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Interactive comment on “The effect of land-use change on the net exchange rates of greenhouse gases: a meta-analytical approach” by D.-G. Kim and M. U. F. Kirschbaum

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Dear Reviewer for bg-2013-646:

Please accept our revised version of manuscript bg-2013-646 with the modified title “The effect of land-use change on the net exchange rates of greenhouse gases: a global compilation of estimates” (Please find the revised manuscript in the supplement). We also submit our detailed responses to your comments.

We thank you for their constructive suggestions that have substantially improved the manuscript.

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The main changes to the manuscript were:

1. Title was changed to “The effect of land-use change on the net exchange rates of greenhouse gases: a global compilation of estimates”.
2. Biomass carbon stocks (2.2.1 Quantifying changes in biomass carbon stocks) were newly determined using the information from FAO (2010), IPCC (2001) and WBGU (1998) and the newly applied methodology, news results and relevant discussion were incorporated in text, tables, figures, and references accordingly.
3. Results (3. Results) and Discussion (4. Discussion) sections were combined into a new section (3. Results and Discussion) and the discussion was enhanced throughout the manuscript.
4. Figure 2 was omitted since Table 6 provided very similar information already.

Further detailed responses to your comments have been given in the below message. Finally, we have acknowledged your constructive and valuable comments in the ‘Acknowledgments’ section.

This study has not been published and is not under review in any other journal or book. All authors have approved the manuscript and agree with its submission. We hope you share our enthusiasm for this study and consider it for publication in Biogeosciences.

Sincerely,

Dong-Gill Kim on behalf of all authors

Response to the reviewer 1’s comments

The very ambitious objective of this paper is to assess worldwide greenhouse gases emissions arising from land-use change between 1765 and 2005. The estimates are based on extremely limited literature reviews of phytomass C stocks and soil changes

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in emissions of N₂O and CH₄. Soil C stock changes are assessed combining results from previous meta-analysis studies. The study is entitled meta-analysis although the approach used for quantifying CO₂ emissions resulting from phytomass and soil C stock changes and soil N₂O and CH₄ flux changes is not a meta-analysis. The authors do not apparently understand the concepts of a meta-analysis statistical approach and should hence rename their study.

Response: We recognise that our study does not constitute a formal ‘meta-analysis’ and therefore did not claim that term for our work. However, even our term ‘meta-analytical approach’ might have implied a more rigorous statistical treatment than we used in this work and we therefore have changed the title to: “The effect of land-use change on the net exchange rates of greenhouse gases: a global compilation of estimates”

The review on phytomass C stocks is not only limited but erroneous. At a time when the scientific research is moving from global estimates to more accurate regional or country-specific estimates, this paper does the contrary by calculating an average worldwide C stock for some land-use types, in particular forest.

Response: We believe that there is a need for both approaches. There is certainly value in providing region-specific information, and the increasing availability of more sophisticated data bases makes it possible to make advances in that area. Nonetheless, we believe that there is also value in providing some averaged responses of the change in greenhouse gas (GHG) emissions for the broad range of land-use change transitions. What are the quantitatively most significant transitions, and what gases make the biggest contributions to those overall changed GHG fluxes? We consider that it is valuable to have some information about the average response across the globe. That is why other compilations, like those of the IPCC that we have based our biomass estimates on, also use broad categories similar to the ones used here.

Land-use classification is not clear. The definition given to secondary forest seems to

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include mono-specific tree plantations but the assumption made for assessing biomass C stocks in this category doesn't consider the low C stocks and short rotation time of some mono-specific plantations.

Response: These points are valid, but in aiming to provide a comprehensive assessment of the change in all GHGs and for a whole range of land-use transitions, it was not possible to also go into great detail and depth in any of the specifics that contributed to the whole. Much of those details are treated more comprehensively by the various studies that we obtained our data from, but, as for our response to the previous, other compilations, like those of the IPCC also use broad categories similar to the ones used here.

