

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 (CO₂ from deforestation; CO₂, N₂O and CH₄ from fires; soil C loss and, N₂O from cropland soils; soil C loss, N₂O, and CH₄ from rice paddies; CO₂ from wood harvesting; N₂O and CH₄ from livestock).

Comment 1: "Although no novel data is presented here, an added value of combining different data sources can be justified" We disagree with the reviewer that no novel data are presented. The AFOLU emission estimates at the landscape scale offered in this research and their associated uncertainties are novel data. They derive from existing, spatially explicit datasets (all our datasets are spatially explicit), but their merging offer novel, spatially explicit, AFOLU emission data.

Comment 2: "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". Our manuscript separates the concept of mitigation potential from economic/technical feasibilities but we agree with the reviewer that Figure 5 and associated text benefitted from further contextual information to avoid confusion. See lines 329-349. The way the manuscript is written, keeps the further differentiation between mitigation potential and economic/technical feasibilities for the conclusions. We have hopefully improved this issue now.

Comment 3: "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". We agree with the reviewer that defining net/gross accurately is important. We had already devoted lines 98-112 to deal with these differences, but we have now explicitly stated what we are NOT considering as part of the gross AFOLU flux (e.g. forest growth, secondary forest regrowth after land use change including reforestation/afforestation, forest recovery after disturbance not leading to deforestation such as fire, soil organic carbon storage in forests, agricultural areas or wetlands). For some reasons the reviewer focuses on forests sinks but AFOLU has more sinks to consider. We could agree with the reviewer that the value of net emissions is scale dependent, but the concept is not. We disagree that at the stand scale, all is gross: wood growth (with or without disturbance) and soil organic carbon storage, are absorptions to consider as sinks, which would make the resulting value different from gross fluxes. Further information about gross/net approaches is offered at SOM.

The problem of net/gross definitions is quite a core one in the reviewer's comments and a problematic one, since we believe the reviewer is defending a personal understanding of net/gross that is not in line with common understanding. None of the authors included in this manuscript agrees with his conceptual approach, and since the reviewer himself mentions there is not a common agreement, we have improved the description of our own understanding, for readers to understand our assumptions, rather than adjusting to the reviewer's.

Comment 4: "But more problematically, the different components that went into the CO₂ emission estimate are sometimes gross emissions, sometimes net emissions". This is not true. The only exception would be, perhaps, the exclusion of grasslands and agricultural fire emissions and removals based on assumptions on carbon neutrality exposed in the IPCC 2006 AFOLU Good practice guidance. No absorption other than this has been considered and the paper is consistent in its gross approach (or at least with our understanding of gross, which does not seem to match the reviewer's).

Comment 5: "The deforestation component (based on Harris et al., 2012) is based on remotely-sensed above-ground biomass loss. At it's spatial resolution ("MODIS data at 18.5 km") it's a net flux". We disagree. Harris data only considers biomass (AGB-BGB) removals, and part of the complications Harris had to publish her research in Science was, precisely, that her data was only gross emissions (and uncomplete accounting, since not all carbon pools were considered). We believe the reviewer is defending a personal understanding of net/gross that is not in line with common understanding, or we are not following well his ideas.

Comment 6: "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)" We are not sure what is the message here, nor the rationale used to sustain it, but we believe the reviewer applies the concept of gross/net based on a *a priori* temporal scale (e.g. what processes happen to forests, and nonforests before the emissions/disturbances are produced), while we (and we believe most people in the AFOLU world) would apply the concept of net emissions based on a posteriori approach (what grows or is stored in soils after disturbance). We do not include regrowth compensation after harvesting/fire, and whatever was compensated before (forest growth, soil carbon accumulation) the emission is produced (e.g. deforestation, fire, wood removal), is not accounted for. Whatever biomass is lost, or whatever non-carbon GHG emission is produced as a result of the disturbance (deforestation, degradation, agricultural production, livestock dynamics) is considered to be the gross emission.

