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Earth System Dynamics Discussions

Interactive comment on "A theoretical framework for the net land-to-atmosphere CO_2 flux and its implications in the definition of "emissions from land-use change"" *by* T. Gasser and P. Ciais

Anonymous Referee #1

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General remarks:

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.

Detailed comments:

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?

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.

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

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

Eq. 6 & 8: The last "tau" each should be "t".

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

Fig. 2 is referenced before Fig. 1

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.

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.

p. 189, l. 16: “than” should be “as”

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.

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

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?

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?

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 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. 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.

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.

p. 198, l. 7: “others” should be “other”

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?

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.

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"

p. 204, l. 6 should read "the total CO_2 flux integrated over time".

p. 205, l. 9: it is unclear what "change" refers to – add "due to CCN-perturbations".

Interactive comment on Earth Syst. Dynam. Discuss., 4, 179, 2013.