To the editor and reviewers:

We thank the reviewers very much for their constructive criticism and encouraging comments. We are pleased that all three referees judge our study as important and worth publishing after revisions. Please find below our response to each individual comment (reviewer's comment in **bold font**, our response in normal red font, with text cited from the revised manuscript indented and in *italics*).

Best,

Julia Pongratz for all authors

Review by C.D. Jones

This paper is a much needed and detailed exposition of the multitude of ways of defining "land use" carbon emissions. It has long been known that land-use emissions of carbon are hard to either measure in reality or diagnose from model studies. It is becoming recognised that results of attempts to do so are actually quantifying different quantities and that these should not be compared side-by-side. For example Arora et al have previously presented results from several experiments and discussed different definitions of land-use flux.

No changes required.

But there has been lacking a comprehensive review of the very many aspects of this discussion, and their implications. This paper provides that and hence offers a road to some clarity on the issue. The bad news is that the issue is extremely complex! But that simply makes this paper more important. This paper will become a good reference text for anyone designing an experiment - it is crucial to define up front exactly what you want to quantify. Secondly it will provide a good "best practive" guide for how to present results. Given the complexity, it was useful therefore for the authors to explicitly pick out 3 key aspects which they saw as most important.

No changes required.

I think it is beyond the remit of this paper (or any paper) to recommend a specific single definition of "land use flux" and the authors wisely stop short of this. Instead it is likely that we are stuck with multiple definitions of land-use emission - but at least removing misinterpretation of different published values is achievable.

No changes required.

I particularly liked the comparison of published studies as a way to demonstrate the use of the framework. Up to the end of section 2 the discussion is in times quite abstract and hard to follow. The text, table and figures together do a good job at explaining things, but the concept is sufficiently complex and subtle that it is impossible to follow at first reading. Some "worked examples" therefore are very useful.

No changes required.

My biggest request would be to extend the analysis to include the IAMs which produced the RCP scenarios. "land use emissions" are provided as an output from these (e.g. follow links from here: http://cmip-pcmdi.llnl.gov/cmip5/forcing.html) - can you use your framework to elucidate the exact definition of what they mean? Two important questions: a) the land-use emissions (as used to drive ESMs in CMIP5 for example) have been harmonised to merge smoothly with Houghton et al at year 2005. (e.g. see Jones et al., 2011, GMD documentation of HadGEM2-ES experiments). Is this a sensible thing to do? or have we wrongly harmonised timeseries of inconsistent things? b) there are 4 RCPs from 4 IAMs - are they even using the same definition of "land use emission" between them?

We thank the reviewer for this excellent suggestion. Digging through references' references and contacting the IAM groups we have identified that two different methods for calculating the land use emissions have been used: the IMAGE model performed with/without-LULCC offline simulations with their biosphere model component. MESSAGE, AIM, and GCAM all used an approach identical or similar to Houghton's bookkeeping method. These two methods correspond to approach D3 and B, respectively, in our manuscript, with the consequence that the IMAGE estimates include the unrealized emissions from the loss of additional sink capacity (LASC). Therefore, the IMAGE emissions should not have been harmonized with the other three models to a common value for 2005 (1.2 GtC/year), as described by Van Vuuren et al and Meinshausen et al, Clim. Change, 2011; and they should not have been harmonized with the historical bookkeeping estimates. This inconsistency may have gone unnoticed and may not matter practically very much, because it happened to have an only moderate effect: as discussed in our paper, the LASC has been small for the historical time period, and IMAGE happened to provide the weakest RCP, where changes in atmospheric CO_2 are relatively small, so that the LASC effect is small as well. (To reassure the reviewer: We believe this inconsistency caused by terminology is of minor relevance as compared to the much more severe changes (of up to 200%!) of the original IAM data when averaging them to a common 2005 value and scaling all data until 2050, which are caused by inherent model differences in the representation of basic processes and the different choice of carbon density and land cover data.)

We have added to the discussion part a section (now 4.6) discussing these findings. The section discusses the relevance of terminology for any study applying net land use flux estimates of various definitions. This includes a cautionary statement that the CMIP5 CMOR output for "Net Carbon Mass Flux into Atmosphere due to Land Use Change" may also be provided for the different models on the basis of different terminologies (e.g., the MPI-ESM modeling group reports in this variable only instantaneous harvest fluxes, thus a definition similar to approach "S" and very different from the definitions this modeling group usually uses in publications as their "net LULCC flux"). Also included in this section is the GCP RECCAP study.

> *4.6 Relevance for studies applying net LULCC flux estimates of multiple definitions*
>
> *Beyond its relevance for the definition of the residual terrestrial flux, the exact definition of the net LULCC flux becomes particularly important when estimates from several studies applying different methods are combined in one synthesis. Houghton et al. (2012) explicitly discussed the particular treatment of environmental changes as one reason for the large spread of the net LULCC flux estimates, and indeed the 13 referenced datasets used 4 different definitions (methods B, D1, D3, D5; Fig. 2 indicates the choice of method for each reference). Similar differences exist between the 5 net LULCC flux estimates used in the quantification of the global carbon budget by Le Quéré et al. (2013) (B, D1, D3, S, and a combination of B and S). Based on such experiences, it is plausible to assume that the estimates reported for the Coupled Model Intercomparison Project 5 (CMIP5) as "Net Carbon Mass Flux into Atmosphere due to Land Use Change" face a similar divergence in terms of definitions applied.*
>
> *Terminological differences also occurred during the development stage of the four scenarios for future anthropogenic emissions and climate for the IPCC Fifth Assessment Report produced by four Integrated Assessment Models (IAMs). The IAM models estimated change in land cover and management in accordance with economic assumptions, but also quantified CO2 emissions from LULCC. These quantifications were needed by some of the ESMs included in the CMIP5 comparison that did not endogeneously calculate the net LULCC flux and thus needed to prescribe LULCC emissions from the IAMs in the same way as fossil-fuel emissions were prescribed. While 3 of the 4 IAMs, GCAM, AIM, and MESSAGE, followed an approach identical or similar to Houghton's bookkeeping method (method B in our paper), the IMAGE model included effects of changing atmospheric CO2 on plant productivity. Following the method by van Minnen et al. (2009) (method D3), the net LULCC flux provided for the IMAGE model includes the effect of environmental changes on instantaneous and legacy emissions as well as the unrealized emissions from the loss of additional sink capacity (LASC). Despite this discrepancy in methods, all four scenarios were harmonized to a common value of the net LULCC flux in 2005 (it needs to be noted, however, that differences between the 4 IAMs due to different processes represented by the models and differences in input data led to differences in net LULCC flux estimates that are at least as large as terminological differences; see*