CO₂ emissions from the soil are estimated using differences in C stocks but neither the soil depth considered nor the method for C stock calculation (bulk density correction after land-use change) are specified.

Response: Reported stock changes were the average of whatever was presented in different research studies, and to whatever depth it was measured in those studies. These differences between different studies clearly cause some problem but are unavoidable while there is no universally agreed standard methodology for assessing soil carbon changes. However, for percentage changes, some of these differences between studies might have cancelled out. We have added some extra text to the manuscript to draw attention to some of those issues.

The review on soil flux changes of CH₄ and N₂O is extremely limited, with most cases based in China and Australia. There is much more data available in the literature.

Response: We recognize that there are many papers reporting greenhouse gas (GHG) emission in various ecosystems and land use types. However, in our literature search, we did not actually find many research studies that met our inclusion criteria – that is having reported the effects of land-use change on GHG emissions in paired study sites in the field with different land-use types and providing at least annual GHG emis-

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sion estimates. That means that short term studies or laboratory incubations were not included. For these reasons we included fewer papers than the reviewers perceived that should have been available. There was certainly no intent to deliberately select papers reporting data from specific regions, and most publications were found through searches in the international literature. If some regions appear to be more frequently represented than others, it probably represents the nature of current GHG research spurred by greater concern about land-use change and GHG emission in some regions than in others.

Phytomass CO₂ changes from land-use change are assessed as a simple difference in C stocks between land use categories and this is by no means a meta-analysis. The effect of land-use change on soil C stock changes is calculated by developing relationships between time after conversion and differences in soil C stocks. Again, this isn't a meta-analysis statistical approach. The effect on soil emissions by land-use change type is calculated as a simple average of the study cases so this is not a meta-analysis.

Response: We accept that the title inappropriately implied that our study was a meta-analysis. We therefore have changed the title to: "The effect of land-use change on the net exchange rates of greenhouse gases: a global compilation of estimates"

The study fails to differentiate rice fields, which are strong CH₄ emitters, from croplands. It also fails to include the high emissions resulting from fires which are very common in the Tropics when land-clearing.

Response: We did not consider rice paddies as croplands because they are subject to fundamentally different processes, especially being sub-merged for extended periods, which leads to very large CH₄ emissions. We have now included an extra sentence in the Methods (2. 1. Types of land-use change assessed in this study) to clearly state that. We have included the role of fire in the text, but it is also another one of the issues that could not be treated comprehensively. There is a limit to the number of important

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issues that can be treated in a single publication.

Finally conversion effects are evaluated over a 100 year conversion period; which is an arbitrary period of time chosen by the authors without any scientific background supporting this choice. Land-use change modelling scenarios taking into account the different rotation or land-use change periods would be the only way to accomplish such an objective.

Response: Yes, it is an arbitrary accounting period – that is a point we ourselves tried to make in the text: “A problem arises in that carbon-stock changes are one-off carbon-stock changes whereas changes in the flux of the other GHGs constitute on-going changes. To bring these changes to common units, we chose to analyse the changes over a time frame of 100 years, as this is a commonly used time frame in GHG accounting, such as in the calculation of global warming potentials (GWPs). However, there is no substantive reason for choosing a 100-year calculation interval rather than any other.” The choice of accounting period is thus not a scientific question, but it links the science of on-the-ground observations with the needs of GHG accounting, which introduces completely different considerations. They can on one level be considered as arbitrary, but we believe that this has been handled and described appropriately within the present context.

I would highly recommend the authors to downgrade their goal and limit their study to Australia or Asia for instance where they seem to have a better knowledge on the parameters involved.

Response: The whole purpose of the present work was to provide an averaged global assessment. The SOC data were certainly globally representative. Biomass and enteric methane emissions were also derived from global estimates of the relevant quantities. Hence, there are no regional biases in those. Estimates of changes in CH₄ and N₂O emissions were based on published literature found through global literature searches. If any regions were over- or underrepresented, it would have not been due

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to our biases in preferentially selecting data from some regions rather than others.

