Blue text, reviewers' comment.
Black text, authors' response.
Double bar and grey text, quote from previous version.
Double bar and black text, quote of proposed changes.

Review 1

1.1. (General)

This paper is an excellent blend of mathematical precision and narrative clarity. It is not light reading, but it contains a number of explicit cautions and a few "ah-haaahs" for those who persevere.

We thank the reviewer for his support.

1.2. (Specific)

Page 200, line 29: I don't see why the authors say def. 2 "is even superior to" def. 3 in some instances. What criterion are they using?

By "superior", we meant that the value of $ELUC_{def2,\tau lim}$ was greater than the one of $ELUC_{def3}$ for certain values of τ_{lim}. To make it clearer, the text is changed accordingly:

> [Previous text]
> In 2005, $ELUC_{def2,\tau lim}$ is even superior to $ELUC_{def3}$ for small values of τ_{lim} (<20 yr).

> [Proposed changes]
> In 2005, the value of $ELUC_{def2,\tau lim}$ is even greater than that of $ELUC_{def3}$ for small values of τ_{lim} (< 20 yr).

1.3.

Page 188, line 19: "than" should be "as".
Page 191, line 16: "keep" should be "keeps".
Page 196, line 20: "others" should be "other".
Page 199, line 19: "less" should be "least".
Page 201, line 21: "implies to run" should be "would require running".
Page 203, line 6: delete "up".
Page 203, line 8: replace "results which definition" with "results in which the definition".

All done.

Review 2

2.1. (General)

This study creates a mathematical framework and uses a simple carbon cycle model to illustrate the relevance of several different components that can be included or excluded in land use emission estimates. These components reflect effects of changes in vegetation cover due to conversion of biomes, effects of changes in climate and other external conditions, and both. The study is not intended to be a comprehensive analysis of all possible ways how to compose a land use flux, but instead has its strength in a clear focus on the difficulty in choosing a definition with respect to land use vs climate perturbation. With this, the study highlights some important sources of uncertainty in emission estimates and will certainly be a valuable contribution to the literature.

The manuscript is complex to read and I suggest a number of clarifications throughout the manuscript as outlined below. In particular, assumptions – and which cases they refer to and which not – need to be made explicit. Further, the reader is pointed to additional effects not included in the mathematical framework several times with the note that they easily could be. Since these include important influences on the global carbon balance – e.g. management effects such as wood harvest or natural dynamics in geographic vegetation distribution – I recommend expanding the framework accordingly.

We thank the reviewer for the careful review of our ms, and especially for having gone through the appendices.

Concerning the main concern raised by the reviewer about the complexity and clarity of the paper, we want to point out that there is a difficult balance to reach between the rigor and accuracy required to develop a theoretical framework, and the clarity and readability needed in a scientific paper. This balance is the reason why we chose to discuss some cases apart from the main framework.

However, to address the reviewer comment, several inconsistencies have been dealt with (see specific answers below), and we offer the revisions explained below.

2.2. (Specific)

p. 182, l. 20: "The aim is to clarify the definitions of ELUC so that the scientific community can dispose of estimates whose differences do not reflect the choice of different definitions." This sentence is unclear. There is the choice of definition, and the choice of everything else, such as type of model. Why should we want to dispose of estimates from different models?

It is a fact that there are different definitions of ELUC used across the literature, but that not all the community is aware of the fundamental implications that these definitions have on simulated fluxes. Here, we show that a part of the discrepancy between simulated fluxes (like in [Houghton et al., 2012]) is only due to this choice of definition. Hence, if you expect to understand why your own model performs as it does thanks to an intercomparison in which all other participants use a different definition than yours, you may draw the wrong conclusion.

We propose to rephrase:

The aim is to clarify the definitions of ELUC encountered in the literature, and to present a framework for designing model simulations so that one can compare simulated estimates of ELUC from different studies without the bias due to the choice of different and incompatible definitions.

2.3.

p. 183, l. 14 f: Why does it matter if the effect of non-CCN perturbations is small? Conceptually, they could be treated just like CCN.