Meinshausen et al., 2011). To achieve a continuous time series of past and future LULCC emissions, the IAM data was further harmonized with the historical net LULCC flux by adjusting Houghton's bookkeeping estimates to match the present-day fluxes as simulated by the IAMs (e.g., Jones et al., 2011). While both harmonizations are thus inconsistent, the actual discrepancy introduced by comparing two different definitions may have happened to not be very large in this specific case: As discussed in Section 4.2, the LASC has been small for the historical time period, and is likely small also in the IMAGE scenario providing the Representative Concentration Pathway of 2.6 W/m2, which assumes the lowest changes in atmospheric CO2 concentration of the 4 RCPs.

Nevertheless, a framework as the one provided by our study can in the future serve as a reference to identify up front which components of the net LULCC flux (and the residual sink) will be quantified by an agreed model setup. It may thus, for a given scientific question, contribute to a convergence of methodology and therefore a reduction in the uncertainty of the most uncertain components of the global carbon budget.

To reflect this extended discussion on the relevance of the terminological differences in net LULCC flux estimates for estimates of the residual sink, we have further added the sentence to the abstract:

The spread in estimates caused by terminological differences is conveyed to those of the residual sink. Furthermore, the application of inconsistent definitions of net LULCC flux and residual sink have led to double-counting of fluxes in the past.

In our effort to keep up to date with the literature we have further added a discussion of the recent paper by Shevliakova et al in Section 4.5 and of Le Quéré, subm:

Even if consistently the same model is used to derive the components of the carbon budget, so that no double-counting occurs, the choice of definition for the net LULCC flux determines the definition of the residual sink (Gasser and Ciais, 2013). For example, in the recent study by Shevliakova et al. (2013), method D1 has been used for the net LULCC flux and this estimate has been subtracted from the net land-atmosphere flux of a coupled ESM simulation accounting for LULCC and fossil-fuel burning; in this case, the resulting residual sink includes the additional instantaneous and legacy emissions of LULCC induced by environmental changes, δlfI and δlfL, which is not in line with other estimates of the residual sink (e.g., Le Quéré et al., subm).

I think an addition of this would be very valuable, after which I recommend this paper be published with minor revisions. A few specific comments and suggestions follow.

Chris Jones

See previous response.

- p. 683 - either the authors or the typesetters need to be very careful here that "I" in equation 2 does not look identical to "l" on line 27. There's already enough scope for confusion without two symbols looking the same!

Unlike the discussion format, ESD final publications use font with serifs, which should eliminate the potential for confusion; we will be careful in checking this later.

- I found the definition of "p" (potential natural veg on managed land) hard to follow early on and wondered how it differed from "recovering areas". It became clear later that it is a hypothetical (what would have been there) state. But this doesn't come through clearly at first definition.

Table 2, which contains the detailed definitions, will be at the appropriate place in the main body of the text in a published version. We have further extended the explanation of the need for and meaning of p in the text:

> *A minimal set of land types needed to distinguish the different methods comprises managed land, including recovering areas ("m"), natural land unaffected by direct LULCC activity ("n"), and potential natural vegetation on managed land, which represents the same areas as managed land but assumes the vegetation were unaffected by LULCC ("p") (see Tab. 2 for detailed definitions). The potential natural vegetation is important for the "without-LULCC" simulation that is needed as reference to quantify the human impact by LULCC as compared to a world without LULCC interference, and thus assumes the hypothetical land cover unaffected by man , "p", in lieu of "m".*

In response to the other reviewers' comments we have added on the role of the without-LULCC reference (for which "p" is important) in the discussion of eq. 1, which should further clarify this issue.

- in figure 1b what drives your choice of sign for each arrow? the fact some are up versus down implies you have determined the sign of each, but it's not clear why - e.g. many of these could be either + or - couldn't they? e.g. environemntal changes due to fossil fuel burning might have created a source or sink depending on local climate changes.

We have expanded our clarification of the arrow direction in the figure caption (Fig. 1b, where the arrows first occur):

> *Note that all fluxes apart from instantaneous emissions (I) may act as source or*

sink of carbon on land; the arrow directions indicated here loosely refer to historical evidence for global fluxes, but in fact depend on region, assumed scenario of LULCC and environmental conditions, and model.

- sec. 4.4 - nice discussion around what is observable. But even if we can't observe a total or net flux, can you comment on any constraints we can get on components of the problem? e.g. ESMs (and I assume DGVMs too) vary hugely in their biomass (more than factor 2-4 between CMIP5 EMSs - see Anav et al., 2013, J. Clim). So presumably observational constraints on biomass will to some extent reduce the part of land-use emissions to do with removal of biomass. Are there any other observations which can help?

We fully agree that the use of observational data in the evaluation of certain model variables (such as biomass density) is useful in reducing the range of emission estimates. However, such a discussion is beyond the scope of the paper: As clarified further in response to comments by reviewer Thomas Gasser (see introduction) our study aims at an illustrative framework that allows for comparing terminologies, but not at actual quantifications of the net land use flux, for which observational constraints are crucial. The issue is partly addressed in other studies (e.g., Houghton et al, BG, 2013) and in new initiatives of which the reviewer is part, and we therefore prefer to not add it to our already long and complex paper.

