, particularly prior to the mid-20<sup>th</sup> century “green revolution”, resulted in a net loss of soil carbon. Treating cropland as grassland is an unrealistic assumption because the productivity of cropland is different than natural grasslands, and dramatically so in the past. We also note that treating pasture as grassland also produces unrealistically low LUC emissions since many areas classified as pasture are relatively low productivity, often semi-arid, ecosystems.

This raises the issue of how cropland and pasture, which are land-use categories, are treated in spatially-explicit ecosystem models, including those embedded in global earth systems models, which are designed to represent different ecosystems. While specific crops can be represented in ecosystem models, we emphasize here the importance of including, perhaps exogenously, the net productivity and physiological changes (e.g. harvest index) that occurred over time due to changes in management practices and changes in crop phenotypes.

Spatially detailed models seem less likely to contain biases in representing conversion of land to pasture given that these models explicitly represent spatially varying productivity. Some ambiguities are still present, however, such as how, from an ecosystem perspective, the conversion of, for example, forested land to pasture should be represented. Representation of pasture could be a larger issue in simpler land-use models.

Pg. 4176, l. 27. “While the substantial uncertainty in LUC emissions were about 10% lower than in our central case,” doesn’t make sense in the context of the sentence. Please rephrase.

Sentence rewritten.

Table 1. Add a line break between “Western Europe” and “Japan”. “Former Soviet Union” has been split across the two columns. “Australia and New Zealand”, rather than “Australian Z”.

Will change. This was not what was in the original submission. This table will be split in two as suggested by the second referee.

Table 2. Stating that uptake is negative in the caption would assist the reader.

Good suggestion, done.

Table 4. I think the third column should be labeled “1850-2000”. Also the totals for G-CARBON are in correctly rounded to 250 and 210, instead of 253 and 211. If the intention was to round to two significant figures, then this appears neither necessary, nor consistent throughout the table.

Yes, column 3 should be 1850-2000. The intention was, indeed, to consistently round to two digits since this is what is how data is reported in a number of previous studies. This way results are consistent across the table.

Table 5. As mention above, significant inconsistencies with the main text and SI.

This will be checked.

Table 6. I presume that the lower row in each section corresponds to GCAM? The labeling is incorrect.

The labeling was, indeed, unclear – corrected as suggested.

Fig. 1. Shrubland and pasture lines are difficult to differentiate. Fig.2 There is no grey line on the key. The comments below apply to the supplementary information. Section 2.1. Units missing for table.

Units added to supplementary table. SM Figure 1 legend corrected. We’ve done our best to make the lines differentiable, although it will be difficult to differentiate many of the lines, which are very similar in magnitude. The entire output dataset, however, will be deposited in a public data repository so the entire dataset can be examined.

First table in Section 3.1. What are the Feedbacks? They are not described in the main text or the SI. Please either add sufficient description and interpretation, or remove these

value from the table.

Description of feedback parameters will be added. While this is not the primary focus of this paper, this is useful for context, and, as suggested we will discuss this in the text.

Section 3.2 VEGAS and CESM have not been previously defined in the SI.  
Definitions will be added.

Pg. 11, l. 15. Do you mean “separated into 10% increments of forest cover”?  
Yes. Text edited to clarify.

Pg. 13, l. 4. “Because peat does not build-up,. . .” C1315  
Changed.

Pg. 14, l. 31. “historical time period”.  
SM text merged with main text as suggested.

Pg. 15., l. 37. “is increased from 15% to 40%”.  
Changed.

Pg. 16, l. 7. Do you have boreal forests in tropical regions?  
The term is used to be consistent with the rest of the text (forests are separated into two categories in this model, boreal and non-boreal forests. There are tiny amounts of boreal forests in some tropical regions, perhaps at high altitudes.).

### **Anonymous Referee #2**

This paper presents a simplified model to quantify land use emissions based on prescribed land use maps. The paper consists of a model description, a short comparison of globally aggregated numbers to selected previous estimates, and an analysis of the sensitivity of the results to selected model and data assumptions.