The reviewer is right. This sentence aimed at explaining the name "CCN" (and not "CCNOPS", for instance). We made the following correction:

[Previous text]
We call this indirect perturbation of the carbon balance of the terrestrial biosphere the “CCN” perturbation, for “Carbon, Climate and Nitrogen”. There are other processes affecting the terrestrial carbon cycle such as the effect of elevated O_3, altered P cycling, SO4 aerosols deposition on wetland plants, but their effects are assumed to be smaller in magnitude.

[Proposed changes]
We call this indirect perturbation of the carbon balance of the terrestrial biosphere the “CCN” perturbation, for “Carbon, Climate and Nitrogen”, noting that it conceptually includes other perturbed processes affecting the terrestrial carbon cycle such as the effect of elevated O_3, altered P cycling or SO_4 aerosols deposition on wetland plants.

2.4.

p. 183, l. 27: Add “System” in “Earth Models”

Done.

2.5.

p. 185, l. 1: “under some minor hypothesis” – which one?

Under the hypothesis discussed in Section 4. To avoid confusing the reader, we propose to change the text:

These two effects will be discussed in Sect. 4. They can be incorporated in our definition framework under some minor hypothesis, but would unnecessarily complicate the notations in the following.

Sect. 4 includes a discussion on how these two effects can be incorporated in our definition framework, but doing so in the following demonstrations would unnecessarily complicate the notations.

2.6.
Eq. 6 & 8: The last “tau” each should be “t”.

We respectfully disagree with the reviewer. With these two equations, we simply illustrate the general vectorial notation we introduce to describe the value of the variable X along the τ-axis, at a given time t. With such a general notation, defining an age class τ older than t is possible. However, we do acknowledge that in our experiments, the values of X^{τ} for tau being greater than t are all equal to zero, because no transition occurred before that date.

For understanding and illustration of our notation, we propose to add one sentence after equation (6):

We note that here all $\delta S^{+,\tau}$ for $\tau>t$ are equal to zero, as no transition occurred before that date.

2.7.
Flipping back and forth through the equations would be easier if a consistent nomenclature were used; “*” should be used right from the beginning.

Agreed. Consistent changes made in Section 2.1.

2.8.
Fig. 2 is referenced before Fig. 1

Reference to figure 1 added in the last paragraph of introduction.

2.9.
p. 187 “Note that there is not reason: : :” Up to p. 185 transition refers to a change in area and is thus an instantaneous event. On p. 186 transition is defined as the entire time period until the system has reached a new equilibrium with respect to CO2 fluxes.
The different time perspective makes the use of the word ‘transition’ confusing. Further, the definition of transition as pre-equilibrium period makes the statement “Note that: : :” obsolete.

To avoid that kind of confusion: (i) we keep on using 'transition' for the whole period before the ecosystem reaches a new equilibrium; (ii) for all other occurrences of the word 'transition' (e.g. in sect. 2.2. or conclusion) we indistinctly use: [area] change or [land] conversion.

We do not fully understand the reviewer's remark concerning the statement "Note that there is no reason for any f^{τ} to equal f^{*} before the termination of the transition". Here, we insist on the fact that the transitioning ecosystem will meet its equilibrium state only at the end of the transition, but at no time before that. It may be obsolete, but we believe it is part of the pedagogy to recall the reader that if an element of the vector $\boldsymbol{f}$ is equal to f^* before the end of the transition, it is only by coincidence.

2.10.

The authors should go through the manuscript again and carefully identify which of the many assumptions made are valid only within the respective section and which are universally valid. For example, it merits explanation that f_0 is 0 only for undisturbed areas. P. 184 (“f_0 are equal to zero”) is not explicit about this, which leads the reader to wonder if the respective terms in Eq. 10-13 should not be 0 as well.

Contrarily to the reviewer's statement, there are not "many assumptions" made in this ms, and no assumptions valid only for one specific section. We make only one strong assumption: the preindustrial (dynamic) equilibrium of the carbon cycle, which leads to the result that f_0^* is equal to zero for all (g,b). It is then discussed in Section 4.

The misunderstanding in the example given by the reviewer may come from the fact that we did not use the superscript * in section 2.1. (corrected from remark 2.7.). It is also linked to remark 2.9. Again, there is no reason that any element of $\boldsymbol{f_0}$ is equal to f_0^* before the end of the transition. Assuming that there is one reason would consist in making an assumption (which we do not make!).

2.11.

p. 189, l. 16: “than” should be “as”

Corrected.

