

Interactive comment on “Large differences in land use emission quantifications implied by definition discrepancies” by B. D. Stocker and F. Joos

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This study quantifies the net land use flux in a coupled Earth system model setup and the corresponding offline setup of the vegetation model. Based on these quantifications the authors describe the difference in flux components contained in each, with the explanation focussing on the terms “replaced sources and sinks” and “land use feedback”. They conclude with recommendations of using the “D1” approach, i.e. quantifying the net land use flux under constant pre-industrial environmental conditions, for global carbon budget estimates and for model intercomparisons.

The manuscript contains some new information that is worth publishing, and ESD is the obvious outlet for it given that two other studies on the topic of flux components contained in different net land use flux definitions have also been published in this journal

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(Gasser and Ciais, 2013 and Pongratz et al, 2014). I find it interesting to see at least a subset of the at least 9 different methods identified in previous studies be quantified consistently in the same model framework and with state-of-the-art land use datasets; this does not give the complete picture, but it is a good start for a comprehensive study at a later point. However, my feeling is that the way the manuscript is framed currently it will contribute to the confusion in the climate-carbon cycle community about the net land use flux rather than reduce it. While I think that little change is needed to the modeling itself and the analysis of the results to resolve this issue, the discussion and interpretation needs substantial shortening (very justifiable for a “short communication” manuscript type of ESD), clarification, and re-structuring.

General comments

I see the main value of the paper in two points:

(1) To give a quantification of some of the effects discussed in earlier studies consistently in the same modeling framework. Note, however, that these quantifications have partly been performed in consistent model setups by Strassmann et al 2008 (SM08) and Arora and Boer, GCB, 2010 (“Uncertainties in the 20th century carbon budget associated with land use change”). Here, more explanation is warranted in the introduction about the novelty of the current estimates. A quantitative comparison is needed to these studies as well as to the summary of the size of effects such as land use feedbacks by Pongratz et al 2014 (Pg14).

(2) To make it easier for the community to understand all the complications with net land use flux definitions discussed by SM08, Gasser and Ciais 2013, and Pg14 by giving a concrete example of model setup. This contribution in eliminating confusion in the scientific community is substantially hampered, however, by not being clear about what is old and what is new. The discussion in Sec. 5 is all right and interesting, but the key thoughts therein are identical with what Pg14 and Houghton, GCB, 2013 (opinion piece on “Keeping management effects separate from environmental effects in terres-

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trial carbon accounting”; not cited, but should be!) presented as conclusions. Presenting it here again in so much detail and without pointing to earlier discussions suggests to the reader that this is yet another effect and this is certainly contra-productive to eliminating confusion.

The manuscript contains “recommendations” for a specific choice of method, which, as explained below, I find subjective. It is up to the editor to accept such statements and treat the paper partly as an opinion piece, but this is not my understanding of ESD’s scope.

Specific comments:

Title:

The title in its current form is the conclusion from Pg14. The title needs to include what is new, along the lines of “Quantification of net land use flux components in coupled vs offline climate-vegetation models”.

Abstract:

l. 1 (and elsewhere, e.g. Introduction l. 21-22): “anthropogenic land use and land use change”: Why not stick with the common “land use and land cover change (LULCC)”? Land use implies the human component, and it does not hurt to spell out the land cover change part.

l. 7 (and elsewhere where “secondary” is used): “primary and secondary components”: I am flattered that the authors use the term “primary emissions” that we introduced some years ago, and understand that “secondary” is the logical counterpart. Still, the more common term are “indirect effects” of LULCC, see e.g. Houghton, GCB, 2013. I suggest to stick with “indirect”, but if not at least the relation between indirect and secondary needs to be stated.

l. 9 (and elsewhere): “up to 20% when quantified from ESM versus offline vegetation models”: Arora and Boer 2010 estimate the same difference, of D3 vs E2, to be more

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on the order of 100% (i.e., estimates for D3 are twice as high as E2). The manuscript would benefit very much from a more quantitative comparison against published estimates, both the individual studies and the summary by Pg14. For this specific line, a “in our modeling framework” has to be added at least.

l. 15 ff: “Therefore, we argue that synthesis studies and global carbon budget accountings should resort to the “least common denominator” of different methods, following the bookkeeping approach where only primary land use emissions are quantified under the assumption of constant environmental boundary conditions.”: See comment on “opinion piece” above. The choice of definition depends first and foremost on the application. For example, for global C budget estimates I argue that the prime aim is to keep the C budget closed and not attribute processes we know of to the residual (error) terms, thus the choice of eLUC definition needs to be made in agreement with the model setup for the residual terrestrial sink.

