Response to the Editor's Comments

We appreciate the Editor's scrutiny and comments on our second submission. Below, we address each comment separately.

> As editor I thank Stocker and Joos for carefully adressing the issues raised by the reviewers. I feel the paper is now much more concise. Nevertheless, when reading the re-submitted paper I still stumbled about the few points below that I would like to ask the authors to address before publication.
>
> - In section 2.1 you intend to revisit estimates of *e*LUC according to method D1 in Pongratz et al. 2014. But what you describe includes also method B of Pongratz et al. 2014, covering in particular the emission estimates of Houghton. I consider it completely legitimate to extend or modify meanings, but please make such changes explicit.

We re-formulated this to clarify the difference between the method of bookkeeping *models* (B in PG14) and what is termed the *bookkeeping method* in Strassmann et al. (2008) and in the present manuscript.

While bookkeeping models are designed to derive LUC-related C emissions from a single simulation (method termed B in PG14), process-based models commonly take the difference in the net land-to-atmosphere carbon flux (F) between a simulation with and one without LUC (method D1; see Eq.2). Here, these conceptually comparable methods are both referred to as bookkeeping method. For method D1 it holds: [...]

> - Eqn. (5) in line 175: To have a self-contained paper, I would appreciate if you would derive eq. (5) explicitely instead of refering only to Gasser and Ciais (2013). I ask for this also because thereby the assumptions implicit to your approach would get transparent: It took me a while to find out that your formulas are valid only for LUC and FF perturbations starting out from equilibrium with no land use present. Only by this transparency one can understand your remark in line 206.

We added text to specify underlying assumptions:

A reference time (or period) t_0 is selected. At t_0 all land with total area A_0 is 'undisturbed' with respect to land use changes that take place *after* t_0. The reference area A_0 may include agricultural land that was converted before t_0. Net atmosphere-land carbon fluxes at t_0 and thereafter may not vanish as the land system may not be in equilibrium with the atmosphere. Under commonly used model setups, the extent of agricultural land in the reference state is small in comparison to the area under natural vegetation. Similarly, models are typically spun-up towards equilibrium and remaining trends in atmosphere-land fluxes are small. For simplicity, we neglect these disequilibrium fluxes below.

Additional fluxes arise due to forcings that occur after the reference time. We separate forcings into a land use change (LUC) and a non-land use change component (FF) such as fossil fuel emissions, nitrogen deposition, ozone changes etc. In a simulation without LUC, these additional fluxes occur on undisturbed land (subscript 'und') and are caused by FF (use of superscript analogous as in Eq. 3 and Eq. 4) and we write $F_0^{\mathrm{FF}}(t) = A_0\,\Delta f_{\mathrm{und}}^{\mathrm{FF}}(t)$. Δ denotes a change in a variable relative to the reference time t_0 (e.g., $\Delta f(t) = f(t) - f(t_0)$). Note that $f^{\mathrm{FF}}(t_0)$ is zero by definition. Below, we drop the specification of t. In a simulation with LUC, we can write fluxes occurring over land that has not been converted since the reference time t_0 (subscript 'und') and land that has been converted after t_0 (subscript 'dis') as [...]

Please note that we have changed the subscripts 'agr' to 'dis' (disturbed) and 'nat' to 'und' (undisturbed). This avoids an ambiguity as A_0 may include non-natural land.