Review by T. Gasser

This paper by Pongratz et al. presents a symbolic framework describing the net land-to-atmosphere CO_2 flux, that is then used to review the various definitions of "emissions from land use and land-cover change" that can be found in the literature.

No changes required.

Although we did write a paper about this issue several months ago (Gasser and Ciais, 2013; GC13 hereafter), some of our conclusions being similar to the ones of Pongratz et al., this new paper has the real added-value of making a full review of the literature, which we did not. Besides, the framework they developed, using only symbols, may be a good pedagogical alternative to our more mathematical framework.

No changes required.

Thus I believe this article is of good interest for the community. However, I have several concerns related to the framework itself, and some suggestions and/or corrections that I would like to see be answered before publication.

No changes required.

1 Introduction

I think it is important to recall the reader that there is an arbitrary, human-made, decision behind the notion of "emissions from land use and land-cover change". Surely for political reasons (negotiations), we have been willing to isolate direct and indirect effects of human activities over the terrestrial ecosystems. However, since Earth is not a simple – linear – system, the sum of two causes (direct + indirect) leads to more than two effects, which in turn leads to all the issues raised in this paper.

We have added a sentence to the introduction clarifying that the definition e.g. of the residual terrestrial flux is a human-made decision:

> *As will be shown in this article, the indirect effects are accounted for to very different extents and in very different ways in published estimates of the net LULCC flux. This is partly due to the political or scientific purpose for which a methodology is employed, but is often also constrained by the nature of available data or modeling tools.*

p.680 l.16–18 It is actually possible to build book-keeping models that respond to transient changes in e.g. CO_2. Though the paper fully describing our model (OSCAR v2) is not yet published, the appendix A of GC13 also provides the equations necessary to do so. Note that the original version of our model (Gitz and Ciais, 2003) was also a book-keeping model accounting for environmental changes.

We have extended the discussion of bookkeeping models, distinguishing between the original, purely inventory-based approach of Houghton et al. and later models in the introduction:

> *Later approaches (e.g., Gitz and Ciais, 2003) include a modification factor to the growth curves depending on the atmospheric CO_2-concentration to account in a simplified form for the effects of environmental changes on carbon stocks.*

Sec. 3.4 reflects this specification and is now entitled "Inventory-based bookkeeping models". Fig. 2 also uses this term now.

p.681 l.3–8 The net land use flux is not always defined with respect to a reference state. See the definition S of this very paper, or my comment on definition B. Here, I would introduce a specific notation for $\Phi_{noLULCC}$ to highlight that there is no land use ; e.g. Φ^*, that I will use hereafter, since I think the "*" already used for simulation at the equilibrium is useless. Hence, the net land use flux becomes : $F = \Phi - \Phi^*$ for definitions using a reference state.

Definition S is a special case by not using a without-LULCC reference, as becomes clear in the text and in Fig. 2, and, due to its incompleteness, does not conform to any other definition of the net land use flux used in estimates of the global carbon budget. We have substantially expanded the discussion of the reference in the introduction:

> *We acknowledge that the reference may not always be simulated explicitly in a separate simulation; in particular in cases where environmental conditions are assumed to not change over the course of the LULCC-simulation the initial state of carbon stocks and fluxes in the LULCC-simulation may serve as implicit reference. It is also possible to quantify individual flux components, such as instantaneous LULCC emissions, without an explicit reference. However, the without-LULCC reference is crucially needed, implicitly or explicitly, to answer the question of LULCC impact as compared to a world that had not seen such human disturbance.*

We prefer to keep to our original, explicit, terminology of "$\Phi_{noLULCC}$" because only this terminology emphasizes the key point of the reference simulation: that it does *not* include the effects of LULCC.

p.681 l.11–14 Do we really, in GC13, "consider only a subset of relevant flux components"? All the definitions given by Pongratz et al. can easily be expressed with our framework. But I acknowlegde that not all were discussed, since we thought that some definitions were clearly wrong definitions! See key issue number 1).

As the reviewer Chris Jones highlights as strength of our paper, we refrain from labeling any approach as "right" or "wrong"; which definition of the net land use flux is the correct one to use entirely depends on the specific scientific question. That all approaches we describe are deemed relevant by the land use and carbon cycle communities is shown by the fact that all have been published in peer-reviewed journals; we therefore find it very important to depict them all.

However, we have clarified in the manuscript that another key difference between our study and previous studies such as Strassmann et al. (2008) and Gasser and Ciais (2013) lies in the fact that the latter provide mathematical definitions that can be used to quantify (within the limits of the given modeling framework) the individual fluxes. Our study by contrast aims at a comprehensive, illustrative framework that allows the reader to understand how published estimates of the net land use flux differ in terms of their definition due to the specific type of model and model setup used to derive them.

> *While these previous studies provided mathematical formulations that allow for actual quantifications of various fluxes, our study aims at a comprehensive, illustrative framework that allows the reader to understand how published estimates of the net LULCC flux differ in terms of their definition due to the specific type of model and model setup used to derive them.*

2 Materials and methods

I am concerned that this whole section could be hard to understand. Despite being well organized, as it progressively introduces the notations, I think some rewriting, with the aim of simplifying the section, could improve the paper.

The implementation of all reviewers' comments has greatly contributed to improving the clarity of this section. We have also reorganized lines of thoughts, eliminated potentials for misunderstanding, and added clarifying statements as needed beyond the reviewers' explicit suggestions. We appreciate that overall the reviewer sees this section as well organized.

Now, about the framework :

2.1 The three fluxes I, L and E

Since the authors acknowledge that the distinction between I and L "is not relevant for [this] study", a point on which I agree, why keep on writing the two fluxes in all

subsequent equations? In the following, I will use only L.