The model and study setup are acceptable, but I am not convinced the paper delivers much new insight scientifically. My concerns are specifically the following:  
(1) The model presented just adds one further model to the pool of a few dozens established models that quantify land use emissions. It is a highly parameterized simple model that is based on input data from a large range of different other models and datasets; this may be fine for the sensitivity studies, but inconsistencies between the model input are obvious. The model variables are not evaluated against any observations, and model results are compared to previous studies only at the aggregated level of global emissions. The simulated emissions are at the very high end and I would need more analysis, e.g. evaluation against other data at the regional level, to trust this model.

We will discuss these points below. These are helpful comments that point out areas that require further discussion/elaboration in the text, which will be added.

The sensitivity tests we perform here are a fundamentally new contribution to the literature. Spatially detailed models are too complex to easily perform the wide range of

sensitivity tests we conduct here. Examination of published papers (for example those listed in Table 4) finds that generally just one (if any) of the dimensions considered here (for example land-use history) is examined. Sensitivity to equilibrium carbon stocks is almost never examined by existing models (in large part because for the most complex models this is an emergent property of the model, although of course, these values differ between models). Nor have different assumptions for land-use change dynamics or different representations of crop or pasture lands been examined. Simple carbon models, such as those generally used in integrated assessment, do not contain enough mechanistic detail to conduct the experiments considered here. This leaves a gap in the whereby there is not one model that can be used to conduct a coordinated analysis across a wide range of parameters.

The sentiment of the referee regarding new models is understandable. We have added additional text to the manuscript placing this work in context:

Analysis of the carbon-cycle is conducted with a range of model structures. The most sophisticated analyses are produced from spatially-resolved process oriented models that aim to produce estimate based on fundamental biological and physical principles. There is, however, still significant uncertainty in such results. On the other end of the spectrum more parameterized, although often physically-based, carbon-cycle representations are used in integrated assessment models (van Vuuren et al. 2009, Wigley 1993). Such models can be used to explore, for example, the implications of uncertainty in the carbon-cycle for climate policy costs (Smith and Edmonds 2006). The model used here is still highly parameterized, but incorporates greater spatial and process-level detail than most integrated assessment models, including full integration of carbon-cycle and land-use dynamics. We note that land-use dynamics is rarely integrated into simple models.

In addition to the type of sensitivity analysis conducted here, this analysis is also useful as a bridge between more complex models and integrated assessment. The G-Carbon model is designed to be calibrated to spatially-detailed ecosystem models so that analysis consistent with these models can be conducted in a more flexible and fast framework (although, as we note later, the data available from more detailed models is generally not provided in an ideal form for this calibration).

It is true that the results here are higher than those from many existing analyses, as we discuss in the paper, a major reason for this is a combination of two issues with many previous analyses: 1) neglect of the impact of forest harvest, and 2) simplistic (or no) treatment of changing cropland productivity over time. Both of these factors increase historical emission estimates.

Unfortunately the dataset used in our analysis (as with the CMIP5 comparison) also appears to contain a data artifact that produces an artificial “bump” in emissions due to data discontinuities in the mid-20<sup>th</sup> century. As now mentioned in the text, this likely contributes to a small overestimate.

Regarding consistency, we disagree that the assumptions made in this model are

inconsistent. We will add discussion on this point to the text. The G-Carbon model was designed to be calibrated using the results of more detailed models so as to provide a simple modeling framework that is consistent with the carbon states simulated by more complex models. As now noted explicitly in the text, our finding that the largest sensitivity is to equilibrium carbon contents supports our approach of calibrating to this output from more complex models. Of course not every potential variable is necessarily available from any one ecosystem model, so some data must be taken from other sources. But this is also a strength of the type of modeling approach taken here: it is possible to construct a more consistent and complete representation of the terrestrial system, albeit in a simplified manner. This is also part of the reason we do not conduct an extensive comparisons with observational data: we calibrate instead to other models, building on that previous work where these comparisons have been done. Also, observational data on, for example, carbon stocks, is not on a scale commensurate with the regional scale of our model. Where we do compare with such data, as we now make more explicit in the text, our results focus on model results without including climate and CO<sub>2</sub> feedbacks, which are, of course, reflected in the results of, for example, regional carbon inventory datasets.

