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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.

Anonymous Referee #1

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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. 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

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organic carbon leading to more efficient water use and higher yields).

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.

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. 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.

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?

I concur with the authors that this approach is uncertain but can still be used to measure progress within countries/regions over time, rather than absolute reduction in the light of all uncertainties attached.

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.

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?

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

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.

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?)

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).

Figure 5. Do I interprete 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 ?

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