

# ***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***

## **Anonymous Referee #1**

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### General 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 in emissions of N<sub>2</sub>O and CH<sub>4</sub>. 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 CO<sub>2</sub> emissions resulting from phytomass and soil C stock changes and soil N<sub>2</sub>O and CH<sub>4</sub> 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. The review on phytomass C stocks is not only limited

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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. Land-use classification is not clear. The definition given to secondary forest seems to 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. Phytomass CO<sub>2</sub> 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. CO<sub>2</sub> 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. 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 review on soil flux changes of CH<sub>4</sub> and N<sub>2</sub>O is extremely limited, with most cases based in China and Australia. There is much more data available in the literature. 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. The study fails to differentiate rice fields, which are strong CH<sub>4</sub> emitters, from croplands. It also fails to include the high emissions resulting from fires which are very common in the Tropics when land-clearing. 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 modeling scenarios taking into account the different rotation or land-use change periods would be the only way to accomplish such an objective. 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.

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

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

P2, l 18-21, “A retrospective analysis indicated that land-use change from natural forests to agricultural lands contributed a cumulative  $1326 \pm 449 \text{ GtCO}_2 \text{ eq}$  between 1765 and 2005” Can we really extrapolate back to 1765 using data from 1950-2000? (Change in climate? Representativeness?)

P3, l 10-11, “Annual mean global C emissions from land-use change were estimated to be  $1.1 \text{ GtCyr}^{-1}$  between 1980 and 2000 (Houghton et al., 2012)” Does this estimate take into account C loss from biomass C change only or also soil emissions of GHG? Are we talking about forest conversion or other land-use change types? What surface of land-use change does this study take into account? Might be better to express the results in  $\text{CO}_2 \text{ eq}$  to be able to compare with your own estimation.

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  $\text{CO}_2$  losses can be massive particularly in tropical peatlands.

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

P3, l 21-22, “The effect of land-use change on  $\text{CH}_4$  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  $\text{CH}_4$  emissions” What about land-use change effect in wetlands,  $\text{CH}_4$  emissions in paddy fields and those from fire occurring during land-use change transitions?

P4, l 21-23. “However, we are not aware of any previous comprehensive and quantita-

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tive summary reports that have combined the effect of land-use change on changes in biomass C, SOC, CH<sub>4</sub> and N<sub>2</sub>O 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!

P5, I 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.

P6, I 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?

P 6, I 6-8, “The impact of land-use change on net GHG exchange was determined through quantifying changes in biomass C, SOC, CH<sub>4</sub> production through enteric fermentation, and net soil fluxes of CH<sub>4</sub> and N<sub>2</sub>O.” Important omissions are GHG emission from fires which are part of land-use change transitions across the Tropics; CH<sub>4</sub> emissions by plants (cf. rice fields).

P6, I 15-17, “Global average biomass C stocks in natural forests were estimated to be  $118 \pm 39$  tCha<sup>-1</sup> (Mean  $\pm$ 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

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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 Asia and Africa? And Central America?

P6, l 17-19, "We assumed that 75% ( $89 \pm 29$  tCha<sup>-1</sup>) 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<sup>-1</sup>, and its rotation time is < 10 years.

P6, l 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?

P7, l 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.

P7, l 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! Why is the 'contribution to the atmosphere' evaluated over a 100

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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.

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.

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.

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

P8, l 16, "ÉSi 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?

P8, l 18-21, "The SOC in land prior to land-use change and the change rates (É) 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. The calculation of soil C stock changes using relationships between time after conversion and differences in soil C stocks isn't a meta-analysis.

P10, l 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.

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P10, I25-27, “We compiled CH<sub>4</sub> (n = 34) and N<sub>2</sub>O (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. 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.

P 12, I 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.

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

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.

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?

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

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to change their methods, deepen their literature review and focus on a much smaller geographical area of observation.

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

Technical corrections”: typing errors, etc.

p2, l14, Replace “net fluxes” by “net emissions” p2, l16, Replace “reduced net emissions” by “led to net uptakes” p2, l 25-26, Rephrase “but land use change (LUC) also contributes an important net greenhouse gas (GHG) exchange” p3, l 15-16, Suppress “since the loss of land-based C stocks increases atmospheric CO<sub>2</sub>” P3, l 21-22, Rephrase “The effect of land-use change on CH<sub>4</sub> fluxes is related to both processes in the soil and enteric fermentation by grazing animals.”. Which are “both” processes? P 10, l 3, Missing reference “Clark et al., 2005” P 11, l 21, “After converting the specific activity data of all GHGs” These aren’t activity data but emission factors. 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.” P 36, Table3, Please provide CI for the estimated coefficients P 37, Table 4, The legend of the Table is too long. What is the depth of soil C stocks? 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.

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