Introduction:

l. 58 ff: The study by McGuire et al accounted for climate/CO2 changes due to both LULCC and fossil-fuel emissions, not just “LULCC-induced environmental change”.

l. 83-84: “They state that the discrepancy between methods stems from the inclusion of the land use feedback on actual natural land.” Should rather read “...stems predominantly. . .”. As can be seen from Pg14 Fig. 2, E2 and D3 also differ in terms of fluxes on potential natural vegetation and synergy fluxes.

l. 90-98: It sounds as if the present study would quantify all possible methods, while indeed you focus on D1, D3, and E2. Please clarify this (and justify why you deem consistent quantification of these the most important).

l. 115: “total CO2 emissions from land use change”: misleading, better is “total C fluxes induced by LULCC” as also sink terms (e.g. the feedback) are included.

l. 120 ff and eq. 3: The replaced sources/sinks term as defined in this study indeed

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just refers to changes in environmental conditions by causes other than LULCC, but as discussed in Pg14 this term (under this or the other names, loss of additional sink capacity or land use amplifier effect) may also refer in published studies to LULCC-induced changes, or both. This needs to be clarified. In the definition used in eq. 3 it also comprises the effect of environmental changes on instantaneous emissions and legacy fluxes (the delta-f-I and delta-f-L fluxes in Pg14), which is worth clarifying because this is not obvious and because it makes comparison with Pg14 easier. Similarly, state that the last term in eq. 4 includes delta-I-I and delta-I-L.

I. 123: It would help many readers if you clarify that the “natural” in “natural land” refers to the vegetation types, not to the land area (which is agricultural). The term “potential natural vegetation” Pg14 adopted from the geography community was meant to do exactly this. In this context it won’t harm to explicitly state that eLFB affects both natural and agricultural vegetation (I. 122).

I. 128: What do you mean by “direct”?

I. 146 and elsewhere: You equate the bookkeeping approach to D1, which is what SM08 did but not what Pg14 did, so best is to be explicit how you use the term “bookkeeping” in your study.

I. 145: “abandonment of agriculture”: Which model would not include this term in eLUC?

I. 157: I believe the OSCAR model by Gasser and Ciais uses NPP, not C stocks, as input.

Sec. 2.2: Most of this section is rephrasing from Pg14. As stated above, this is confusing. I suggest to shorten the implications parts of this section and refer to the corresponding equations in SM08 and Pg14.

Eq. 8 ff: State explicitly that A0 refers to potential and actual natural vegetation. Further, the statement “Unlike suggested by PG14, the formal treatment presented here

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reveals that the difference is related to the land use feedback for the reference distribution of natural land and not the actual distribution.” is wrong. If you subtract the fluxes of E2 and D3 in Fig. 2 of Pg14 one can easily see that exactly the fluxes due to LUC-induced changes in environmental conditions on land of potential natural and actual natural vegetation remain. Eq. 13b and 15c in Pg14 show the same. Pg14 even show that there is an additional synergy term by which the two methods further differ. The formalization used by Pg14 may differ to the one presented in this study, but I don’t see any disadvantage thereof and cannot see any effect presented in the present study that has not been properly described in SM08 and Pg14 already. The key difference is that in the present study you refer to one very specific modeling framework, which allows you to eliminate much of the ambiguity that the land use feedbacks and the replaced sources sinks/loss of additional sink capacity/amplifier effect contain across earlier studies using different frameworks (discussed in Pg14 Sec. 4.1, 4.2 and 4.5). If you shorten this section, reference earlier work and clarify that you look at these two terms under a very well defined modeling framework then this section would not be confusing and set the basis well for the following analysis.

Methods:

I. 204 ff: Pre-industrial environmental conditions for the year 1700 are a pretty wild mix of 1700-CO2 and early-20th-century climate. The model shows substantial sensitivity of C stocks to environmental conditions (shown in I. 328), which I would expect to stem not only from difference in atmospheric CO2, but also in climate.

I. 210: Since the community knows and appreciates your earlier studies at other resolutions, could you quantify what the change in resolution means for eLUC for easier comparison to your other studies?

I cannot find the description of the setup for the bookkeeping approach.

I. 223: “As outlined in Appendix I”: It is not obvious where eRSS is derived in the Appendix.

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I. 226: Here is another intuitive way to understand eRSS: $(F^{(FF+LUC)}_{LUC} - F^{(FF+LUC)}_0) - (F^{LUC}_{LUC} - F^{LUC}_0)$, which is eq. 9 assuming linearity $F^{(FF+LUC)}_0 = F^{FF}_0 + F^{LUC}_0$. This is the difference between with- and without-LULCC simulations under total vs only LULCC-induced environmental changes.

Results:

I. 236: “~23%”: Compare to earlier work, e.g. Arora and Boer 2010, and discuss model-dependence of the estimates.

I. 243: I am confused: Why do you cite CRU climate here when you prescribe ESM climate to your simulations? (And does the 364 ppm refer to your ESM results or to observations?)

I. 263: “cumulative total emissions”: Clearer to speak of “cumulative net LULCC/LUC flux”. What do the percentages refer to, i.e. 6 and 21% relative to which method?

I. 264: “total (eLUC2) and primary emissions”: Why “total”? Might “net” be the better term again? Add “(eLUC0)” to “primary emissions”.