Comment 7: "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 CO₂ 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". The reviewer is confusing our reading here. The reason not to include grasslands and agricultural fire emissions is not because we consider them to be natural (non-anthropogenic) but because there is an assumption of carbon neutrality over these non-woody materials affected by fire, as exposed in IPCC 2006, AFOLU good practice guidance (but please note that this only applies to CO₂ but not to N₂O nor to CH₄). Deforestation fires are excluded to avoid double counting with deforestation emissions, since they are produced from independent data sources and could offer repeated data. We have slightly improved this section, although we believe it was quite clearly exposed already (lines 159-166).

Comment 8: "Moreover, instantaneous fire emissions are not to be equated to a net CO₂ 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 ". We agree with the reviewer with this first statement: changes in fire regimes can fully change the amount of carbon not compensated in the recovery after fire (N₂O and CH₄ are never compensated so fires are always net sources of emissions when dealing with GHGs and not with carbon assessments). Independently of this fact, the reviewer's statement does not affect our manuscript in any way since no a posteriori fire recovery is here included.

Comment 9: "Inclusion of wood harvesting: Basically the same argument as above goes for CO₂ emissions from wood harvesting. C in wood extraction is not to be equated to CO₂ emissions. Regrowth (partly) compensates initial reductions in forest C storage. In simulations published in Stocker et al. (2014), global wood extraction of ~1.1 GtC/yr are accounted for. The respective net effect on global CO₂ emissions in these simulations 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 ~5 and is therefore not appropriate". Again, the reviewer's understanding of gross and net is not a common understanding (e.g. regrowth/recovery are not included in our gross assessment, nor soil organic carbon accumulation in forests). A more specific answer to this comment comes from the owner of our wood harvesting data: "Wood harvest here constitutes the annual removal of roundwood and fuelwood from forests as reported by countries to the FAO. The fate of the harvested product is either as waste (i.e., slash) or as product (i.e., paper, furniture, construction), and thus we acknowledge that the instantaneous flux from wood harvest would be lower when the lags in decomposition are considered. In terms of forest regrowth, wood harvest is a gross flux since no regrowth is considered. In any case, the replanting of forests following harvest does compensate to a small extent the biomass removed in wood harvesting, but the small growth of first year seedlings is not comparable to the removal of mature trees in terms of stocks. While the study of Stocker et al (2014) suggest otherwise, the scale at which forest demographic processes are represented in their model simulations are likely to be coarse to accurately reflect the carbon balance of regrowth and gross wood harvest removals. For example, in their simulation, feedbacks between wood harvest and the size of the 'representative individual' concept would reduce overall biomass resulting in the lower flux suggested in their study "

Comment 10: "Inclusion of CO₂ 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?" The reviewer is right that there might be some space for overlapping for CO₂ emissions on areas of peat fire in Van der Werf et al. (2010) and deforestation in Harris in Indonesia, although this will not affect non-CO₂ emissions (since soils are not included in Harris). Unfortunately, Van der Werf et al. (2010) does not clarify the way peat burning is separated from tropical humid deforestation, and recognizes: 'our inability to separate increased fire persistence due to repetitive burning of aboveground material from increased fire persistence due to burning of peatlands. In other words, the high fuel consumption in Equatorial Asia may be a consequence of the co-existence of forests and peat soils, especially in deforestation areas where drainage canals expose peat', but Van der Werf states that only Indonesia is affected since the lack of spatially-explicit maps, peat and organic soil burning outside Indonesia were not included. The fact that Indonesian peat emissions are based on spatially explicit maps on peat distribution makes us assume that the emissions on this area are assigned to peat fires, independently if deforestation was also part of the emissions, and there might be some overlapping with Harris data. Since organic soils are confined to certain parts of Indonesia and the emission contribution from soils is much larger than the forest contribution, we believe this overlapping will not affect the CO₂e budgets offered in this research. Some warning is included in lines 165-167.

Comment 11: "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". This is an interesting point of difficult solution. Since wood harvesting mainly derives from national reporting to FAO, it is assumed that it mostly affects forests remaining forests (legal logging). A visual validation of deforestation emissions and harvesting emissions, as offered in Figure 3 in the SOM, shows different spatial locations of these emissions, somehow corroborating that peak emissions of deforestation and peaks of wood harvesting are kept spatially separated. Because the spatial grid is 0.5, there is also room for deforestation and wood harvesting to overlap. So, while there may be a little double counting, it is difficult to quantify and to resolve.