The distinction is not relevant for our study with respect to the exact timescales and processes that are used to split emissions into instantaneous and legacy. The distinction is very important to our study, however, because the instantaneous emissions are the only flux accounted for in all studies quantifying the net LULCC flux, while the legacy emissions are partly omitted by some approaches (such as "S"). We have phrased this more explicitly now:

> *However, it is necessary to keep the two fluxes separate in our framework; it will illustrate that the instantaneous emissions are the only flux always accounted for, while the legacy emissions are partly omitted by some model approaches. Note that for satellite and some inventory-data based approaches, the time period of analysis is shorter than the timescale of legacy effects, so that legacy emissions may be partially omitted, or all emissions may be assumed to be instantaneous (e.g., DeFries et al., 2002; Harris et al., 2012; IPCC 2006; FAO 2013).*

2.2 The LULCC perturbation only Hence, the preindustrial case is: $F = L_U + E_U = L_U$ 2.3 The environmental perturbation only

No changes required.

I think the changes in environmental conditions should not be presented as being either LULCC or fossil. Although this paper focuses on the CO_2 flux related to land use, the environmental perturbation is not only CO_2-driven! Actually, the complementary perturbation to LULCC is non-LULCC, and it includes everything else, from aerosols emitted by fossil fuel burning to nitrogen compounds emitted because of fertilizers. Hence, I strongly suggest the use of three environmental conditions : "U" (for preindustrial), "L" (for land use-related) and "X" (for anything else). When both land use and others are present, I use the "LX" notation. (Plus, I'd rather use upper case subscripts for environmental conditions).

We agree that the framework should not be limited to either LULCC or fossil CO2 emissions. We had clarified in the introduction of the original version of our manuscript that environmental changes refer to not only CO2-fertilization, but other processes such as climate change and nitrogen changes in nutrient cycles as well.

Our framework is in general valid for both emissions caused by land use change / land cover change and emissions caused by land use activities (such as fertilizing). It is the ability and setup of an actual model used in a study that determines which processes are attributed to LULCC and which not. We have followed the reviewer's suggestion and have clarified now that "f" includes everything not included by "l".

LULCC-induced changes in environmental conditions ("l"), and other human-induced changes in environmental conditions, most notably due to fossil-fuel burning (thus labeled "f").

It should be noted that almost all the papers cited consider that X is only fossil fuel. But it is not a reason to restrict the framework, especially as it could mislead the reader.

See previous response.

Plus, I don't find equation 4 very relevant for the paper, as it is not used at all after that point.

Eq. 4 (now Eq. 5) is a clarification of the flux "E" and is needed to understand in particular the loss of additional sink capacity defined in Eq. 7 of the original manuscript (now Eq. 8). Aiming at maximum clarity we prefer to keep the equation. We have simplified the representation, however, and for ease of reading explicitly written down the two cases for a simulation with managed land and a simulation with potential natural land.

2.4 Effect of E over L

The term "managed" might be misleading: abandoned (hence, not managed) lands are included here. The "managed" land are what we called, in GC13, "disturbed" lands ; the "natural" are our "undisturbed" lands.

We agree that the term "managed" may not be ideal, however "disturbed" is equally or more confusing because areas unaffected by humans are disturbed by natural processes such as wind-throw and fires. Under UNFCCC reporting, managed lands once abandoned are still reported under managed lands. "Disturbed" specifically relates to natural disturbance. We believe that a clear definition of the terms used is sufficient, in particular because the attribution of an abandoned field to either managed/disturbed or natural/undisturbed is relevant only when the fluxes are actually quantified, not to understand how published methods generally differ with respect to their terminology of the net land use flux. A clear definition is given in Table 2 and will, unlike in the draft version that was accessible to the reviewers, be placed prominently at the appropriate place in the text in the published version so that it cannot be overlooked.

With my notations, the equation becomes: $L = L_U + \tilde{\delta}_Z L$; with Z being LX, L, X or U; and $\tilde{\delta}$ being an operator that describes the perturbation.

No changes required.

p.686 l.7 δI and δL are consequences of δE prior and *posterior* to the LULCC event (e.g. changes in temperatures will affect the rate of soil carbon density stabilization).

We thank the reviewer for pointing out that this was confusing. Indeed, environmental changes after the LULCC event influence the temporal distribution of emissions – e.g., the delayed emissions arising from the decay of wood residuals after deforestation will depend on the soil moisture and temperature evolution. However, these effects are already accounted for in our terminological framework within the terms I and L. We have clarified this in Tab. 1:

> *The cumulation of I + L over time yields the „committed emissions" (see Houghton et al., 2012) under the respective scenario of future environmental changes that a study assumes.*

Note again that our paper, unlike Strassmann et al. (2008) and Gasser and Ciais (2013), provides a terminological review but does not aim at the actual quantification of the fluxes, for which the exact temporal evolution of environmental conditions need to be known.

2.5 Effect of L over E With my notations : LASC = $\tilde{\delta}$ ZEm − $\tilde{\delta}$ ZEp = $\tilde{\delta}$ Z[Em − Ep].
2.6 Non-linearity

This section is a bit unclear. And it reveals a shortcoming of the framework : the authors acknowledge that the δX En flux is also a (second order) function of the LULCC perturbation! This raises the question of the consistency of the notation, and hence of the framework.

Response following below.

Despite being more complicated, I suggest that the details of the non-linearity issue should be exposed, and dealt with thanks to a relevant notation. My proposal uses the operator notation that encompasses different definition of the δ-fluxes, depending on the environmental conditions :

$\tilde{\delta}$ L = δL

$\tilde{\delta}$ X = δX

$\tilde{\delta}$ LX =δL+δX+δ^2LX

The δ^2_{LX} is the (second order) non-linearity. With this detailled notation, the δX does not depend on the LULCC perturbation, in any case.