2.12.

I recommend consistent usage of variables throughout the manuscript; e.g., “t” is usually used as time since preindustrial and “Delta” as the disturbance from preindustrial times until t (p. 184). In A2 “Delta” refers to the whole transition period, i.e. up to the equilibrium with respect to the LUC perturbation – but t is not necessarily equivalent to the time when the equilibrium is reached.

This has been double-checked. We use t' in the Appendix when necessary. We also use τ_d instead of τ in the expression of *hr* as a function of *npp* in Sect. 2.1.

2.13.

All appendices should be referenced at the respective equations or discussion in the main text.

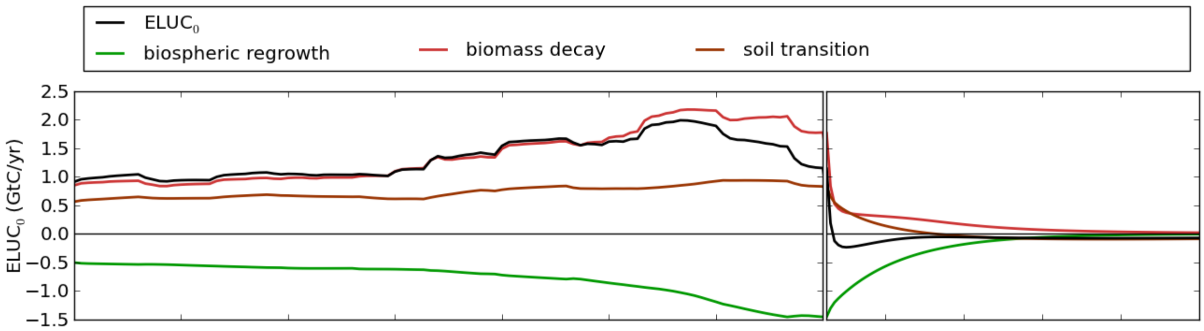
They now are. However, appendices are not essential to the main conclusion of the paper: they are one step further in the development of the framework (and they can help in understanding some specific behaviors of the four component fluxes), as stated in the abstract.

2.14.

p. 191, l. 15: Why is the legacy flux of ELUC0 beyond 2005 negative (no further changes in vegetation distribution allowed)? The text later says this is "driven by soil C accumulation due to biospheric regrowth", but I would expect legacy emissions from forest clearing, including product pool decay, would still yield substantial emissions as compared to C uptake in recovering soils, which should be limited to the currently abandoned N hemisphere agricultural lands. Is this result mostly an issue of the specific turnover rates chosen in this model?

Thanks to the reviewer, we investigated further this behavior and realized that the negative legacy of $ELUC_0$ is mainly due to the regrowth of biomass (and not only soil carbon) which is induced by the large agricultural and pastoral abandonment in the 1990s. However, we have no knowledge of recent study that tried to assess the committed land-use flux, so that we would be able to compare the response of our model to estimates from independent study...

See following figure (from a previous version of the paper) for illustration:



Despite being clearly not the main point of this paper, we propose a more accurate explanation in the main text. First, we add the following sentence in the first paragraph of Sect. 2.4.2:

> The legacy of $ELUC_0$ is negative few years after 2005 and it (slowly) tends toward zero when all transitioned ecosystems have recovered. In our model, the negative sign of these "committed emissions" is due to biomass regrowth [see also Houghton, 2010] induced by significant agricultural and pastoral abandonment in the 1990s (i.e. conversions from croplands or pastures to forests).

Then, in the following paragraph about ΔELUC:

> Contrarily to $ELUC_0$, the legacy of ΔELUC after 2005 is positive. This result may be model-dependent, and it is explained in OSCAR v2 by the fact that the change in living biomass carbon density is faster than the change in soil carbon density (i.e. $\Delta c^*/c_0^*$ is greater in vegetation than in soils). Thus, the legacy of ΔELUC is driven by the increased emissions due

to dead biomass after deforestation, while the legacy of $ELUC_0$ is driven by soil carbon accumulation due to biospheric regrowth (see also Houghton, 2010).

