

## ***Interactive comment on “Hotspots of tropical land use emissions: patterns, uncertainties, and leading emission sources for the period 2000–2005” by Rosa Maria Roman-Cuesta et al.***

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Received and published: 24 April 2016

This paper presents a spatial dataset of greenhouse gas emissions from agriculture, forestry and other land use ('AFOLU'), covering the Tropics (incl. extra-tropics in South America and Africa) and years 2000-2005. This is a combination of several different published spatial (and non-spatial?) datasets for individual sources and different greenhouse gases (CO<sub>2</sub> from deforestation; CO<sub>2</sub>, N<sub>2</sub>O and CH<sub>4</sub> from fires; soil C loss and, N<sub>2</sub>O from cropland soils; soil C loss, N<sub>2</sub>O, and CH<sub>4</sub> from rice paddies; CO<sub>2</sub> from wood harvesting; N<sub>2</sub>O and CH<sub>4</sub> from livestock). Although no novel data is presented here, an added value of combining different data sources can be justified. Using respective greenhouse-warming potentials for N<sub>2</sub>O and CH<sub>4</sub>, all data is converted to

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CO<sub>2</sub>-equivalents. This enables a localisation and comparison of major AFOLU GHG emitting regions. Using a measure for uncertainty of this data, the authors then go on to identify priority regions for mitigating AFOLU GHG emissions. They conclude, that (although their uncertainty analysis suggests otherwise) mitigating emissions from deforestation is particularly desirable.

The study provides a data product, offering "a spatially detailed benchmark for monitoring progress on reducing emissions from the land sector in the tropics" (1.52-53). The dataset potentially bridges the gap between science and policy in that it offers numbers for GHG emissions in a form (spatially, uniform unit and a single dataset) that makes it most useful for policy-making and administration (outside science). I guess there is a demand for such products also outside the scientific community and the spatial presentation and the accounting of uncertainties is appealing and useful (I also think that it's important to convey an idea of the uncertainty of presented data, and in general, to do the best possible is to be preferred over doing nothing).

Publication of such work in a scientific journal is to be encouraged, with peer-review guaranteeing scientific rigour and transparency of the analysis. However the paper in the current form is in my view not ready for publication in Biogeosciences. I have several questions regarding, in my view, major issues. If these can be addressed, a revised version this study may warrant publication.

One of my major concerns revolves around the distinction between 'gross' and 'net'. Even within the land use change (modelling) community, these terms are not used consistently. In my reading, 'net' is used here as the land-atmosphere flux (aggregated over a spatial domain) that results from any human-induced land use and land use change, with regrowth after abandonment and reforestation compensating C loss after deforestation. 'Net' is thus inherently scale dependent (at the scale of a forest stand/tree, all is gross). A concise definition of what these terms refer to here is missing.

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But more problematically, the different components that went into the CO<sub>2</sub> emission estimate are sometimes gross emissions, sometimes net emissions. The deforestation component (based on Harris et al., 2012) is based on remotely-sensed above-ground biomass loss. At its spatial resolution ("MODIS data at 18.5 km") it's a net flux. In contrast, the wood harvest and fire data represent gross fluxes (regrowth compensates X% of the initial reduction in C density; where  $X < 100\%$  in the case of an increasing trend in the disturbance regime w.r.t frequency and/or severity; and  $X > 100\%$  in case of an increasing trend in the same). In view of the choice of publication outlet (Biogeosciences) and the (apparently) targeted readership outside the policy/administration community, I also encourage that authors provide a precise definition of how CO<sub>2</sub> emissions from are quantified, following e.g. the categorisation by Pongratz et al., 2014.

Below, I provide a list of major issues in a more specific way:

- Inclusion of fire emissions: GHG emissions from fires from "woodlands", "forests", and "peatlands" are included, while emissions from "savannah", "agriculture" and "deforestation" are not. It is not clear to me why fires from "woodlands" and "forests" are considered to be generally and necessarily non-natural (=anthropogenic). In view of the fact that fires in "savannahs" are generally not, this is an arbitrary choice. Moreover, instantaneous fire emissions are not to be equated to a net CO<sub>2</sub> source on larger spatial and longer temporal scales. Regrowth after fire compensates for initial emissions and only a change in the fire regime (intensity, frequency) induces a net source or sink. The gross emissions used here are therefore not appropriate. In view of the large contribution from fires (see Fig 5), this aspect substantially undermines the total numbers presented here.

- Inclusion of wood harvesting: Basically the same argument as above goes for CO<sub>2</sub> emissions from wood harvesting. C in wood extraction is not to be equated to CO<sub>2</sub> emissions. Regrowth (partly) compensates initial reductions in forest C storage. In simulations published in Stocker et al. (2014), global wood extraction of  $\sim 1.1$  GtC/yr are accounted for. The respective net effect on global CO<sub>2</sub> emissions in these simula-

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tions is only about 0.2-0.3 PgC/yr. The treatment of this component therefore implies an overestimation of respective emissions by a factor of  $\sim 5$  and is therefore not appropriate.