Specific comments

p2, l 9-10 “by averaging stock changes over 100 yr” Is this representative of land-use change transitions worldwide?

Response: As stated in a previous comment, the use of 100-year assessment periods was not a scientific assessment trying to be ‘representative of land-use change transitions worldwide’. It was a necessary step to link stand-level activity data with a metric that is relevant for assessing the important of net GHG emissions.

P2, l 17-18: “Land-use change impacts were generally dominated by changes in biomass carbon.” Which percentage?

Response: We revised the sentence as below: In all land-use changes involving forests, changes in biomass carbon dominated the overall change in net GHG emissions.

P2, l 18-21, “A retrospective analysis indicated that land-use change from natural forests to agricultural lands contributed a cumulative 1326449 GtCO₂ eq between 1765 and 2005” Can we really extrapolate back to 1765 using data from 1950-2000? (Change in climate? Representativeness?)

Response: We do not understand what the reviewer means by ‘data from 1950-2000’. We did not use any data from 1950-2000. The retrospective analysis was based on the work of Meiyappan and Jain (2012) who used historical reconstruction methods to estimate the extent of deforestation between 1765 and 2005. We consider that to be the appropriate data source for the present analysis and are unsure what criticism the reviewer intended to make.

P3, l 10-11, “Annual mean global C emissions from land-use change were estimated to be 1.1GtCyr⁻¹ (Houghton et al. 2012)”. biomass C change only or also soil emissions of GHG? Are we talking about forest conversion or other land-use change types? What

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surface of land-use change does this study take into account? Might be better to express the results in CO₂eq to be able to compare with your own estimation.

Response: We agree with the reviewer that the number should be more consistently expressed as CO₂eq in the context of the present paper and we have recalculated it in the revised version of the manuscript. Annual mean global C emission of 1.1GtC yr⁻¹ was reported in Houghton et al. (2012) summarizing thirteen recent estimates of net carbon emissions from land use and land cover change. Houghton et al. (2012) described the estimates as below:

“In addition to deforestation, all analyses considered changes in the area of agricultural lands (croplands and pastures). Some considered, also, forest management (wood harvest, shifting cultivation). None included emissions from the degradation of tropical peatlands.”

P3, l 16-20, “While the changes in SOC following land-use change are mainly attributable to shifts in the balance between carbon-input rates and specific decomposition rates of organic matter (e.g., Murty et al., 2002; Guo and Gifford, 2002; Don et al., 2011), soil erosion may also play a role in erosion-prone landscapes (e.g., Lal, 2003; Post et al., 2004; Gaiser et al., 2008).” What about the effect of fires which are very common during land-use change transitions? The effect on soil CO₂ losses can be massive particularly in tropical peatlands.

Response: We added the effect of fires in the sentence as below:

“While the changes in SOC following LUC are mainly attributable to shifts in the balance between carbon-input rates and specific decomposition rates of organic matter (e.g., Murty et al., 2002; Guo and Gifford, 2002; Don et al., 2011), soil erosion may play a role in erosion-prone landscapes (e.g., Lal, 2003; Post et al., 2004; Gaiser et al., 2008) and, where fire is associated with LUC, it may contribute to the changes also deplete SOC stocks (e.g., van der Werf et al., 2006 and, 2010).”

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We also additionally briefly discussed issues of draining wetlands and fire in Section 3.5. Draining wetlands.

P3, l 20, What about the effect of land-use change on biomass C?

Response: We considered 5 key net GHG emissions (biomass C, soil organic carbon, enteric methane emissions, net soil methane emissions and soil N₂O emissions). So, we obviously did consider biomass C changes and highlighted it in the Abstract as ‘dominant changes’. We are unsure what else the reviewer means with that question.