Contrarily to $ELUC_0$, the short-term legacy of ΔELUC becomes positive after 2005. This result may be model-dependent, and it is explained in OSCAR v2 by two effects. First, the change in biomass carbon density is faster than the change in soil carbon density (i.e. $\Delta c^*/c_0^*$ is greater in biomass than in soils), which implies that the relative role of dead biomass in the committed emissions is greater in ΔELUC than in $ELUC_0$. Second, global warming induces an increase in heterotrophic respiration rate, which in turn leads to a "faster" carbon soil emissions than it would have been under preindustrial CCN conditions (see Sect. A2 for detailed equations).

2.15.

p. 192, l. 6 ff: Mention that the strength of CO2-fertilization is particularly uncertain across models. Is OSCAR on the high or the low end of sensitivity?

To highlight that point, we propose to add a sentence at the end of the last paragraph of section 2.4.:

The causes of the stabilization after 2080 are exactly the same as for $LSNK_0$, and are ultimately dependent on the model's sensitivity to CO_2, climate change, and other environmental changes (the default setup of OSCAR v2 having a relatively high sensitivity to CO_2 increase [Gasser et al., 2013].)

2.16.

The 20% uncertainty due to difference in definitions of land use emissions are placed too prominently without further discussion of how well the OSCAR model performs in simulating the global carbon balance. It may be that there is some model evaluation in the referenced literature, but such uncertainties need to be stated in this ms.

The "20%" value only appears three times in the ms. Each time, it is clearly stated that it is a result related to our single simulation with OSCAR. Here, we propose to add another precision in Section 3.4.2:

[...] can be up to about 20% during the 1980s and the 1990s. Despite being clearly model-dependent, this result highlights the importance of the choice of the definition to quantify land-use-related emissions and compare different model estimates.

As for the model's performance, we also add the following statement at the end of the introduction:

For the purpose of illustration, however, we give numerical applications so as to roughly quantify definition-related differences in ELUC, using the OSCAR v2 carbon-climate model (Gasser et al., 2013, see also Appendix B). This model has previously been shown to perform satisfactorily in reproducing recent estimates and trends in the global carbon budget, as simulated fluxes for global land and ocean are within uncertainty ranges calculated by Denman et al. [2007] and Le Quéré et al. [2009].

This statement is illustrated with the following figure and table, extracted from the [Gasser et al., 2013] reference of this paper.