We thank the reviewer for pointing us once more to the subtle issue of non-linearity. But we prefer to stick largely to our formulation because it is more compact than the one proposed by the reviewer as it includes all nonlinearities to arbitrary order and not only up to the quadratic ones – although this could obviously not be sufficiently communicated with our exposition in the manuscript. What the reviewer proposes is an expansion into the perturbations by land use and fossil fuel emissions up to second order. By completing the first two equations from above by the quadratic terms δ^2_{LL} and δ^2_{XX}, respectively, one could indeed come to a consistent second order representation of all our equations along the lines pointed out by the reviewer. In such an expansion the δ-operators represent the effect of the linear contribution from the perturbation. Instead our δ-notation is meant to include also all nonlinear contributions. This misunderstanding might have been triggered by use of the relation $\delta_{LX} \approx \delta_L + \delta_X$ which in view of reviewer's third equation above indeed looks at first sight like a linearization. But in the spirit of the factor analysis by Stein and Alpert (J. Atmos. Sci. 1992, now cited in our manuscript) it is meant differently in our study: δ_l and δ_f, which correspond in our notation to the terms denoted δ_L and δ_X by the reviewer, are considered to be nonlinear to arbitrary order in the respective perturbations. By the above approximation we thus dropped only particular nonlinear terms, namely the synergy, but not all nonlinear terms. In this way we wanted to simplify the presentation since accounting for the synergy does not add additional insight for the topic of our paper. Nevertheless, thanks to the reviewer we now realize that to prevent further confusion we have to keep the synergy term explicitly throughout our presentation. Therefore we have expanded the discussion of non-linearities to include synergistic effects now explicitly (Sec. 2.6) and have spelt out now that $\delta_{lf} = \delta_l + \delta_f + \sigma_{lf}$ (the latter term indicating synergies). Accounting in this way for the synergy keeps all equations unchanged, except those where we adopted the above approximation, namely equations (12b) and (13b), the latter detailing method E2. We have also added to Fig. 2 the synergistic term to E2 on natural land (all other synergistic terms are either included in our definition of δ_{lf}, which is now addressed in the caption, or cancel).

I want to highlight that these so-called "second order" components are not necessary negligible. A quick simulation with OSCAR v2.1 brings that the excess atmospheric CO_2 estimated in 2008 when only fossil fuel burning occurs is $\Delta F[CO_2]$ = 62 ppm; when only LULCC activities occur is $\Delta L[CO_2]$ = 32 ppm; and when both occur is $\Delta LF[CO_2]$ = 104 ppm. Hence, a non-linearity for the atmospheric CO_2 of about 10% that will then propagate in the biospheric carbon cycle through e.g. NPP.

See above response.

2.7 Direct and indirect fluxes

I don't find this section relevant, the main reason being that the distinction made by the authors between the direct and indirect fluxes is arbitrary and doesn't help the reader understand the framework or the physics behind. For instance, why should the author's $\delta_{\gamma=\mathrm{f}}$L be accounted for as direct, and $\delta_{\lambda=\mathrm{lf}}$L as indirect? Why should the extra-emissions induced by fossil fuels be more direct than the ones induced by fossil fuels and land use? I believe there are enough arbitrary definitions in the land use- related issues to avoid adding one. Once again : this section isn't helpful to understand either the framework or the results of section 3. Furthermore, removing that section also avoid the use of λ and γ.

The reviewer is correct in that the distinction between direct and indirect terms is not mandatory for our study. However, these are widely used terms (see e.g. Houghton, GCB, 2013, "Keeping management effects separate from environmental effects in terrestrial carbon accounting"), and we therefore have to show how our analysis links with this previous terminology. Separating direct management effects from indirect environmental effects is also extremely relevant to UNFCCC GHG inventory calculations where in theory parties should only report and account for fluxes due to direct human activity. Furthermore, separating the effects of direct and indirect fluxes in our study is part of understanding if and how feedback effects are incorporated by different studies. We would like to note that our use of "direct" and "indirect" refers to the *difference* to a reference simulation (see Eq. 9) and does not refer to individual terms as the reviewer enumerates them.

Generally speaking, I do not recommend the use of the "direct" or "indirect" adjectives when naming things specifically (like fluxes or effects), as many authors before did use them with different meanings. Sometimes "direct" refers to the instantaneous flux (as it is still visible in Table 2, where one can see the authors of this article initially named the I-flux "D"), others to the land use feedback, or to the full effect of environmental conditions change on LULCC emissions, etc.

We have corrected Table 2.

3 Results

p.692 l.13 E1 and E2 descriptions are inverted.

We thank the reviewer for noticing this. Corrected.

In this section, I will express the net land use fluxes obtained with the modified framework, commenting when necessary.

Definition E1:

$$F(E1) = \Phi mn,L - \Phi^{*} pn,U \qquad (1)$$

$$= LU + \delta LL + \delta LEmn$$

Definition E2:

$$F(E2) = \Phi mn,LX - \Phi^{*} pn,X \qquad (2)$$

$$= LU + \tilde{\delta} LXL + \tilde{\delta} LXEmn - \delta XEpn = LU + [\delta L + \delta X + \delta L^{2}X]L + [\delta L + \delta L^{2}X]Emn + \delta X[Em - Ep]$$

Here, the detailed notation becomes interesting! One can see were the non-linear term is accounted for.

No changes required (see our remarks on nonlinearities from above).

Definition S:

$$F(S) = \Phi mn,LX \qquad (3)$$

$$= LU + \tilde{\delta} LXL + \tilde{\delta} LXEmn = LU + [\delta L + \delta X + \delta L^{2}X]L + [\delta L + \delta X + \delta L^{2}X]Emn$$