It would be useful to compare with results at the regional level from other models (for that matter, as mentioned above, it would be useful to have more results from models to use as calibration data). These data are not available at present in a form that enables such comparisons without extensive data collection and processing (e.g., what is needed is multi-model comparison databases of ecosystem and land-use model simulations without CO<sub>2</sub> and climate feedbacks). We hope that some of the on-going comparison projects (e.g., MsTMIP) can provide such data.

(2) The scientific question answered in this manuscript seems to be “How sensitive is our new model to various assumptions and how well can it reproduce estimates of historical and the RCP4.5 IAM emissions?” This is of interest to those who will use this model in the future, but I am not sure why any other reader, having established, documented, and validated tools at hand, should care about this at the moment. The core of this paper should be an interesting scientific question, not a model description. It should become clear why the authors chose to develop a new model instead of using an established one.

The primary scientific question we pose, as stated in the introduction is: “how do different assumptions for ecosystem properties and the representation of anthropogenic land-uses impact estimates of the resulting net release in terrestrial CO<sub>2</sub> over time”. As discussed above, this question has not been examined with existing models. (Note also the comments by the first referee supporting this approach.)

It is true that we also use this model to examine results from the simple land-use model used in GCAM, although this is not the main focus of this paper. Of course this research was conducted within the same research group that developed GCAM, so we hope that the results of this more detailed model can be used to improve the carbon-cycle representation within GCAM.

(3) Uncertainties associated with land use emissions are huge and sensitivity studies as



performed here are therefore very valuable. However, the motivation behind the selection of which variables to test does not become clear. The sensitivity analysis does not cover the full range of potential uncertainty and the rationale behind testing the sensitivity to specific datasets and not others is not described (e.g., the choice of CASA, CESM, and VEGAS output for carbon densities seems arbitrary). The sensitivity analysis, if better justified, is very helpful and something other publications of new models often lack. But the analysis per se is not a novel scientific question, because the relative and absolute sensitivities depend strongly on model assumptions and are not generally applicable to other land use emission models.

We will add further discussion for our choice of input parameters to the text. As this referee usefully points out, we did not sufficiently discuss the choice of carbon content values in particular. The choice of CASA, CESM, and VEGAS was motivated by data availability. While some additional data sets have recently become available that might be useful for this purpose (for example the ESMs participating in CMIP5), at the time of this research these were the only models that shared their data. (Others were contacted but did not respond.) Even so, we feel that this work will provide valuable guidance to future comparison exercise that, hopefully, will be more comprehensive and use a wide range of models. One substantial limitation we encountered is that most ecosystem models output carbon values only at the grid cell level instead of at the ecosystem level (given that the representation within most models accommodates some level of heterogeneity within a grid cells). This means that forest carbon stocks (above and below ground) must be inferred. We found this to be a significant issue in both understanding the results of other carbon-cycle models and also using those results for calibration. One conclusion of our work is that this should be rectified in the future.

Where we could identify a relevant uncertainty range, our sensitivity analysis covered enough of the range so as to be able to determine the relative sensitivity of results to the specified variable. For many of these variables there is no rigorous method to define what “full range” of variable values would be, so we have reviewed the literature and used our best judgment. The data limits discussed above did limit our exploration of carbon density, and it would be useful to extend this analysis using data sets from a wider set of models. We feel that the results here covered enough of the range to identify key uncertain parameters. We were able to conclude, for example, that equilibrium carbon density values are the key outputs needed from more complex models in order to use a tool such as the one used here in an emulation mode to examine questions that would be logistically difficult to explore using more complex models.

As noted by the reviewer, sensitivity analysis can be used to determine where future work should be focused. In addition to the issues associated with equilibrium carbon densities discussed above, we show here that the representation of cropland (and perhaps pasture as well) in ecosystem models needs to be carefully considered. There is no reason to think that these results would not generally apply to other models given that we have represented, in a mechanistically sound manner, the basic dynamics of the terrestrial carbon system. (Of course if this is not the case, this would also be scientifically interesting.)

(4) Much of the relevant information on the model is put into the supplemental material. The frequent references to the SM make the paper hard to read. Also, the specific chapter of the SM should be referenced.