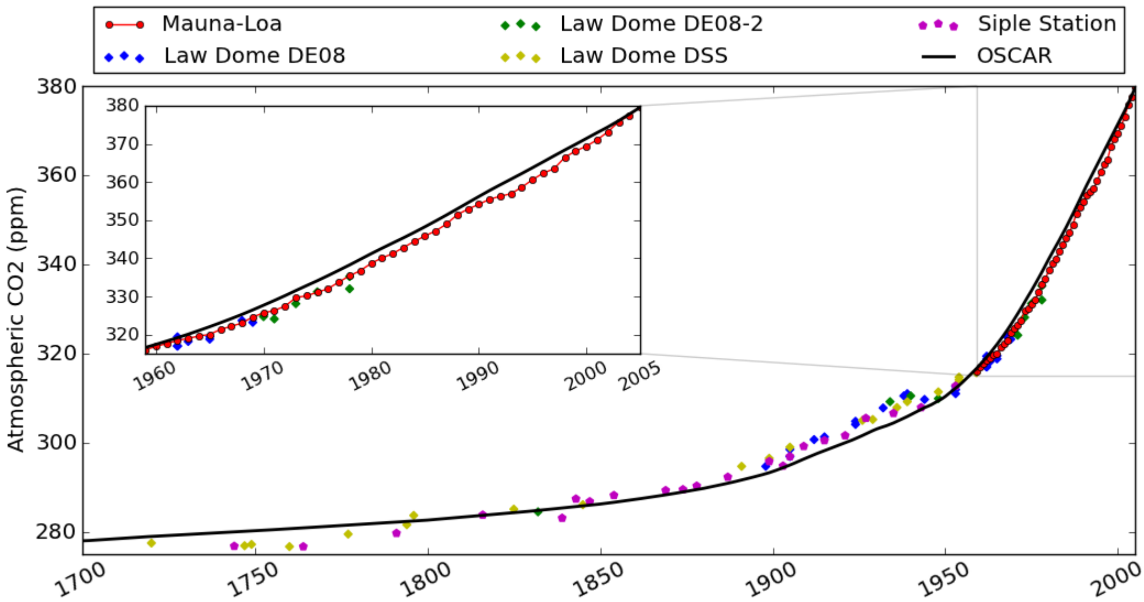


Table 1. Comparison of OSCAR's simulation of the global carbon budget

Flux[a]	1980s: Denman et al. [2007]	1980s: OSCAR	1990s: Denman et al. [2007]	1990s: Le Quéré et al. [2009]	1990s: OSCAR
Atmospheric increase	3.3 ± 0.1	3.2	3.2 ± 0.1	3.1 ± 0.1	3.2
Fossil fuel emissions	5.4 ± 0.3	5.5	6.4 ± 0.4	6.4 ± 0.4	6.4
Ocean sink	-1.8 ± 0.8	-2.1	-2.2 ± 0.4	-2.2 ± 0.4	-2.4
Net land-to-air flux:	-0.3 ± 0.9	-0.2	-1.0 ± 0.6	-1.1 ± 0.6	-0.7
= Land-use change flux	$1.4\,[-1.0\ +0.9]$	1.9	$1.6\,[-1.1\ +0.9]$	1.5 ± 0.7	1.7
+ "Residual" land sink	$-1.7\,[-1.7\ +1.9]$	-2.1	$-2.6\,[-1.7\ +1.7]$	-2.6 ± 0.7	-2.4

2.17.

The authors mention the issue of land cover change vs other land use activities such as wood harvest on forest in Section 2.2 but limit their mathematical framework to land cover change. Since the OSCAR model includes not only land cover change, but also wood harvest (as explained in Appendix B) and more and more biosphere models are moving in this direction it would be good to include both effects in the framework. I don't believe the equations would change very much but it would be reassuring for the reader to see this, and it will increase the universality of the concept.

Land-use activities are in fact included in the framework (in the illustration with OSCAR, only forestry is included). But it is not clearly explained in our ms. Thus, we want to change a part of the first paragraph of section 2.2.:

In the following, we consider only "land-use change" activities, but the first type of activities can be represented as well by a partial transition from one biome to itself (i.e. δS from (g,b_1) to (g,b_1)). Land-use change activities induce a local perturbation of the terrestrial carbon cycle that leads to a net emission or absorption of CO_2 over time.

In the following, land-use activities going with land-cover change, as well as activities going without land-cover change, are accounted for; the latter being represented by a land conversion from one biome to itself (i.e. a conversion δS from (g,b_1) to (g,b_1)). Thence, all land-use activities (be it with or without land-cover change) induce a local perturbation of the terrestrial carbon cycle that leads to a net emission or absorption of CO_2 over time.

We also want to add this clarification at the very end of Section 2.4.1, when explaining what is the ΔLSNK flux:

This last term was called "amplification effect" by Gitz and Ciais (2003) and "loss of sink capacity" by Pongratz et al. (2009); and it is equal to zero for land-use activities that are not associated with land-cover change (see Sect. A4).

2.18.

The manuscript could then also keep speaking of "land use change", while else I would strongly suggest to use "land cover change" for land-use driven changes in vegetation distribution throughout the ms, consistent with Turner in Lambin&Geist ("Land-use and land-cover change", 2006) or Houghton et al, 2012.

Thus, we keep using "land-use change".

2.19.

I believe the present approach and the one by Shevliakova et al. differ in what is counted towards "disturbed land". In the present approach, land that has reached a new equilibrium with respect to the land use disturbance moves from the disturbed to the undisturbed category, while Shevliakova et al, and p. 200, l. 17, treat once disturbed land forever as disturbed.

Both approaches are present in this ms. The first, where the land is then moved to the undisturbed category, is the one we call the truncated definition 2. The second (also used by Shevliakova et al.) is the general definition 2 (that is equivalent to the truncated definition if τ_{lim} is taken infinitely huge).

2.20.

p. 198, l. 7: "others" should be "other"

Changed.

2.21.

The analysis of truncated definitions is an important one, because more sophisticated models like DGVMs are usually not investigating this issue in depth due to computational limitations. The discussion on Fig. 4 (and respective Conclusion Section) should therefore be extended. It is clear that the results are model-dependent, but it would be helpful to know why exactly. What are the key model-dependent parameters and assumptions that influence the truncated results?

