Editor’s comment on
Stocker and Joos

“Quantifying differences in land use emission estimates implied by definition discrepancies”

Earth System Dynamics Discussions, 6, 547-577, 2015

October 22, 2015

As editor I thank Stocker and Joos for carefully addressing 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 $e_{\text{LUC}}$ 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.

• Eqn. (5) in line 175: To have a self-contained paper, I would appreciate if you would derive eq. (5) explicitly instead of referring 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.

• Upon request of the reviewers you included the additional terms arising from the combined action of LUC and FF (denoted by $\delta_{\text{nat}}$ and $\delta_{\text{agr}}$ in your paper). You interpret these terms as “non-linearities”, suggesting implicitly 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 $\Delta f^{FF+LUC} = \Delta f^{FF} + \Delta f^{LUC} + \delta$. This splitting can be interpreted in two ways: (1) As a Taylor expansion where $\Delta f^{FF}$ and $\Delta f^{LUC}$ are linear in the strength of FF and LUC perturbations, respectively, so that $\delta$ covers all remaining nonlinear terms. (2) As a splitting in the sense of factor analysis (as in Pongratz et al. 2014), i.e. $\Delta f^{FF}$ comprises the full nonlinear Taylor expansion in the strength of only FF, while $\Delta f^{LUC}$ the full nonlinear expansion in the strength of only LUC, so that $\delta$ comprises only those nonlinear terms mixed in FF and LUC. Following Stein and Alpert (1993) this latter term was called “synergy” in Pongratz et al. 2014 (and denoted as $\sigma$). The interpretation of your splitting affects how the different flux components of $e_{\text{LUC}}$ 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 $F^{FF}$ and $F^{LUC}$ already contain nonlinearities in the $FF$ and $LUC$ forcing, respectively, to all orders. Hence the matching of the black curve ($F^{FF+LUC}$) and red curve ($F^{FF} + F^{LUC}$) 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” between FF- and LUC-forcings are small. In fact you never test for linearity in the forcings in your paper.

- Line 195: I think $eLUC$ should read $eLUC_{E2}$. If not: Why not?
- Line 263: I failed to derive eqn. (14) on my own. Please provide a justification for this equation.
- 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.
- Lines 203-205: These lines come a bit as a surprise, because in line 191 you already introduced $A_0 \Delta F^{FF}_{nat}$ as $F^{FF}_0$.
- 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 $e$RSS term in $e$LUC 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 $e$RSS in $e$LUC, but not by changing the vegetation.
- Please make in Fig. 1 labels and caption consistent: The label of the red curve is $F^{FF}_0 + \Delta F^{LUC}_0$ but in the caption you refer to it as $F^{FF}_0 + F^{LUC}_0$. I know that $F^0_0 = 0$, but it confuses on first sight.

Christian Reick