Comment 12: "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". That was the reason why we excluded deforestation fires. Please refer to lines 164.

Comment 13: "Greenhouse Warming Potentials (GWP): The comparison of different GHGs relies on the GWP metric for N₂O and CH₄, expressed in CO₂-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 CH₄ and N₂O 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 gCO₂-e/gCH₄ and 265 gCO₂-e/gN₂O (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 reviewer is right that this point required attention. We have improved table 1, separating molecular weights from GWP and better referring AR4 (included as IPCC 2007 reference). Justifications of why AR4 is chosen against AR5 are exposed in lines 270-273. All the emissions datasets used in this research were produced prior to the launching of AR5, and used 100 GWP based on AR4, we respected their selected choice to be consistent with datasets where emissions could not be reproduced using the new GWP from AR5. Also, EDGAR FT2.0 AFOLU data used 100 year GWPs from AR4.

Comment 14: "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". Tian et al. (2016)'s research has a direct implication on climate forcing, while ours doesn't. Moreover, even if we applied different GWP, the trends and conclusions would not change much since annual AFOLU emissions in the tropics, for 2000=2005, are largely led by CO₂ emissions (ca. 70%) (Table 2).

Comment 15: "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 CO₂ 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") " We have improved the contextual information around figure 5 (lines 325-345), to make our points clearer. We understand the reviewer confusion, but our conclusion regarding priorities starts with the statement of 'effectiveness of the mitigation action'. If we are searching to guarantee mitigation effectiveness (not efficiency), high emissions with high uncertainties would not be the

target. However, besides the importance of mitigation potentials (gross emissions that could be reduced), there are technical and economic feasibilities that would make mitigation action on agriculture difficult, and appealing in forests (because it is cheaper and it is easier to implement). This fact would reduce the efficiency of prioritizing agricultural mitigation action. Please refer to the revised text (lines 329-349). Also note that we reserve further details on this point for the conclusions, where we refer to estimated mitigation costs to validate the fact that forests are still high mitigation priorities, for their efficiency rather than for their effectiveness.

Comment 16: "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". The reviewer points out a truly important point here, which does not have a perfect solution since there is no information about the spatial correlation of the data when changing to a different spatial support unit (from the 0.5° cell grids to continental or tropical scales). As exposed in lines 282-286 and further described in page 25 in the SOM, we took a conservative approach and assumed full spatial dependence (perfect autocorrelation) which results in 'a worst scenario possible' for the uncertainties. Thus, under this assumption uncertainties take their most extreme thresholds. Due to the large implications of assuming full spatial dependence versus full spatial independence during the spatial aggregation of the uncertainties, we are now currently developing another manuscript where both assumptions are considered, and data are offered to help understand what are the statistical implications for these choices, and how they would affect the prioritization of mitigation areas.

Careful about plagiarism: I.470-471: Exact same wording used as in Harris et al., 2012. Yes, some changes made (line 489-491)

title: specify emissions (e.g. greenhouse gas emissions). We prefer not to make the title longer

I.35: same as above: what emissions? Emissions are specified at line 42, when we refer to our own research.

I.37: "... roughly contributes with a quarter ": source? – this is an abstract, no reference added, but the cite comes from Smith et al. (2014).

I.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. Not in the abstract.

- I.61: why 450 ppm? source? 450ppm relates to RCP 2.6 scenario, which is used for the 2 degree target 'The RCP 2.6 scenario represents 2.6 W/m² radiative forcing in 2100, or ~450 ppm of CO₂e in 2100, which results in a 66% or "likely" chance of staying below the UNFCCC's 2°C warming limit (van Vuuren et al., 2011)'. The citation is IPCC 2014, at the end of the paragraph. The quote of 450ppm is rather contextual and informative for the reader, and we would rather not introduce further citations at this point.

I.63: To stabilise concentrations, emissions of N₂O and CH₄ don't have to be reduced to zero, only those of CO₂ have to be zero. While this might be true, it is out of focus of what is written in lines 59-64. Moreover, Table SPM.1 on emissions scenarios, at the Summary for Policy makers (IPCC 2014) talks about CO₂e, so we have added CO₂e, instead of CO₂, in line 61.

