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Discussions

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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REVIEWERS' RESPONSES

We would like to thank the reviewers for their useful comments and constructive criticism. Some of the raised concerns coincided and they might have been developed in more detailed only once. For this reason, we would kindly request to double check all our responses.

REVIEWER 2 This paper presents a spatial dataset of greenhouse gas emissions from agriculture, forestry and other land use ('AFOLU'), covering the Tropics (incl. extratropics 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 rational used to sustain it, but we believe the reviewer applies the concept of gross/net based on a priory 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 posteriory 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 CO_2 but not to N_2O nor to CH_4). 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 CO_2 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 (N_2O and CH_4 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 CO_2 emissions from wood harvesting. C in wood extraction is not to be equated to CO_2 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 CO_2 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 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 CO_2 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 CO_2 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-CO_2 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 coexistence 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 necesserarly 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 doesnt. 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: l.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 l.35: same as above: what emissions? Emissions are specified at line 42, when we refer to our own research. l.37: " . . . roughly contributes with a quarter ": source? – this is an abstract, no reference added, but the cite comes from Smith et al. (2014). 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. Not in the abstract. - l.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. l.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. l.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. l.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. l.125: What is the "deforestation layer"? specific product? very community-specific language. Agreed, paragraph improved and 'deforestation layer' term removed (line 128) l.137-142: Needed here: specification of what "deforestation" means. C only? above-ground biomass only? Legacy effects? Agreed. Added. Lines 140-143. l.147-149: Unclear: "expressed" where? Van der Wert et al. 2010 is added in the line (line 153) l.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. l. 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. 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? 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. l. 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. l. 219: what is the "AFOLU Monte Carlo simulation"? Confusing sentence, removed. 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) 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. l.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. l.334: what is "AFOLU budget"? total GHG emissions from the AFOLU sector? Yes. Budget changed to emissions elsewhere in the paper. l.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. 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. 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

REVIEWER 1 The authors describe a novel spatially comparable dataset containing annual means of gross Agriculture Forestry and Other Land Use emissions, an important contributor (one fifth in 2010) to the total global emissions. They identify a breakdown of the most important sources of CO2-e in the different part of the world; deforestation in Central/South America, forest/savanna fires in Africa and peatland/agriculture/rice emissions in Asia.

Comment 1"They also claim that although agriculture and forestry roughly have the same mitigation potentials, but their economic feasibilities differ, with the forestry sector being much more cost effective than agriculture, which is an important outcome. However, mitigation strategies in agriculture could be interesting for other reasons than mitigation of emissions (stopping/reversal of degradation, improvement of soils for soil organic carbon leading to more efficient water use and higher yields) ". Yes, true. We have improved paragraph 330-350, but would rather focus on climate change mitigation arguments, to avoid complicating the discussion.

Comment 2 "The paper addresses uncertainties, and tries to identify hotspot regions for the best abatement possibilities. However, this is derived from various guestimates, which makes the end result a bit less robust in my opinion (page 7, line 174 'authors expert opinion', page7 line 189 'known poor performance of the DAYCENT model over organic soils', page 8, line 209 ; authors export judgement', page 8, line 221 'expert judgment'., suppl. material also. In this way, many educated guesses are introduced and it is not clear to the reader on what ground these estimates were based, and more important, how this might influence the end result. However, I do realize that at this moment this is probably the best spatially explicit effort available and the paper therefore has its own merits". We agree with the reviewer that several approaches in this research make the final results on uncertainties less robust (e.g. expert judgement for the uncertainties of two emission sources), but even the models that estimate emissions uncertainties are full of assumptions, so having the real spatial uncertainties for our two missing datasets would be desirable, but it would not necessarily guarantee higher robustness. We have tried to be as transparent as possible in this manuscript, so that the reader can weight how trustable they find the results. For us, even more problematic than these guestimates are the assumptions behind uncertainty aggregation when scaling up. Thus, assumptions regarding data correlation (data complete dependence versus complete independence) have an impact of orders of magnitude on the final aggregated uncertainties. We are currently working on a paper on this topic, since it impacts the prioritization of mitigation regions and emission sources.