- Upon request of the reviewers you included the additional terms arising from the combined action of LUC and FF (denoted by Ît'nat and Ît'agr in your paper). You interprete these terms as "non-linearities", suggesting implicitely that the other terms (e.g. in Eqs. (6)-(1)) represent the linear contributions. Please re-think this interpretation for the following reasons: In line 191 you make the splitting $\Delta f^{\mathrm{FF+LUC}} = \Delta f^{\mathrm{FF}} + \Delta f^{\mathrm{LUC}} + \delta$. This splitting can be interpreted in two ways: (1) As a Taylor expansion where Δf^{FF} and Δf^{LUC} are linear in the strength of FF and LUC perturbations, respectively, so that δ covers all remaining nonlinear terms. (2) As a splitting in the sense of factor analysis (as in Pongratz et al. 2014), i.e. Δf^{FF} comprises the full nonlinear Taylor expansion in the strength of only FF, while Δf^{LUC} the full nonlinear expansion in the strength of only LUC, so that δ comprises only those nonlinear terms mixed in FF and LUC. Following Stein and Alpert (1993) this latter term was called "synergy"âĂİ" in Pongratz et al. 2014 (and denoted as σ). The interpretation of your splitting affects how the different flux components of eLUC that you specify in eqs. (6) to (10) must be extracted from simulations. As far as I understand you want to employ the first interpretation, but your actual handling of simulation results is different: The simulated fluxes FFF and FLUC already contain nonlinearities in the FF and LUC forcing, respectively, to all orders. Hence the matching of the black curve ($F^{\mathrm{FF+LUC}}$) and red curve ($F^{\mathrm{FF}} + F^{\mathrm{LUC}}$) in Fig. 1 until the early 21st century is not indicating linearity in the forcings, but additivity in the flux components, meaning that the particular non-linearities showing up in the "synergies"âĂİ" between FF- and LUC-forcings are small. In fact you never test for linearity in the forcings in your paper.

In our submitted manuscript, we use the term "non-linearity" in the sense of a "synergy" effect following Stein and Alpert (1993), thus the second interpretation offered by the editor. Of course, linearity of a function is not fully given when only additivity is satisfied (which is what we assess in Fig. 1). We thank the editor for requesting this clarification. We have generally replaced the term 'non-linearity' with 'synergy'. For example (new text in bold):

[On line 169:] We then show that $e\mathrm{LUC}_{\mathrm{E2}} = e\mathrm{LUC}_0 + e\mathrm{RSS} + e\mathrm{LFB}$ **plus synergy terms**.

[On line 317:] Figure 1 reveals that global fluxes due to FF and due to LUC forcing alone combine in an almost perfectly **additive fashion** [...] and discernible synergy effects (δ, (Stein and Alpert, 1993)) emerge only in a future scenario [...].

[On line 308:] [...] wich is formally identical to Eq. 15, assuming **additivity of the FF and LUC forcings**.

[Caption of Fig.1:] The differences between the combined and the sum of effects correspond to the synergy terms δ, following Stein and Alpert (1993).

- Line 195: I think eLUC should read $e\mathrm{LUC}_{\mathrm{E2}}$. If not: Why not?

Thank you for spotting this typo.

- Line 263: I failed to derive eqn. (14) on my own. Please provide a justification for this equation.

This can be derived as follows. With the information given in Table 2, $F_0^0 = 0$ and $F_{\mathrm{LUC}}^0 = \Delta A\, f^0$:

$$
\begin{aligned}
e\mathrm{LUC}_{\mathrm{D3}} - e\mathrm{LUC}_{\mathrm{D1}} &= F_{\mathrm{LUC}}^{\mathrm{FF+LUC}} - F_0^{\mathrm{FF+LUC}} - F_{\mathrm{LUC}}^0 + F_0^0 &(1)\\
&= (A_0 - \Delta A)\, \Delta f_{\mathrm{und}}^{\mathrm{FF+LUC}} + \Delta A\, f^0 + \Delta A\, \Delta f_{\mathrm{dis}}^{\mathrm{FF+LUC}} - A_0\, \Delta f_{\mathrm{und}}^{\mathrm{FF+LUC}} - \Delta A\, f^0 &(2)\\
&= \Delta A\, \Delta f_{\mathrm{dis}}^{\mathrm{FF+LUC}} - \Delta A\, \Delta f_{\mathrm{und}}^{\mathrm{FF+LUC}} &(3)\\
&= \Delta A (\Delta f_{\mathrm{dis}}^{\mathrm{FF}} + \Delta f_{\mathrm{dis}}^{\mathrm{LUC}} + \delta_{\mathrm{dis}}) - \Delta A (\Delta f_{\mathrm{und}}^{\mathrm{FF}} + \Delta f_{\mathrm{und}}^{\mathrm{LUC}} + \delta_{\mathrm{und}}) &(4)\\
&= e\mathrm{RSS} + \Delta A\, (\Delta f_{\mathrm{dis}}^{\mathrm{LUC}} - \Delta f_{\mathrm{und}}^{\mathrm{LUC}}) + \Delta A\, (\delta_{\mathrm{dis}} - \delta_{\mathrm{und}}) &(5)
\end{aligned}
$$

The last line (5) is what is given by Eq.14 in the manuscript.