We do agree with the reviewer that this issue is important for DGVMs. However, having a complete analysis of the sensitivity of the truncated definition would require a large amount of work, that would be best exploited in future work, and compared with outputs from some DGVMs, yet with no insurance of having less model-dependent results.

We believe this is equivalent to doing and then writing a whole new paper, on the basis of a single issue raised in this one. This is not in our short-term plans, but we'd be glad to discuss such things with the reviewer and his/her team.

2.22.

Section 4, the framework's shortcoming concerning migration of biomes: The idea of using a "mean biome" state needs some more clarifications. Please add equations.

Details added:

> To do so, at each time step and over each grid cell, all natural biomes *b* have to be aggregated into one "mean" biome before applying our framework. In that way, the migration of natural biomes finally appears in the net areal flux Δf^* (the CCN perturbation) but not in the area changes δS (the LUC perturbation) that is limited to transitions from natural to anthropogenic biomes (and conversely) and to transitions between anthropogenic biomes.

> To do so, at each time step and over each grid cell, all natural biomes *b* would have to be aggregated into one "mean" biome before applying our framework. For example, in a vegetation model in which a limited number of PFTs are defined, part of them being natural vegetation (*nat* sub-ensemble hereafter) and the other part human-appropriated ("anthropogenic" biomes), one can define the mean natural PFT, over each gridcell *g*, as $\tilde{b} = \sum_{b \in nat} b$. Consequently, the mean areal flux over $(g, \tilde{b})$ is:
>
> $$f(g, \tilde{b}) = \frac{\sum_{b \in nat} f(g,b) S(g,b)}{\sum_{b \in nat} S(g,b)}$$
>
> When this aggregation is used with all equations of Sect. 2, the migration of natural biomes, that can be seen as a land conversion from one natural biome to another, could be accounted for in the net areal flux $f(g, \tilde{b})$ (i.e. in the CCN perturbation). The effect of direct anthropogenic land-use change, that is limited to conversions from natural to anthropogenic

biomes (and conversely) and to conversions between anthropogenic biomes, would still appear in the area change δS (i.e. in the LUC perturbation).

2.23.
p. 203: “three uncertainties related to data” – I would be more general here, as there are other data-related uncertainties than the mentioned three, e.g. the classification of PFTs, while carbon areal densities is not a data uncertainty for DGVMs that simulate this variable internally. “Variability linked to the structure of the models” should be “differences between models due to structural differences”

The initial "three uncertainties" were a direct reference to [Houghton, 2010]. But it is not essential to the conclusion, hence we propose to change as follows:

> Since this adds up to the three uncertainties related to data (on area transitions, carbon areal densities and emission dynamics) and to the variability linked to the structure of the models, we highly recommend [...]

> Since this adds to all other kinds of uncertainty (related to data: on area transitions, carbon areal densities, emission dynamics; or to the structural differences between models), we highly recommend [...]

2.24.
p. 204, l. 6 should read “the total CO2 flux integrated over time”.
p. 205, l. 9: it is unclear what “change” refers to – add “due to CCN-perturbations”.

Both corrected.

Comment 3

3.1. (General)
The formalism in the present paper (thereafter referred to as GC13) is presented in a simplied and understandable form. However, in one aspect (as I try to demonstrate below) the simplication ignores what (fossil fuel emissions vs. land use change) causes what part of the land-atmosphere C flux. This inconsistency leads to an over-estimation of the land use change (LUC) C flux when assessed from the CCN+LUC and the CCN-only experiments (\CCN" used in the meaning of GC13). A more rigorous separation of different fluxes can solve this problem and can also serve to accomodate other flux components within the formalism introduced by GC13 that have been defined in previous publications (LUC fertilisation feedback flux, replaced sinks/sources). In general, a strengthening of such references to and comparisons with previously published Strassmann et al. (2008); Pongratz et al. (2009); Stocker et al. (2011) flux component definitions could - in my view - even further improve this important paper. In particular, the paper by Strassmann et al. (2008) already provides a formalistic framework to quantify LUC flux components.

+ following demonstrations

We thank the reviewer for his comment and for having raised this very important issue. Let us rephrase the comment as follows.