- Double counting:

- Inclusion of CO<sub>2</sub> emissions from peat (burning?): This part was unclear to me. "Peat" fires were included from the Van der Werf et al. (2010) data, but also the Harris et al. (2012) data seems to include emissions from peat. Could this be a double-counting?

- If wood harvesting is achieved by clear cutting substantial areas, then this should be captured also by satellites and therefore included in the emissions from "deforestation" (Harris et al. 2012 data). I suspect this could imply another instance of double-counting.

- The same for fire activity: If the fire-induced conversion from forested to non-forested land is captured in the Harris et al. (2012) data then additionally accounting for it by "fire emissions" is double-counting.

- Greenhouse Warming Potentials (GWP): The comparison of different GHGs relies on the GWP metric for N<sub>2</sub>O and CH<sub>4</sub>, expressed in CO<sub>2</sub>-equivalents. This involves a necessarily arbitrary choice of time scale for which the GWP values are calculated. The choice of values used here is intransparent. Values used for CH<sub>4</sub> and N<sub>2</sub>O are 21 and 310. These are somewhat "hidden" in Table 1, and are used without reference. Resp. values used in IPCC AR5, WGIII are 28 gCO<sub>2</sub>-e/gCH<sub>4</sub> and 265 gCO<sub>2</sub>-e/gN<sub>2</sub>O (Myhre, G. et al. in Climate Change 2013: The Physical Science Basis (eds Stocker, T. F. et al.) Ch. 8 (Cambridge Univ. Press, 2013).). The choice of the time scale of GWPs directly underpins the results and conclusions drawn here. Tian et al. (2016) presented all their results for both GWPs both at the 20 and 100 years time scale which offers a more robust picture.

- Conclusions for mitigation priorities: I don't agree that high uncertainty of estimates

of land use change emissions justifies low priority. The clue is that (deriving from Fig. 5) there is a very high probability CO<sub>2</sub> emissions from deforestation are higher than other emission sources (lower margin of confidence interval of “deforestation” is higher than upper margin of confidence interval of “livestock”, “crop”, and “rice”).

- Treatment of uncertainties: A rather pragmatic approach is followed here to provide uncertainty estimates from wood harvest and livestock emissions complementing those from more rigorously derived ones provided e.g. for the deforestation data by Harris et al. 2012. An “expert judgment” is used to set them at 20%. Furthermore, I did not understand how the authors dealt with spatial autocorrelation of uncertainties. Aggregating uncertainty across space requires to make an assumption w.r.t the spatial autocorrelation. Assuming zero auto-correlation implies very small uncertainty in aggregated values. I suspect that the low uncertainty in livestock emissions presented in Fig. 5 is linked to such an assumption but I didn’t fully understand the method followed here. I think, the opposite (perfect auto-correlation) is more appropriate here.

- Careful about plagiarism: l.470-471: Exact same wording used as in Harris et al., 2012.

Other points:

- title: specify emissions (e.g. greenhouse gas emissions) - l.35: same as above: what emissions? - l.37: “... roughly contributes with a quarter “: source? - l.47: “gross emissions”: please provide a concise definition of the terms gross and net and specify what the focus of this study w.r.t. gross vs. net. - l.61: why 450 ppm? source? - l.63: To stabilise concentrations, emissions of N<sub>2</sub>O and CH<sub>4</sub> don’t have to be reduced to zero, only those of CO<sub>2</sub> have to be zero. - l.69: why “optimistic”? - l.104: Unconcise use of the term “sink”: Is C uptake during forest regrowth after an anthropogenic disturbance considered a “natural sink” here? - l.125: What is the “deforestation layer”? specific product? very community-specific language. - l.137-142: Needed here: specification of what “deforestation” means. C only? above-ground biomass only? Legacy effects? To

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be most specific, please provide a definition according to the classification by Pongratz et al., 2014. - l.147-149: Unclear: "expressed" where? - l.164: Why is the original high resolution data not used but the 1degree res. data instead? - l. 174 onwards: What information goes into simulating impact on SOC? change in litter input? management? - l. 187: Does this mean that global emissions from that dataset only capture losses from 61% of all cropland area (and thus represent only ~60% of global emissions)? or is the rest rice (and therefore included in respective emissions as described below) plus peat/histosols? - l. 217: unclear what this means. does it conserve global totals when area-specific values are integrated over? - l. 219: what is the "AFOLU Monte Carlo simulation"? - l. 225 onwards: Unclear: How is this information used? All of these categories are covered by other sources already. (after reading it again I realised that what is presented under "Databases" is just used for comparison). - l.252: But cropland dSOC data is all 'legacy emission'! - l.334: what is "AFOLU budget"? total GHG emissions from the AFOLU sector? - l.338: "good agreement" is not surprising as these are not independent sources. - l.409: I suspect this statement is wrong. CO2 uptake in their analysis is net land balance (land use emissions minus residual sink), therefore the statement as made here suggests too high effect of CH4 and N2O versus CO2. - there are two Fig. 5 - no uncertainties provided in Fig. 6

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Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-99, 2016.

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