Discussion:

See comments above. Much of the discussion in Sec. 5.1 is not new over previous studies. In particular here, Houghton, GCB, 2013 argues the same way as you do in I. 308 -310; I. 313 f (implications for the residual terrestrial sink) has been discussed in Pg14.

I. 318: “The inclusion of secondary LUC fluxes [. . .] in eLUC and in turn in estimates of the implied residual sink is misleading when comparing to observational data”: This does not seem to be a valid argument since attributing C fluxes to the net LULCC flux as compared to the residual terrestrial sink is not doable on the large scale based on observations and always requires modeling.

I. 334: “quantifying eLUCD1 under preindustrial conditions is a viable and pragmatic

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solution”: I agree that using 1700-climate is a pragmatic solution, but what is the evidence that it is a viable one? Given the long history of LULCC, the choice of any time period is a subjective one; e.g. mid-Holocene conditions or Little Ice Age temperatures would give different results but could equally be justified. Wouldn't the only objective decision be to use environmental conditions from the time land use change first emerged? Similarly the definition of “present-day” is subjective; LULCC emission quantifications will likely be performed 10 years from now as well, when the atmospheric CO2 concentration will have substantially risen.

I. 345 ff: “In summary, we recommend not to rely on results from method D3 or E2 in the context of the global (or regionalized) carbon budget, but to apply method D1 (under preindustrial conditions).” I agree with recommending D1 for deriving net LULCC flux estimates from multi-model studies when the aim is to narrow down the model spread, because it excludes some of the highly model-dependent processes such as CO2-fertilization strength. But as mentioned above for global C budget estimates the method for eLUC depends on the method chosen for the residual terrestrial sink, if an independent method is used for the latter, to close the C budget. If the present-day residual terrestrial sink is derived as residual from coupled simulations and eLUC estimates, then fluxes induced by environmental changes since the preindustrial era, in particular higher emissions due to higher standing biomass stocks, are attributed to the residual terrestrial sink if eLUC is derived from D1 simulations. This is so counter-intuitive that I would recommend method D3 instead (it also disagrees with the authors' argumentation that D1 represents observable processes: observable changes in C stocks upon deforestation include effects of higher standing biomass). My point is: Such a recommendation is subjective and unless the editor decides that this is an opinion piece I would refrain from any subjective recommendations. I recommend that the authors restrict the text to the discussion on the advantages of method D1 in terms of ease of setup in common MIPs and narrowing down of the model spread, which are undisputable and relevant points. Then the limitations of D1 as e.g. above should shortly be mentioned, but for longer discussions of the purpose of different methods

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the reader can be referred elsewhere.

Sec. 5.2: I am not sure what the purpose of this section is. To state that C fluxes are just one of many effects of LULCC on the Earth system? But then the effects in paragraph 1 seem a bit random – what about biogeophysical effects, or other greenhouse-gas-related management effects such as wetland management effects on CH₄? The discussion of paragraph 2 on eRSS and eLFB is repetitive from earlier sections. See comment before (“opinion piece”) on the recommendation of l. 370 f.

l. 372 f: “We argue that offline-vegetation model setups are not capable of separating eRSS and eLFB as defined here.”: No need to argue, this is a fact.

Conclusions:

l. 377: See comments above on “recommendations”. Again, who defines what “pre-industrial” means?

Appendix:

The equations are all right and good, but I would structure them in a more helpful way: Show with the equations on the one hand which simulations need to be subtracted from each other to isolate individual flux components (basically the information of Tab. 2), and on the other hand how each of these components would be expressed in the area-flux notation. For example, it is not immediately clear that l. 117 refers to ePS, l. 420 to eLUC0, or l. 425 to eLUC0 + eLFB. Derivation of eRSS seems to be missing.

l. 412: Add again the assumption about linearly adding fluxes.

l. 418-419: “the land does not “see” any changes in climate and CO₂ (no fossil fuel emissions)”): Confusing. How about “A run with LUC but with prescribed environmental conditions unaltered by LULCC and fossil-fuel burning”?

Figure and Tables:

Fig. 2 looks very similar in content and numbers to Fig. 8 by SM08. Given that

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also much of the discussion in the manuscript is rephrasing SM08 and Pg14 I feel the current form of the manuscript is really overselling the novelty of this study.

Typos etc:

l. 8: Introduce abbreviation “ESM” here (used later).

l. 13: Explain abbreviation “C” here. It seems a bit arbitrary at which occasions the authors use “C” and at which “carbon” throughout the manuscript.

l. 99: quantification should be singular.

l. 110: ST08 should be SM08; “of” missing after “definitions”.

l. 113 and elsewhere: Be consistent: book-keeping or bookkeeping? (I believe the latter)

l. 121: “which” should be “that”.

l. 137: typo in deposition

l. 149: “is” missing

l. 160: remove “for”

l. 372: Remove hyphen in “offline-vegetation model setups” (the setup is offline, not the vegetation).

l. 391: typo in following

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