P3, l 21-22, “The effect of land-use change on CH₄ fluxes is related to both processes in the soil and enteric fermentation by grazing animals” & p 4, l 3-4. “that are likely to dominate the overall change in net CH₄ emissions” What about land-use change effect in wetlands, CH₄ emissions in paddy fields and those from fire occurring during land-use change transitions?

Response: The reviewer is correct in pointing out that the dominant role of enteric fermentation does not cover the cases of conversions involving rice paddies or natural wetlands. This sentence has been changed to reflect that exception, and we have added clear statements to make it clear that conversion to or from wetlands are not being covered in the present study.

P4, l 21-23. “However, we are not aware of any previous comprehensive and quantitative summary reports that have combined the effect of land-use change on changes in biomass C, SOC, CH₄ and N₂O fluxes.” The reason is probably that to make such an assessment in a rigorous manner; one should proceed to in-depth meta-analysis of all factors mentioned taking into account all sources of GHG, including fires. Doing so for worldwide land-use changes is a huge work!

Response: We agree that a global analysis is a huge task, and we would not claim that the present study represents the culmination of all work that should be done in this area. It is a first comprehensive assessment that has not previously been published,

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and we hope that it can provide a valuable stepping stone. The reviewer has pointed out some short-coming in our analysis that we are largely aware of. We hope that future work can take the first stepping stone of our work and build on it by providing more refined estimates of some of the quantities that we were most uncertain of.

P5, l 17-19, “Secondary forests can be local indigenous forests that are naturally regenerating or forests planted for specific human purposes, and they may include indigenous or introduced species.” Are mono-specific tree plantations such as Eucalyptus, rubber or Acacia plantations included in this category? Please specify.

Response: This definition includes Eucalyptus, rubber and Acacia plantations – this should be clear from the stated definition. And, yes, it covers a wide range of possible forest types, including the short-rotation Acacias and longer-growing pine forests referred to in other comments. This is both the strength and weakness of a global study.

P6, l 1-3, “without being able to provide any weighting by either the areal extent of different bioclimatic zones of each vegetation type or the global distribution of different land-use changes.” How were then the estimate in the abstract made?

Response: This statement states that we had to use global biomass averages for our estimates of the biomass changes involved in land-use changes. The numbers in the Abstract and everywhere else in the paper are thus based on these average values. This is clearly explained in the Methods, and we are unsure what else the reviewer meant with that comment.

P 6, l 6-8, “The impact of land-use change on net GHG exchange was determined through quantifying changes in biomass C, SOC, CH₄ production through enteric fermentation, and net soil fluxes of CH₄ and N₂O.” Important omissions are GHG emission from fires which are part of land-use change transitions across the Tropics; CH₄ emissions by plants (cf. rice fields).

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Response: We recognise that fire can emit various gases including the GHG CO₂, CH₄, and N₂O. C loss through CO₂ emissions from fires is included in change in biomass C and SOC following land use change. However, it was not possible to quantify fire-originated CH₄ and N₂O emissions associated with land use changes since we are not aware of any global data quantifying the extent of fire associated with various land use changes. With respect to CH₄ emissions by plants, we assume that the reviewer refers to the fact that rice plants can act as conduits for CH₄ production in the root zone to reach the atmosphere. This is probably an important specific process, but, as for fire and as stated above, this paper could not cover everything that might be important and conversions involving rice paddies have now been specifically excluded from the present paper. Some of these important missing issues have now been added to ‘3. 2. Changes in soil organic carbon stocks following land-use change’, 3.5. ‘Draining wetlands’, and ‘3.6. Implication and suggested future studies’.