I.69: why "optimistic"? These are optimistic projections of what could be achieved if mitigation implementation was easy and smooth (no price variations political will, etc), and if bioenergy did not result into further deforestation. It is a warning adjective to call attention to the readers that these numbers are optimistic...not truly relevant.

I.104: Unconcise use of the term "sink": Is C uptake during forest regrowth after an anthropogenic disturbance considered a "natural sink" here? See comment 3.

I.125: What is the "deforestation layer"? specific product? very community-specific language. Agreed, paragraph improved and 'deforestation layer' term removed (line 128)

I.137-142: Needed here: specification of what "deforestation" means. C only? above-ground biomass only? Legacy effects? Agreed. Added. Lines 140-143.

I.147-149: Unclear: "expressed" where? Van der Wert et al. 2010 is added in the line (line 153)

I.164: Why is the original high resolution data not used but the 1degree res. data instead? – Irrelevant. This decision concerns Poulter reference not to us.

I. 174 onwards: What information goes into simulating impact on SOC? change in litter input? management? Please refer to the reference section and Li and Ogle references for further information.

I. 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? The DAYCENT model produced emissions for only six major croplands, excluding other croplands because the model was not yet well parameterized to estimate those emissions (Ogle pers. Comm). This results in 40% of area difference between FAO's global cropland areas and DAYCENT's global cropland areas, which of course do not mean a difference of 60% of emissions since area and cropland emissions are not linearly related. We are just

finalising a paper that compares AFOLU gross emissions from six major datasets, and this problem is further explained there. Some hints of this problem appear in Figure 6.

I. 217: unclear what this means. does it conserve global totals when area-specific values are integrated over? We have removed this sentence, since it is better explained in lines 275-277. This comment is specifically directed to GIS readers who are likely to question about this issue.

I. 219: what is the “AFOLU Monte Carlo simulation”? Confusing sentence, removed.

I. 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) The reviewer is right, the goal of comparing these datasets was not properly introduced in the manuscript (there used to be a question but somehow this version of the manuscript had dropped it). We have now re-introduced question 4, at the end of the introduction (lines 124-125) to correct this problem.

I.252: But cropland dSOC data is all ‘legacy emission’! The reviewer has a point here, but it does not affect dSOC only but all gases. Thus, changes in soil organic carbon contents dSOC (CO₂) are estimated through the DAYCENT, and DNDC models for the years of our analysis 2000-2005. These models have indeed temporal spin ups, as it is also the case for the GFED fire emissions with the CASA model. Their estimated emissions in 2000-2005 (not only dSOC but all the other gases) include, therefore, some legacy effects. The warning rather referred to remote sensing based data such as deforestation. The entire paragraph has been improved. lines 254-263.

I.334: what is “AFOLU budget”? total GHG emissions from the AFOLU sector? Yes. Budget changed to emissions elsewhere in the paper.

I.338: “good agreement” is not surprising as these are not independent sources. We disagree. Only wood harvesting is dependent, and only so, because Poulter’s dataset improved FAO’s using more detailed national statistics, including national forest inventories. All other emission sources used in this research were either fully independent (deforestation, fire, cropland), or only partially dependent (stock heads for livestock, and rice areas for paddy rice). In any case, FAOSTAT applies Tier 1 while most of our emissions have been estimate at Tier 3. So the agreement is quite surprising, especially because the disaggregation of the AFOLU budget among emission sources for the three datasets shows large disagreements.

I.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. This point might be true, but it is irrelevant. The use of Tian et al. (2016) results in lines 428-431 was to reinforce our prior claim on the importance to run multi-gas AFOLU research, instead of only focusing on carbon modelling (CO₂), which offers a partial understanding of the role of atmospheric GHGs on climate forcing.

there are two Fig. 5. Thanks. Solved.

no uncertainties provided in Fig. 6. Correct. No uncertainty offered in the paper, because we already have too many figures, but will be offered in the website data access.

http://www.wageningenur.nl/en/project/Agriculture_Forestry_and_Other_Land_Use.htm