Comment 3 "I agree that an effort such as this could contribute in potential to 'improve our understanding of where and how much countries could enhance their AFOLU ambition from what currently is reported', but there should remain a strong focus on decreasing the uncertainties in all methods applied. Although we do recognize the importance of contrasting NDCs to external independent emissions datasets, we do not necessarily imply that our data are too well fitted for this particularly purpose. Thus, countries report their INDCs based on net emissions while we report gross emissions. For those countries reporting sectorial targets, our emissions might help double check areas with large divergences between their intended targets and their historic gross emissions. In any case, our research should include a temporal component and move towards net emissions and decreased uncertainties, to be a more useful tool for that purpose.

Comment 4 "Perhaps a similar effort such as, or in cooperation with, the Global Carbon Project (GCP) should be established, in order to try to provide regular updates/improvements of this dataset. Also a direct comparison of the CO2 component with the GCP would enhance the credibility of this study". The idea sounds appealing, and we are opened to cooperation. However, we believe that at this stage we are not producing similar data, and the comparison of CO2 emissions with the GCP would prove difficult since our emissions are gross and tropical, while the GCP offers net and global.

Comment 5 "One questions remains whether this methodology could also be applied to the rest of the world to get a full global picture? If so, why didn't the authors do so? " The methodology can be applied to larger areas and multiple time periods, the problem is data. All the emission sources used in this research have global coverage except deforestation. This is the reason why we restricted to their study area (tropics and subtropics). So far, there are no global datasets on deforestation emissions out of the tropics. Hansen et al. (2013) have estimated deforested areas, but emissions are not yet available, although they are being produced. Temporal estimates would also be important, but datasets such as livestock only offered emissions for 2000. This research is a multi-step process. We hope to move towards net emissions, global and multitemporal.

Technical Remarks Page 2, line 52; the claim 'we offer a spatially detailed benchmark' gives the impression that spatial is always better than non-spatial? Seems a bit strong statement to me. We believe that the comparative advantage of this research is its spatially explicit nature, and the transparency of its methods and assumptions. It allows countries to check for subnational and regional emissions and detect which emission sources (eg. Deforestation, degradation, livestock, cropland soils, paddy rice) are behind the highest emission trends in which areas. Spatially explicit emissions offer a lot of interesting insights that non spatial results don't (e.g. interaction with climate, with socio-economic variables, etc)

Page 3, line 74, 75: The authors claim that reporting on a country scale is not adequate for implementation of mitigation measures. Why is that? National scale statistics on AFOLU emissions offer an aggregated view of a country's emissions that does not allow countries to identify which regions within their country need priority action, and which emission sources within each region are more important. Implementation benefits from more detailed spatial scales that allow untangling the sources and start checking the drivers, so that policies and measures can be effectively applied on the ground (e.g. how could a country with large deforestation emissions develop new policies and put mitigation action in place to stop deforestation if the regions where deforestation occur are unknown?)

Page 13, line 367 (Balch et al under review). In the reference list it says ' in press'. Many thanks. It is under review. Changed

Page 21, reference Le Querre et al is not correct, should be: Le Quéré, C., Peters, G.P., Andres, R. J., Andrew, R. M., Boden, T. A., Ciais, P., Friedlingstein, P., Houghton, R.A., Marland, G., Moriarty, R., Sitch, S., Tans, P., Arneth, A., Arvanitis, A., Bakker,

D. C.E., Bopp, L., Canadell, J. G., Chini, L. P., Doney, S. C., Harper, A., Harris, I., House, J.I., Jain, A. K., Jones, S. D., Kato, E., Keeling, R. F., Klein Goldewijk, K., Körtzinger, A.,Koven, C., Lefèvre, N., Maignan, F., Omar, A., Ono, T., Park, G.-H., Pfeil, B., Poulter, B., Raupach, M. R., Regnier, P., Rödenbeck, C., Saito, S., Schwinger, J., Segschneider,J., Stocker, B. D., Takahashi, T., Tilbrook, B., van Heuven, S., Viovy, N., Wanninkhof,R., Wiltshire, A., and Zaehle, S. (2014) Global carbon budget 2013, Earth Syst. Sci.Data, 6(1): 235-263, doi:10.5194/essd-6-235-2014. Many thanks. Changed.