P6, l 15-17, “Global average biomass C stocks in natural forests were estimated to be 118 ± 39 tC ha⁻¹ (Mean 95% confidence intervals; taking the average of biomass carbon in Table 1 in Kirschbaum et al., 2012).” This default value poses several problems. First I believe there are strong differences in forest C stock across the world and a single forest world category doesn’t make sense. I doubt the CI of this world forest category to be as low as 39! Have a look at the variation for SE Asia in the paper of Ziegler et al. (2012) ‘Carbon outcomes of major land-cover transitions in SE Asia: great uncertainties and REDD+ policy implications’. Second, Table 1 in Kirschbaum et al., 2012 is questionable: - Tropical forest C stocks are estimated as twice the amount of living above-ground biomass reported by Houghton (2005) to account for roots, dead wood and forest-floor carbon stocks. Houghton (2005) already accounted for roots and calculated forest C stocks in the 3 tropical regions from country means weighted by forest area. - The weighting of Houghton (2005) doesn’t appear in the study of Goodale et al. (2002) used for the stocks in Canada, US, Europe, Russia and China. - I don’t believe exotic pine stands of New Zealand can store 1.6 times more carbon than tropical forest of Asia. - There are missing areas of the world in this table: what about non-tropical

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Asia and Africa? And Central America?

Response: We acknowledge the validity of the issues raised by the reviewer and have now recalculated the numbers for biomass carbon based on information available in the FAO (2010) publication. These new data sources are global and should avoid any regional biases the reviewer was concerned about. The description and results of recalculation are reported in “2. 2. 1. Quantifying changes in biomass carbon stocks”.

P6, I 17-19, “We assumed that 75% (89 ± 29 tC ha⁻¹) of biomass C stocks in natural forests can accumulate in the biomass of secondary forests over 100 yr.” Is there any study supporting this assumption? Given the vague definition given to secondary forest I’m afraid this can’t be true; especially if mono-specific tree plantations are included there. For instance the time average C stock in an Acacia plantations is < 30 Mg C ha⁻¹, and its rotation time is < 10 years.

Response: The estimates for both natural and secondary forests have now been recalculated based on the numbers provided in WBGU (1998) and FAO (2010). The ratio of biomass contained in natural and secondary forests is now based on data compiled for WBGU (1998). The description and results of recalculation are reported in “2. 2. 1. Quantifying changes in biomass carbon stocks”.

P6, I 19-21, “Biomass C stocks in cropland, grassland and savannah (Table 1) were determined from estimates of global vegetation C provided by Eglin et al. (2010) divided by the area estimates of Ramankutty et al. (2008)” Eglin et al. (2010) used C stock values from IPCC (2000), so I don’t understand why the approach is so complicated here. Why don’t you just simply use IPCC values? Also the stock in the forest accounts for roots, dead wood and litter; is it also the case for croplands, grasslands and savannahs?

Response: As the reviewer suggested, we have now directly used biomass C stocks (including both shoot and root) in cropland and grassland provided by IPCC (2001; Table 3.2). We added the information in “2. 2. 1. Quantifying changes in biomass

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carbon stocks”. IPCC: Climate Change 2001: The Scientific Basis. Contribution of Working Group I to the Third Assessment Report of the Intergovernmental Panel on Climate Change [Houghton, J.T., Y. Ding, D.J. Griggs, M. Noguer, P.J. van der Linden, X. Dai, K. Maskell, and C.A. Johnson (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 881pp, 2001.

P7, I 1-15, Is this a discussion? Why do we have this in the method section? If you want to test your assumption you should probably go back to the source documents i.e. check which studies were used to derive the IPCC (2000) C stock/stock change values.

Response: We have moved this section to the new Results and Discussion section as suggested.

P7, I 20 and Table2 Why can savannah only be converted to grassland? Can't we have savannah conversion to cropland? Wetland: no data available? For sure there are data available!

Response: The impact of LUC on net GHG exchange was determined through quantifying changes in biomass C, SOC, and net soil fluxes of CH₄ and N₂O. In the case of land-use changes involving wetlands and savannahs, we did not have access to all the required data that would have been needed to comprehensively determine the impact of LUC on net GHG exchanges. Therefore, we have removed wetland and savannah from Tables 1, 2 and 5 and made it clear in the text that this review does not cover LUCs involving wetlands or savannahs.