Definition D1:

$$F(D1) = \Phi mn,U - \Phi^{*} pn,U \qquad (4)$$

$$= LU$$

Definition D2:

$$F(D2) = \Phi mn,L - \Phi^{*} pn,L \qquad (5)$$

$$= LU + \delta LL + \delta L[Em - Ep]$$

Definition D3:

$$F(D3) = \Phi mn,LX - \Phi^{*} pn,LX \qquad (6)$$

$= LU + \tilde{\delta}_{LX}L + \tilde{\delta}_{LX}[Em - Ep]$

Definition D4:

$$F\ (D4) = \Phi_{mn,LX\_CO2} - \Phi^{*}_{pn,LX\_CO2} \quad (7)$$

$$= LU + \tilde{\delta}_{LX\_CO2}L + \tilde{\delta}_{LX\_CO2}[Em - Ep]$$

Actually, the framework should be extended for this particular case D4. Introducing the notation $\tilde{\delta}_{Z1,Z2}$, where Z1 is the specific environmental condition "atmospheric CO2" and Z2 represents all the other environmental conditions (climate, etc.). However, doing so implies to go further with the infinitesimal notation, since we have (by definition) :

$$\tilde{\delta}_{LX,LX} = \delta_{L,L} + \delta_{X,X} \quad (8)$$

$$+ \delta_{L^2,X} + \delta_{X^2,L}$$

$$+ \delta_{L^3X,X} + \delta_{L^3X,L} + \delta_{X^3,LX} + \delta_{L^3,LX}$$

$$+ \delta_{L^4X,LX}$$

For consistency, we can link the elements of $\tilde{\delta}_{Z1,Z2}$ to the ones of $\tilde{\delta}_{Z}$:

$$\delta_L = \delta_{L,L} \quad (9)$$

$$\delta_X = \delta_{X,X}$$

$$\delta_{L^2X} = \delta_{L^2,X} + \delta_{X^2,L} + \delta_{L^3X,X} + \delta_{L^3X,L} + \delta_{X^3,LX} + \delta_{L^3,LX} + \delta_{L^4X,LX}$$

And finally, the correct expression of F (D4) shoud be :

$$F\ (D4) = \Phi_{mn,LX,LX} - \Phi^{*}_{pn,LX,X} \quad (10)$$

$$= LU + \tilde{\delta}_{LX,LX}L + \tilde{\delta}_{LX,LX}Emn - \tilde{\delta}_{LX,X}Epn = LU + \tilde{\delta}_{LX,LX}L + [\tilde{\delta}_{LX,LX} - \tilde{\delta}_{LX,X}]Emn + \tilde{\delta}_{LX,X}[Em - Ep]$$

I won't expand that equation : it is clear that the notations are becoming too heavy. So staying at the level of my equation 7 (same as the authors' equation 14d) may be the best compromise. I just wanted to insist on the question of non-linearity, and by the way show that the experiment by Brovkin et al. calculates a pretty far-fetched

land use flux...

No changes required (see our remarks on nonlinearities from above).

Definition D5:

$$\Phi_{mn,LX(t=today)} - \Phi^{*}_{pn,LX(t=today)} \quad (11)$$

$$F(D5) = = L_U + \tilde{\delta}_{LX(t=today)}L + \tilde{\delta}_{LX(t=today)}E_{mn} - \tilde{\delta}_{LX(t=today)}E_{pn}$$

Here, it is sufficient to explain that, because t is set for environmental conditions (it is not a transient simulation), all E-fluxes are equal to zero. Hence, no need to introduce the notation for the equilibrium of the pools.

We thank the reviewer for suggesting ways of simplifying our approach; however, we disagree at this instance: the notation "*" (or some other such notation) is needed to distinguish the approach by Shevliakova et al. from that of the inventory-based bookkeeping approaches: I+L occur on the basis of carbon stocks that are in equilibrium in the case of Shevliakova et al, but are in disequilibrium (because they are derived from observational data) in the case of the bookkeeping approach.

Definition B:

$$F(B) = \Phi_{m,LX(t=ref)} \quad (12)$$

$$= L_U + \tilde{\delta}_{LX(t=ref)}L$$

The previous remark still stands for the book-keeping definition. Note that there is no reference simulation, […]

We have added the word "reference" to the following sentence. This emphasizes that the reference simulation is only implicit.

> *Unlike in ESM and DGVM studies, the without-LULCC reference simulation is performed only implicitly: when an area is transformed from one vegetation type to another, the carbon loss or gain is determined by the difference between the two vegetation types' observation-based parameters.*

We have further clarified in method D5 that Shevliakova et al. did not perform the reference simulation explicitly either, but derived fluxes from within their with-LULCC equilibrium simulation:

> *This approch has been used by Shevliakova et al. (2009) (note that the without-*

LULCC reference simulation is only performed implicitly).

[...] and we only consider “managed” lands since natural lands are not represented in a book-keeping model.

We agree that carbon changes only occur when managed land is involved, but for clarity include the index for natural land as well in our equation to highlight the bookkeeping assumption that natural land does not respond to environmental changes.

And finally, there is a second book-keeping definition of LULCC emissions in the literature. We proposed it in GC13 (and use it in our model), in the case of a book-keeping model coupled with changing environmental conditions. We call it “book-keeping definition” in GC13, as definition B is the particular case of it when the LX conditions are not transient. It is based on making the difference, for managed lands, between the calculated flux and the one that would occur if the managed ecosystem’s carbon pools and fluxes were not disturbed by LULCC. It gives :

$$F(B2) = \Phi_{m,LX} - \Phi^{*}_{m,LX} \quad (13) = LU + \tilde{\delta}\,LXL + \tilde{\delta}\,LXEm - \tilde{\delta}\,LXEm = LU + \tilde{\delta}\,LXL$$

To be exhaustive, we could add the definition used in OSCAR v1, where the F-flux was supposed to be the net flux occuring over managed lands that had been disturbed up to e.g. 60 years before the year of estimation. That reference age of separation being dependent on the type of land (forest, corps, etc.) :

$$F\,(B3) = \Phi_{m(t<ref),LX} \quad (14) = LU_{(t<ref)} + \tilde{\delta}\,LXL_{(t<ref)} + \tilde{\delta}\,LXE_{m(t<ref)}$$

But maybe this definition should be ignored, as I know no currently used model using it. And it wouldn’t really help the reader to add a new definition with new notations related to the “ref” year.

We agree with the reviewer that this is too much detail for the scope of our study.

As stated in earlier responses, we have now clarified that different types of bookkeeping models exist.