- In lines 200-203 you refer to your simulations before you have introduced them. I suggest to shift these few lines to the results section.

We chose to refer to Figure 1 here in order to illustrate the validity of the additivity assumption (synergy negligible) when introducing the formalism. Of course, this implies that results are shown before the model and the simulation protocol is introduced. In this sense, this is a dilemma. We follow the editor's suggestion and moved this text to the beginning results section and added a sentence to provide context.

Figure 1 reveals that global fluxes due to FF and due to LUC forcing alone combine in an almost perfectly additive fashion to the flux induced by the combined effect of FF and LUC up to present and discernible deviations (δ) emerge only in a future scenario of continuously rising CO_2 and changing climate and contribute ~10–20% by 2100 in RCP8.5. This confirms the validity of the additivity assumption ($\Delta f^{\mathrm{FF+LUC}} = \Delta f^{\mathrm{FF}} + \Delta f^{\mathrm{LUC}} + \delta$) that underpins the flux component decomposition in Sect 3.

- Lines 203-205: These lines come a bit as a surprise, because in line 191 you already introduced $A_0\, \Delta F_{\mathrm{und}}^{\mathrm{FF}}$ as F_0^{FF}.

Please note that we write $F_0^{\mathrm{FF}} = A_0\, \Delta f_{\mathrm{und}}^{\mathrm{FF}}$ (see l.187), where the capital F refers to the land-atmosphere flux in a given setup, whereas lowercase f is the area-specific flux over a given land use class (no land unit 'dis' present in simulation F_0^{FF}). We added some words to clarify this:

$A_0\, \Delta f_{\mathrm{und}}^{\mathrm{FF}}$ **is the land-atmosphere flux in a simulation forced only by FF and** can be interpreted as the potential land C sink (ePS) under environmental change caused by FF.

- Lines 421-424: The text in these lines entered the manuscript in response to remarks of Reviewer #1, but I fail to understand them. In these lines you argue that including the eRSS term in eLUC is "misleading in view of the actual reduction of land C sinks due to reduction of natural vegetation". You continue "This reduction of the residual sink due to the replacement of natural by agricultural vegetation" – I do not understand why "the replacement of natural by agricultural vegetation" could change the residual sink. The residual sink differs by including or excluding eRSS in eLUC, but not by changing the vegetation.

With this text, we wanted to highlight that the method by which *e*LUC is quantified is relevant for the interpretation of the residual sink when doing the global C budget and that replacing forest with agricultural land tends to reduce the sink capacity. We realise that the way it was formulated may cause confusion instead of clarification. We reduced respective text to the key message ("Inclusion of secondary LUC fluxes thus determines where the system boundaries between *e*LUC and the residual terrestrial sink are drawn.") and deleted the subsequent sentences ("The D3 method [...] on D1-type *e*LUC estimates.")

- Please make in Fig. 1 labels and caption consistent: The label of the red curve is $F_0^{\mathrm{FF}} + \Delta F_0^{\mathrm{LUC}}$ but in the caption you refer to it as $F_0^{\mathrm{FF}} + F_0^{\mathrm{LUC}}$. I know that $F_0^0 = 0$, but it confuses on first sight.

We greatly thank the editor for pointing this out. This was not just confusing, but simply a typo. The Δ label inside the figure should not be there. We corrected this.

References

Stein, U. and Alpert, P.: Factor Separation in Numerical Simulations, Journal of the Atmospheric Sciences, 50, 2107–2115, doi:10.1175/1520-0469(1993)050<2107:FSINS>2.0.CO;2, URL `http://dx.doi.org/10.1175/1520-0469(1993)050<2107:FSINS>2.0.CO;2`, 1993.

Strassmann, K. M., Joos, F., and Fischer, G.: Simulating effects of land use changes on carbon fluxes: past contributions to atmospheric CO_2 increases and future commitments due to losses of terrestrial sink capacity, Tellus B, 60, 583–603, doi:10.1111/j.1600-0889.2008.00340.x, 2008.