Why is the 'contribution to the atmosphere' evaluated over a 100 year period? Forest conversion to cropland takes place in a year or so and it's a bit difficult to assume that the cropland will remain cropland for the following 99 years. On the other hand cropland conversion to secondary forest (excluding short-time rotational mono-specific plantations) may indeed take 100 year. I'm afraid this type of long-term calculation needs land-use change scenario modeling.

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Response: Using a 100-year accounting period is not based on assessment of the future land use and when it might revert back to its former land use, or any such assumption about its actual future. Instead, it presents a link between stand-level events and resultant net emissions, and the consequent effects on the atmosphere. The accounting options are to either assume that the new land use remains indefinitely, and apportion the C-stock change over a standard accounting period, or do some detailed land-use modelling as implied by the reviewer. However, that latter approach would rely on assumptions about future global food demand, technological changes and land capability. It is not certain whether such modelling would be useful (in the present context) and certainly would be beyond the scope of the present study.

P7, l 22-29, As underlined above this is a major issue in the approach. You should probably quantify the C stock change over the actual conversion period, and then calculated the amount of non CO2 gases released during the same period.

Response: As stated in response to previous comments of the reviewer, we believe that the 100-year averaging approach is the appropriate approach in the present context.

P 8, l 1-2, “Conversely, shortening the integration interval would have increased the inferred importance of carbon-stock changes” This isn’t a scientifically sound justification for choosing 100 years.

Response: This statement is not meant as a justification, but simply to provide information to the reader on to the way that changes in the integration interval would affect the numeric outcome.

P8, l 15, “Si are the average pre- land-use change soil-organic carbon stocks” Which depth?

Response: As stated previously, in our analysis, we had to use the measurement to different depths as had been used in different cited studies. These differences between studies indeed cause problems, but they cannot be avoided in the absence of

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an agreed international default methodology. However, the problem might not been too serious as stock changes would normally be concentrated in the upper part of the profile so that studies that only looked at the top of the soil might have reported a larger change but a smaller stock, and soils with measurements to deeper layer might have been reported as greater stock but smaller relative change. Since we are interested in the stock changes, these differences might have cancelled out.

P8, l 16, “ $\dot{E}Si_j(100)$ is the fractional SOC change estimated over 100 yr”. Do you mean SOC content change or SOC stock change? How was the SOC stock calculated? Was there a correction made for bulk density change?

Response: A fractional SOC change was used whether authors of the original studies measured their changes as a change in C concentration or C stocks. It is correct that bulk density changes confound reported fractional changes and ideally, analyses would include only observations with appropriate bulk-density correction, but restricting the available data in that way would significantly reduce the number of available observations. Fortunately, bulk-density changes lead to errors in opposite directions for studies based on C concentration measurements and C-stock measurements. For measurements based on concentration changes, increasing bulk density leads to apparent reductions in C concentrations because of the dilution of samples by soil from deeper in the profile. For C-stock based measurements, increasing bulk density increases stocks because more mineral soil is included in the accounting if bulk density increases. While all studies without adequate bulk-density corrections are individually wrong, the errors in opposite directions partly cancel each other out. Murty et al. (2002) separately calculated fractional SOC changes after deforestation, and found that bulk-density corrected data were not very different from the mean of all data, probably reflecting the compensating errors in studies based on concentration and stock changes. We therefore believe that inclusion of all data, as was done in our analysis, did not unduly bias the results but a sentence has been added to indicate that data without bulk-density corrections had been included in the study in section 2.2.2.

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P8, I 18-21, “The SOC in land prior to land-use change and the change rates (\dot{E}) of SOC in various types of land-use change (Table 3; Fig. 1) were obtained by combining the global meta-data of Murty et al. (2002), Don et al. (2011), Poeplau et al. (2011), and Power et al. (2011) that include over 230 studies published from 1963 to 2010.” It would be adequate to make this updated database available online.