We have tried to structure the paper so that much of the material focused on model and parameter documentation is contained in the SM. The main paper is focused on the scientific results. We feel that this leads to a reasonable tradeoff between readability and detail, but of course, opinions will differ on this point.

The SM sections will be numbered and specifically referenced (as also suggested by referee #1).

To summarize, the paper is the documentation of a model with which potentially interesting studies can be performed (in particular, the authors' good understanding of the IAM assumptions and how they may differ from biosphere models offers much potential). As such, it is a fine paper, but I doubt that it fits into the scope and aims of BG. I recommend rephrasing the manuscript as a documentation and submitting it to one of the journals that deal specifically with model descriptions (e.g., Geoscientific Model Development).

While model description had to be included in the paper, we feel that the scientific content is both new and relevant for this journal. Much of the model description detail is left to the supplement to keep the paper readable. Only six out of 19 pages of text are devoted to model and data description.

The following suggestions should be taken into account before publication:

- The study accounts for observed trends in crop productivity and for trends in forest NPP. In particular for the first it is not clear in how far management effects can be separated from the effects of environmental changes. The increase in productivity is likely driven also by factors such as CO<sub>2</sub>-fertilization. It may be good to discuss this and to add an analysis that excludes all exogenous trends, which would be comparable to a range of previous studies that simulated emissions under constant environmental conditions. It is further not clear how the assumed trends can be applied to the future.

Note that carbon dioxide fertilization and temperature feedbacks are explicitly not included in the model results discussed in this paper. This is keeping with the standard use in the literature of net land-use emissions being an estimate of net ecosystem changes exclusive of feedbacks. We will note in the paper that any effects of CO<sub>2</sub> and temperature feedbacks on crop NPP are included (since we use observed values). This is a modest in-consistency in the model set-up (although likely to be small). In any event the dominant drivers of crop NPP changes over time are changes in cultivars and management practices (particularly fertilizer use). We do examine cropland trends in more detail in a subsequent paper. We will check the literature for estimates of crop NPP trends over time with and without climate feedbacks. If this has been done, this would be useful to cite, as suggested. Note that an increase in forest NPP (through management and nitrogen deposition) is included as well, but not climate feedbacks.

- Sec. 3.3.1, potential vegetation map: If the dataset is corrected with MODIS data, why not use MODIS right away? This points to the issue of ad hoc choices for many of the input data.

Because MODIS is not a dataset for potential vegetation, it is a dataset of the current distribution of vegetation. Substantial additional work is required to convert this to a potential vegetation dataset and this was beyond the scope of this project. At the time this research was conducted the SAGE potential vegetation dataset was the only such dataset available. Our primary use of MODIS data was for boreal areas where there is minimal anthropogenic land-use change, thus rendering this use tractable (although, as noted in the text, this means that we may overestimate the pre-industrial extent of shrubland). We look forward to the publication of improved potential vegetation datasets.

- Sec. 3.3.2, cohorts of 50 years length: Please elaborate the effect of cohorts. Usually, cohorts are introduced to models to be able to represent the changes in productivity with age, but for this cohorts need to be split up much finer for the first decades of age.

The age cohorts here are used to resolve forest harvest and land-use change dynamics. This is key feature of this model (and, we might add, one that does not always exist even in more detailed ecosystem models). These are not used to resolve productivity changes over time: forests age within their cohorts until a land-use change occurs (forest harvest or conversion to another land use). We will clarify this in the text.

- Sec. 4.1: P. 4165 explains that emissions are attributed to the ecosystem that loses land, but on p. 4167 it says that land converted to cropland remained a carbon source due to slow equilibration of soil carbon, suggesting the legacy emissions are indeed attributed to the ecosystem that gains land (which would make more sense).

We apologize for the confusion (which referee 1 also commented on) and the text will be clarified to make clear our assumptions for cropland carbon dynamics.

- Tab. 1 should be split into two tables.

This is a good suggestion and will be taken up.

- The manuscript reads very well. A few typos/grammar issues are on p. 4170, l. 3; p. 4171, l. 20; p. 4172, l. 17-19.

Thank you, we will attend to these.