4 Discussion

4.1 Land use feedback

I strongly believe including the so-called “land use feedback” in LULCC emissions with- out the non-LULCC feedback is unwise. First, because I don’t think the

environmental conditions related only to LULCC (i.e. δ_L) were calculated correctly in any of the cited studies. Second, because nobody would do that if the feedback flux wasn't CO_2 (e.g. CH_4 release from permafrost thawing induced by LULCC emissions) or the driver wasn't LULCC (e.g.CO_2 natural fluxes induced by warming induced by fossil fuel burning are not included in "emissions from fossil fuel burning"). Third, because I think it is a confusion between trying to quantify the *effects of LULCC* within the Earth system and just choosing an arbitrary definition for the *emissions from LULCC* (see my first remark in this review).

We acknowledge that the reviewer may have a strong opinion on which definitions are "wrong". As discussed before however, and endorsed by the reviewer Chris Jones, our paper does not intend to give recommendations on which definitions are right or wrong; it simply states which definitions have been used in published estimates.

(Note that Pongratz et al. (GBC, 2009) have quantified environmental changes due to only LULCC.)

Anyway, the technical difficulty related to non-linearity has to be mentioned in this section. None of the given definitions completely isolate that " land use feedback", especially because the non-linear term $\delta_L{}^2{}_X$ always remains somewhere in the expression. Besides, since this term is due to LULCC and non-LULCC, there is no way to decide whether it should be included into LULCC emissions or not.

We have added a comment on this:

> *The question becomes even more complicated considering practical limitations such as that the terms arising from a combination of LULCC-induced and other environmental changes can not be attributed to just one or the other forcing [...]*

4.2 LASC

We give estimates of the LASC in GC13: about 0.5 GtC/yr in 2005 and up to 5 GtC/yr in 2100 under the RCP8.5 if no more LULCC activities occur after 2005.

We have added the reference to Gasser and Ciais (2013) and rephrased the text accordingly (thank you for providing us during the revision process with the exact numbers!):

> *Strassmann et al. (2008, Tab. 3), Pongratz et al. (2009, Tab. 2), and Gasser and Ciais (2013, Fig. 2) agree in that the LASC and effects of environmental changes on instantaneous emissions and legacy flux historically amount to less than 10% (4-17 Gt carbon) as compared to the net LULCC flux with no feedbacks (Iu+Lu) (their respective D1 quantification).*

4.3 Legacy

—

No changes required.

4.4 Observable?

I think definition S is comparable to what is observable, given one knows the exact proportion of wood-products.

We have clarified our manuscript in that the complete definition of S is indeed observable, but that the flux actually reported from studies using single simulations is missing part of the observable components:

> *Method S as described in Eq. 15a could capture all observable fluxes over managed land; however, those flux components actually reported by the studies performing a single simulation account only for a part of the legacy flux.*

Maybe it should be reminded that (again) LULCC emissions are an arbitrary partitioning of the net land-to-atmosphere flux. Hence, one cannot expect them to be easily comparable to the observations, which raises all the issues about validation presented in this section.

We agree with the reviewer that all definitions of the net land use flux are arbitrary, but believe this is clear from the multitude of definitions discussed throughout the manuscript, so that for conciseness we prefer to not state this point again.

4.5 Indirect effects

That LU should be included in the LULCC flux is almost an agreement, isn't it? Only Lawrence et al. did not include all sub-components of LU , but it is a very uncommon choice.

We agree (and believe we have not stated anything contrary in our manuscript).

Note also that, even if it is not the role of scientists to choose what is the "best" definition, there can be very clear – scientific –arguments in favor or against certain definitions. In GC13, we tend to prefer the "book-keeping" definition since, with this definition, emissions from LULCC tend to zero if the LULCC pertubation is

stopped, which is not the case for all definitions! Wanting that no LULCC activities implies no emissions is not a too ethical choice, I believe (much more a logical choice).

As discussed before, we refrain from giving a judgment on “right” and “wrong” definitions. It may well be argued that the loss of additional sink capacity, which continues even if there is no further land use change, should be attributed to a country’s contribution to the rise in atmospheric CO_2 concentrations.

p.704 l.15 “separate” is misspelled.

Corrected.

Review by V. Arora

The authors tackle the difficult and important task of summarizing the range of definitions used in the literature for defining "land use change emissions". Clearly, this work must be published but after the concerns of all reviewers are addressed.

See below responses.

My first comment is that in my opinion, in context of this manuscript, the term used should be "land use CHANGE emissions" and not "land use emissions" as the authors have done. It is the effect of anthropogenic CHANGE in land use that we attempt to quantify through land use change (LUC) emissions.

The reviewer is right that most of the published estimates of the net land use flux only refer to emissions from land use change and land cover change, but some do include management that is land use, not land use change (e.g. constant wood harvest), and models are continuously evolving to include more greenhouse gas fluxes due to management activities. Our framework is valid for any such fluxes. However, to acknowledge the current state-of-the-art of the majority of models we have replaced throughout the text the term “net land use flux” by “net LULCC flux”.

The task of summarizing LUC emissions definitions is indeed difficult and this is reflected in the complexity of Section 2 of the manuscript. Despite authors' best intentions, this section comes out as something that is very difficult to grasp especially on the first reading. This section, in my opinion, therefore requires a fair

bit of work. Section 3 of the paper is much better written and relatively easier to follow.

Section 2 has been revised and clarified according to the reviewers' specific comments. We have also reorganized lines of thoughts, eliminated potentials for misunderstanding, and added clarifying statements as needed beyond the reviewers' explicit suggestions.

As a reader, I was also lost between definitions of managed (m), natural (n) and potential natural vegetation (p). p is defined as - the same areas as "managed land", but assuming hypothetically that the vegetation state is the potential undisturbed vegetation instead of actual managed. I wasn't able to follow this and the necessity for this distinction.