Response: We obtained data and permission to use in the study from the authors and acknowledged them for sharing their data. However, making the data available online has not been discussed with the owners of the data. Therefore, it would be inappropriate to make the data available online at this stage.

The calculation of soil C stock changes using relationships between time after conversion and differences in soil C stocks isn't a meta-analysis.

Response: We recognize this and have modified the title of the study as mentioned above.

P10, I 20, “2.2.4 Quantifying changes in soil CH₄ and N₂O emissions” What about CH₄ emissions through plants? Rice fields, for instance, are considered as croplands in this assessment, aren't they? Rice fields are known to emit substantial amounts of CH₄. P10, I 25-27,

Response: We acknowledge that the reviewer is correct in that assessment, but because it would raise a range of other issues that would have gone beyond the scope of the presents study, have made it clear now that land-use changes involving rice paddies are not considered as part of the present study.

“We compiled CH₄ (n = 34) and N₂O (n = 37) emissions data obtained from paired study sites with different land-use types (Tables A to E).” I'm afraid there are much more studies available in the scientific literature. Most of the studies are cases from China, Australia and New Zealand. Studies on forest conversion to croplands are limited to China and Australia, there are plenty of other cases elsewhere.

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Response: As already stated in a response to an earlier comment, the reviewer is correct in stating that there are many papers that have reported greenhouse gas (GHG) emission in various ecosystems and land use types. However, we could not actually find many research studies that have reported how land-use change affects GHG emissions based on paired study sites with different land-use types conducted in the field and obtaining GHG emission estimates for at least one full year. That means that short term studies or laboratory incubations were not included. For those reasons our study included fewer papers than the reviewers perceived should have been available. There was certainly no intent to deliberately select papers from specific regions.

Which metric did you use for quantifying the magnitude of the effect on the emissions? Have you used a random effect model for assessing the effect by land-use change category? It looks like the effect by land-use change type was calculated as a simple average of the values obtained for each case; this is by no means a meta-analysis approach.

Response: As mentioned before, we recognize that our approach constitutes no formal meta-analysis and have avoided use of the term such as in a modified title of the study.

P 12, l 1-12, “2.3 Global estimate of total historical net greenhouse gases contribution by land-use change” There is already so much uncertainty on the results due to the lack of a proper meta-analysis approach, very partial literature review on phytomass C stocks and soil GHG fluxes, absence of land-clearing fire effects on greenhouse gases emissions, lack of disaggregation by climatic zones, lack of consideration of rice fields separately from croplands, etc. Adding that trace gas emissions are known to be affected by climate, nutrient availability, etc. makes this extrapolation in time completely inaccurate. I would highly suggest to remove it.

Response: While it is true that there is a degree of uncertainty in our LUC estimates, we do nonetheless consider these estimates to be useful estimates of global average GHG emissions associated with LUC. If that is correct then it would also make sense

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that there is value in scaling these stand-level estimates by the area that globally has undergone LUC. We believe that we are providing a useful global average here of GHG emissions that have historically been associated with LUC. We see these estimates as a starting point that can be refined through further work in future.

P12, I 15, “calculating the means and standard errors” This is confusing as if we look at Table 1, the footnotes indicates Means 95% CI. Use one metric or another.

Response: That was an oversight and error left over from an earlier draft of the document. ‘Standard error’ has been changed to ‘95% confidence intervals’.

P12, I 23, Please explain if the higher the EF the better the model and provide EF ranges for low medium and good fitness of the model.

Response: Readers not familiar with the concept of model efficiency are referred to Nash and Sutcliffe (1970), who are usually credited with having first popularised this statistical test of the goodness of fit, or to other statistical text books to obtain more information about model efficiencies.

P 13, I 7-9, “Uncertainty in the estimates of other quantities was assessed by treating the numbers reported in different studies as independent observations and calculating 95% confidence intervals of those observations.” Does this mean that the error associated with each study wasn’t propagated along the calculation?