In fact, the CCN perturbation is partly caused by the LUC perturbation itself, hence the perturbed areal flux Δf can be expressed as: $\Delta f = \Delta f^{LUC+FF} = \Delta f^{LUC} + \Delta f^{FF}$ (FF referring to Fossil Fuels). First, we note that a better way to write this would not be to separate the CCN perturbation between LUC and FF, but between LUC and no-LUC effects (since not only FF but also CH_4 and aerosols, for instance, are part of the CCN perturbation). Thus, on the basis that we can write $\Delta f = \Delta f^{LUC} + \Delta f^{noLUC}$, and following Strassmann et al. [2008], the reviewer further breakdown our two fluxes $LSNK_0$ and $\Delta LSNK$ into (i) a land-use feedback flux, equal to $LSNK_0^{LUC}+ \Delta LSNK^{LUC}$; a "real"/"effective" amplification effect (that he calls "replaced sources/sinks") equal to $\Delta LSNK^{noLUC}$; and the "real" potential sink equal to $LSNK_0^{noLUC}$. In his approach, the ΔELUC flux stays in the "interaction term".

First part of our response. There is a fundamental caveat with this approach: the assumption that $\Delta f^{LUC+FF} = \Delta f^{LUC} + \Delta f^{FF}$ is incorrect, because of the non-linearity of the system. To account for the non-linearity, one could write that: $\Delta f^{LUC+FF} = \Delta f^{LUC} + \Delta f^{FF} + \Delta f^{non\text{-}lin}$ (this last term being the non-linearity). Then, with the same breakdown as before, one could simply put all the non-linear terms in the "interaction term". Our first point, here, is that not accounting for the non-linearity is, in the context of establishing a theoretical framework, a source of biased estimate, which we want to avoid in this study.

Second part. We believe that there is a fundamental difference between what the reviewer suggests to do (i.e. to assess the <u>effects</u> of land-use change) and what we do in this paper (propose clear definitions of the <u>emissions</u> from land-use change). Our paper has been written so as to show the importance of the definition of "ELUC" in the context of studying the global carbon budget (see our introduction and equation 1). In that context, we just have to find a proper way to separate the 'residual' land sink and the emissions from land-use change. Trying to understand what are the effects of the land-use activities is a wider (and much more complex) issue. For instance, in this paper we propose a better way to compare the results of uncoupled simulations made with DGVMs. But with the distinction introduced by the reviewer, it would be necessary to make several other simulations with DGVMs coupled to their respective ESM. Hence, the variability of results across models will not only depend on the land carbon cycle model and its land-use change module, but also on all the other parts of the ESM (the atmosphere / ocean models, but also the biophysical properties of the land model).

In fact, assessing the effect of land-use change on CO2 increase and/or climate change, i.e. following and quantifying the cause-effect chain, is a work that is related to regional attribution of climate change (also known as the Brazilian Proposal; see for instance work by the MATCH group), which we also do with the OSCAR model (in the [Gasser et al., 2013] reference).

Consequently, we do not believe this breakdown should be adressed in our paper, because it is out of its scope. However, we do agree that the work by Strassmann et al. [2008] must be cited in the paper, and that the issue should at least be mentioned, which we propose to do by adding the following paragraph in the discussion section.

> Finally, the work by *Strassmann et al.* [2008] must be mentioned, as they also separated different components of the land-to-atmosphere CO_2 flux, but in a fundamental different manner as we did here. The starting point of their analysis was that part of the CCN perturbation is caused by the LUC perturbation itself, as stated in Sect. 2.2. Hence, if we stop looking at the CCN perturbation as an exogenous perturbation (a forcing) and start seeing it as being endogenous, the disturbed areal fluxes Δf could be written as $\Delta f = \Delta f^{LUC} + \Delta f^{noLUC} + \Delta f^{non\text{-}lin}$. The superscript "LUC" refers to the part of the CCN perturbation that is attributable to the LUC perturbation, the superscript "noLUC" to the part induced by everything else (e.g. fossil fuels, methane, aerosols), and the superscript "non-lin" accounts for the non-linearity of the system, which was forgotten by *Strassmann et al.* [2008]. Going through the same demonstrations as in Sect. 2, but with Δf broken-down as above, leads to the breakdown of $LSNK_0$ and ΔLSNK (and ΔELUC) into three sub-components. Thence, we could regroup this components and identify the different terms defined by *Strassmann et al.* [2008] and *Stocker et al.* [2011]: $LSNK_0^{LUC} + \Delta LSNK^{LUC}$ is their "land-use feedback", $\Delta LSNK^{noLUC}$ is their "replaced source/sink", and $LSNK_0^{noLUC}$ is their "(effective) potential sink". $ELUC_0$ corresponds to what they call "the book-keeping flux", and all the other terms (i.e. $LSNK_0^{non\text{-}lin} + \Delta LSNK^{non\text{-}lin} + \Delta ELUC$) correspond to their "interaction term". However, the initial breakdown of Δf into three sub-fluxes raises the issue of (i) what is the system boundary (forced or coupled land carbon cycle)? (ii) what is the cause-effect chain within this boundary? and (iii) how should the non-linearity be dealt with? The three questions are beyond the scope of our paper, but have been partly addressed in studies about the "regional attribution of climate change" (also known as the "Brazilian Proposal", see e.g. [Gasser et al., 2013]).