Figure 2b depicts the uncertainties in AFOLU emissions and (coincidence or not) the regions with the highest emissions have also the highest uncertainty. What does this mean for the overall conclusion and robustness about the authors claim that this spatial explicit approach is better than the country level estimates from FAOSTAT en EDGAR, since the uncertainties are so high?

I do like figure 3, although the dark coloring makes it hard to distinguish details (and where is the dark color in the legend?) We tried several visualizations including white and greys, and black offered the best contrast. We have included the description of black in the legend caption, since the reviewer is right that not all readers are used to RGB visualizations.

Figure 2 and 4. The figure has a somewhat strange classification, based on what? The lowest values in blue are hard to distinguish. Perhaps introduce a separate category 0 (zero) with a different coloring (grey for example). Yes, we agree with the reviewer that several options could exist, depending what was the message we wanted to send to the reviewers. We have been trying several things all along, and these figures have gone through a lot of discussion! Thus, the story behind these two graphics is that we had to choose between 1. reinforcing the colouring of those areas with larger emissions for each gas independently (CO_2, N_2O and CH_4, and the aggregated CO_2e), or 2. choose a legend that allowed a comparative visualization of the emissions for the different gases. The first option would have immediately shown regions where the different gases were higher, but then their legends would not have been comparable (I personally preferred this option, since CH4 and N2O are not too visible now). Coauthors, however, agreed that a comparative visualization of the different gases was better. This is the reason for the strange legend categories (they represent the natural break points for the gas that has lowest emissions, N2O). We believe, however, that the readers do not need to learn from these details, so we have omitted explanations on this point. Greyish tones where proved got confused with yell

Figure 5. Do I interpret it correctly that India as a whole and Indonesia are hotspots for emissions, but India has also a low uncertainty in the estimate of those emissions and Indonesia is therefore much more uncertain ?Correct! More contextual information on this figure has been added in lines 330-350.

DETECTED ERRORS Table 2, on its section "gross AFOLU emissions (PgCO2e.yr-1), Monte Carlo results" has a small problem that requires small changes. The emission values offered in the first section of this table derive from the Monte Carlo simulations. Since two of our emission sources were not Gaussian variables, their assumed probability density functions did not match the data perfectly well, since we did not find a way to perfectly parameterize them. As a consequence, Monte Carlo mean values do not coincide exactly with the values obtained by directly summing the emission data (analytic sum) from the pixel scale to the appropriate scales. Table 2 has the MC results, but to be consistent with other manuscripts that we are about to submit on AFOLU datasets comparisons, we prefer to offer the analytic sums. This means a few small changes in the tables and in the text:

Line 336, 8.2 (5.5-12.2) PgCO2e.yr-1, changes into 8.0 (5.5-12.2) PgCO2e.yr-1 (line 355 in new version) Line 401, 5.7 (3.3-9.5) PgCO2e.yr-1 changes into 5.5 (3.3-9.5) PgCO2e.yr-1 (line 420 in new version) Line 402, 1.48 (1.1-1.9) PgCO2e.yr-1, changes into 1.5 (1.1-1.9) PgCO2e.yr-1 (line 421 in new version) Line 435, 2.8 (1.8-4.4), 2.8 (1.9-4.0), 2.6 (1.7-3.8) PgCO2e.yr-1, changes into 2.7 (1.8-4.5), 2.8 (1.9-4.0), 2.5 (1.7-3.8) PgCO2e.yr-1, for Central and South (CS) America, Africa, and Asia, respectively (lines 460-461 in new version) Lines 437-438, The original units and

values had an error. The proportion 3x1 of the result is maintained, but the absolute value were incorrect. The text has changed from: 'turn Asia into the largest continental source (0.9 TgCO2e.yr-1.ha-1) followed by Africa and CS America (0.39, 0.36 TgCO2e.yr-1.ha-1, respectively)' into 3.2 MgCO2e.ha-1.yr-1 followed by Africa and CS America, with 1.3 and 1.35 MgCO2e.ha-1.yr-1, each (lines 463-464 in new version)

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/bg-2016-99/bg-2016-99-AC1-supplement.pdf

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