In response to all three reviews we have extended the explanation of "p":

> *A minimal set of land types needed to distinguish the different methods comprises managed land, including recovering areas ("m"), natural land unaffected by direct LULCC activity ("n"), and potential natural vegetation on managed land, which represents the same areas as managed land but assumes the vegetation were unaffected by LULCC ("p") (see Tab. 2 for detailed definitions). The potential natural vegetation is important for the "without-LULCC" simulation that is needed as reference to quantify the human impact by LULCC as compared to a world without LULCC interference, and thus assumes the hypothetical land cover unaffected by man ,"p", in lieu of "m".*

Similarly, the terms u, f, l, labmda and gamma are not straight forwardly understood. I believe there is a much simpler way for expressing the nine definitions of LUC emissions and I suggest one way of doing this further down this review.

See response below.

The manuscript also lacks the basic carbon budget equations that should form the foundation of any attempt for seeking clarity for multiple ways in which LUC emissions are defined and calculated. For example, on page 679, line 10 the terms "net biosphere flux" and "net land use change flux" are defined but in absence of any equations it is not clear what they mean.

We have added the basic carbon budget equations as relevant for the later discussions to the introduction (Eq. 1).

I attempt to express the simple cases and without worrying about the distinction between m, n and p lands.

Response further below.

The vertically integrated global carbon budget can be expressed as

$$\frac{dH_A}{dt} = F_O - F_L + E_F = -(F_O + F_l) + (E_F + E_L)$$
$$\frac{dH_L}{dt} = F_L = F_l - E_L; \quad \frac{dH_l}{dt} = F_l$$
$$\frac{dH_O}{dt} = F_O$$
$$\frac{dH}{dt} = E_F$$

H = H_A + H_L + H_0 is the sum of carbon in the atmosphere, land and ocean components (Pg C), F_L and F_0 and are the global atmosphere-land and atmosphere-ocean CO2 fluxes (PgC yr-1), F_L is the natural global atmosphere-land CO2 flux in absence of LUC (also referred to as the residual land sink), and E_F and E_L are the rates of global anthropogenic fossil fuel and LUC CO2 emissions (Pg C yr-1) into the atmosphere.

No changes required.

The net global atmosphere-land CO2 flux is

$$\begin{aligned} F_L = \frac{dH_L}{dt} &= \frac{dH_V}{dt} + \frac{dH_S}{dt} \\ &= (G - R_A) - R_H - E_L \\ &= N - R_H - E_L \\ &= F_l - E_L \end{aligned}$$

where $H_L = H_V + H_S$ is the global land carbon (Pg C) which is made up of live vegetation biomass (*HV*) and dead carbon in soil and litter pools (*HS*). *N* is the terrestrial net primary productivity obtained as the difference between gross primary productivity (GPP) (*G*) and autotrophic respiration (*RA*), and *RH* is the heterotrophic respiration. These equations will also help understand the reader the imbalance between photosynthesis and respiration.

See response above.

In practice, E_L is not straight-forwardly calculated and, at least, two simulations are required that calculate by differencing atmosphere-land CO2 flux from simulations with and without LUC.

We agree, and this is what our manuscript states.

I would define from these two simulations as follows ...

FL(CO2, X) from simulation in which CO2 and X (climate) change. X thus includes changes in temperature, precipitation etc.

FL(CO2, X, LUC) from simulation in addition to CO2 and X (climate) changes, changes in land cover are also prescribed.

So LUC emissions can be defined as ...

EL = FL(CO2, X, LUC) – FL(CO2, X)

... and the terms in brackets make it clear what the forcings are that are driving FL. And, as the authors clarify in Section 3, researchers have used studies in which CO2 is fixed or time varying, X is fixed as in offline simulations and time varying as in a climate model.

We agree with the reviewer that this is one possible way to quantify the net LULCC flux; the previous equation corresponds to our Eq. 13b (12b in the original version)..

Of course, this is easier said than done and this is one way to making things clear. I haven't been terribly thorough in the equations above. Authors need to take into account comments from all reviewers in the end to come up with an easy to follow set of equations.

We believe our accounting for all reviewers' comments has greatly improved the clarity of our manuscript. Furthermore, throughout the manuscript, we have rephrased sentences or made explanations more explicit to increase clarity.

The paper should also mention upfront the two ways in which land use change affects the climate (biophysical and biogeochemical). This is done, for example, on page 683 without the use of the biophysical and biogeochemical terms which are used later in the manuscript (e.g. line 11 on page 685).

Actually, biogeochemical and biogeophysical effects are first discussed much earlier than the reviewer suggests: the discussion of ESMs in the introduction contains these terms three pages earlier.

To further highlight the importance of these two groups of effects we have made the reference to them in Sec. 2.3 more explicit:

> *LULCC-induced environmental changes include both biogeophysical effects, such as changes in albedo, and biogeochemical effects, such as changes in atmospheric CO2.*

Page 686, line 11. I am unclear what does "environmental changes may be simulated in a realistic transient way" means since climate models also have biases in simulated climate.

We had used "realistic" to clarify that in reality, environmental conditions are changing transiently (as opposed to modeling methods that assume constant environmental conditions). We have eliminated the term "realistic" in the revised version.

Please change Arora et al. (2010) reference to Arora and Boer (2010) in the text.

Corrected.

Finally, the manuscript doesn't actually show the uncertainty in LUC emissions, as the title of the paper suggests, when they might be calculated in nine different ways. May be a title of the manuscript along the lines "On the multiple ways of calculating carbon emissions from anthropogenic changes in land use" would be more appropriate so that it is clear that the manuscript talks about the multiple ways but doesn't actually quantify the uncertainty.

We thank the reviewer for this constructive comment. The use of the term "uncertainty" in our title is indeed meant in two ways: (1) the uncertainty of what is meant by "net LULCC flux", i.e. which component fluxes are included in a specific study and (2) the quantitative uncertainty arising from (1). Both are addressed in our study (the Results section, in particular Table 3, gives first-order estimates of the latter) and we therefore prefer to keep the title of our study as it is.