3.2. (Specific)

I was puzzeled by whether δS^+ and δS^- were equal. Obviously they are not, because fluxes are presented for each *g* (geographic location) and *b* (biological unit/PFT). I personally would prefer to see fluxes presented for the integral (sum) over all *b*s (see GC13, Eq. 5), but differentiated between two types of land: natural and agricultural. I find such a distiction helpful because it makes evident that fluxes (Δf^{FF+LUC}) are different on natural and agricultural land, and that this difference defines the *replaced sources/sinks* flux (F_{RSS}).

About δS^+ and δS^-, in Sect. 2.2. we want to add that:

> where ΔS^- is the cumulative destroyed area of primary ecosystems, and ΔS^+ is the cumulative created area of secondary ecosystems, of *b* over *g* since preindustrial. Both quantities are positive but not necessarily equal, since they do not come from the same land conversion (the first is due to a conversion from (g,b) to (g,b_n) while the second is due to a conversion from (g,b_m) to (g,b); hence, they are equal only for land-use activities that do not induce land-cover change).

As for the question of presenting fluxes integrated over *b*s, doing so requires to write more complex equations in section 2 by immediately introducing b_1 and b_2 as the initial and final biomes of the conversion, while it is not necessary in order to develop our framework (it is however done in appendix). Our approach is more fundamental, making no assumption about the different *b*s (anthropogenic or natural), except when discussing the migration of biomes (in Sect. 4).

Despite being a bit counter-intuitive in the first place, this notation brings simple results for the four component fluxes, as shown in equation (16), that can easily be understood. Conversely, the use of b_1 and b_2, in appendix A5, make the comparison between $ELUC_0$ and $\Delta ELUC$ (or between $LSNK_0$ and $\Delta LSNK$) less immediate than in equation (16).

Hence, we believe it is the right notation for thinking at the areal level, while the one proposed by the reviewer should be used when thinking at global/integrated scale (like in appendix A).

3.3.

An integral over *b* would imply that *b* is a continuous dimension, but it's not. Distinguishing between natural and agricultural lands, or by PFTs, or biomes, always leads to discretisations. Thus, presenting sums instead of integrals would make more sense. But, as mentioned above, I would suggest to reduce *b* to natural and agricultural land and always present the sum explicitly written out to make the difference in associated fluxes (Δf^{FF+LUC}) evident.

This comment is linked to the previous one (**3.2.**). It is true that, as of now, all models do include biological units as discrete units. However, we did not feel that it was necessary to do so in our framework: the integrals are still mathematically defined with continuous *b*s (since they are always bounded by the total area of the Earth / the total amount of carbon on Earth), and it does not hinder the understanding of the framework.

Here, as in comment **3.2.**, we simply wanted to create the most general framework we could, without further consideration about the structure of the models.

3.4.

page 191, line 9: "... without any LUC perturbation after the year 2005": I assume it means without any further change (land distribution is "frozen") rather than returning immediately to zero land use areas? However, then I don't understand why $ELUC_0$ is negative after 2005 AD. Can you clarify?

Clarification added:

> [...] without any LUC perturbation after the year 2005 (i.e. no new land conversion nor biomass harvest).

The negative legacy comes from the strong biomass regrowth due to land abandonment in the 1990s (see response to comment **2.14.**).