Response: That is correct. Hence, as responded to similar comments above, we recognise that the present study is not a statistical meta-analysis, and we have refrained from using any terms suggesting that it is.

Results I won’t comment on the results or discussion as the methods adopted in this study are not appropriate for such an assessment. I highly encourage the authors to change their methods, deepen their literature review and focus on a much smaller geographical area of observation.

Response: We are not sure what this sweeping criticism of our methods is based

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on. The reviewer has raised a number of specific issues, which we have, we believe, adequately responded to. With the changes that were made, and clear and transparent omissions of some issues that were considered to be outside the scope of this analysis, we believe that our approach is now adequate and appropriate for the task. We have also responded to the claim that our data compilation is in any way biased. This is not warranted as all the information presented here was sourced through the general international literature. There would thus be no rationale for limiting the regional scope of our analysis, and it would, in fact, undermine its key purpose. There seems to be no point in doing that.

Supplement Table B, new category “Agroforest”, in which of the defined LU categories does this one fall? Table E, same comment as above for wetlands

Response: That term was accidentally included in the manuscript and has been removed.

Technical corrections": typing errors, etc.

p2, l14, Replace “net fluxes” by “net emissions” p2, l16, Replace “reduced net emissions” by “led to net uptakes”

Response: Changed as suggested.

p2, l 25-26, Rephrase “but land use change (LUC) also contributes an important net greenhouse gas (GHG) exchange”

Response: Changed to: “but land-use change (LUC) also leads to important additional greenhouse gas (GHG) exchanges.”

p3, l 15-16, Suppress “since the loss of land-based C stocks increases atmospheric CO₂”

Response: Changed to “since any loss of biospheric C stocks increases atmospheric CO₂”

P3, l 21-22, Rephrase “The effect of land-use change on CH₄ fluxes is related to both processes in the soil and enteric fermentation by grazing animals.”. Which are “both” processes?

Response: We had hoped that this would have been clear. Enteric fermentation is one process and soil processes that produce or consume methane are the other. However, to avoid future misunderstandings, this has been changed to: “The effect of land-use change on CH₄ fluxes is related to enteric fermentation by grazing animals and any soil processes that produce or consume CH₄.”

P 10, l 3, Missing reference “Clark et al., 2005”

Response: Clark et al. (2005) is already in the reference list.

P 11, l 21, “After converting the specific activity data of all GHGs” These aren’t activity data but emission factors.

Response: As suggested, we have changed this sentence to :”After changing the changes in net emissions of all GHGs to the same units. . .”

P28, l 26, bibliographic reference should probably be “Kelliher, F. and Clark, H.: Ruminants In Methane and Climate Change, edited by: Reay, D., Smith, P., and van Amstel, A., Earthscan, 136–150, 2010.”

Response: This is exactly what the reference already says. We are unsure what point the reviewer is trying to make by stating what the reference should say when it already does say exactly that.

P 36, Table3, Please provide CI for the estimated coefficients

Response: The Table already gives the confidence intervals.

P 37, Table 4, The legend of the Table is too long. What is the depth of soil C stocks?

Response: The question of the depth for measuring soil C stocks has already been

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addressed and answered above. As to the length of the Legend, we do not consider it to be excessive and instead consider it important to clearly convey to readers the details of what is being presented in respective tables.

P40, Figure 1, please enlarge the figures as it's extremely difficult to read them. What is exactly plotted there? SOC content change or SOC stock change? Please provide a formula in the legend or in the text.

Response: We have sympathy with that view, but this technical issue is beyond our control. The figure should have been printed to the full page width, and the data would have been more readily discernible. The formula corresponding to these data have already been provided in Table 3.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/11/C2227/2014/bgd-11-C2227-2014-supplement.pdf>

Interactive comment on Biogeosciences Discuss., 11, 1053, 2014.

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