

## ***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

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Response to the reviewer 3’s comments

This study investigates the impact of land use change (LUC) on greenhouse gas emissions based on a compilation of published studies. The analysis presents global averages of LUC effects with the major finding being that LUC impacts were dominated

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by changes in biomass carbon. This study is ambitious and potentially very valuable. However, in my opinion, the analysis is limited by a number of weaknesses and assumptions in the methodology and the results provide little new insight, especially when considering the large uncertainties and biases involved. My major concerns are: 1) I do not consider the database to be global. The data is heavily biased towards individual regions. The forest to grassland transition effect is based on studies in China and Australia while cropland to grassland/secondary forest is based on studies in Europe/USA. Secondary forest to cropland conversion effects are based on one single study for which interestingly a mean value was computed! The effect of wetland drainage is based solely on tropical wetlands, while information existing in the literature on peatland to forest conversion in Europe was completely ignored. Moreover, the lack of clearly defined land use types weakens the data set. For instance, does secondary forest include both naturally recovered forest and plantation forests? Thus, the data set is clearly too limited to speak of a global data set and the regional bias introduced does not allow to compute a meaningful global average.

Response: For our study, we compiled all available data that were found in the international literature and that met our selection criteria. There may have been some papers that were not incorporated because the provided data was not suitable for the purpose of this study. We only used field-based comparisons of paired sites with different LUC covering at least one full year of measurements. The included papers were the only ones found through the international peer-reviewed literature, and there was certainly no intent to exclude data from any particular region. We recognise that the GHG consequences of draining wetlands are highly complex and involve important changes in biomass, carbon stocks and in on-going fluxes of methane. To do justice to conversions from wetlands would require a whole dedicated study on its own. We therefore did not include it in our summary tables and figures. We have now also excluded it from Tables 2 and 5 to make it clear that this study does not aim to deal with the multitude of issues that are important in the conversions from wetlands. However, because it is such an important land-use change, we felt it important to include it in at least a short

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paragraph in the Discussion to point to its importance while making it clear that the present study is not able to provide any important new data.

2) Apart from the lack of data for computing global averages, I also question the usefulness of global averages as main result. I think it is more interesting to investigate how LUC effects vary for the same land use pair in different climate zones and site conditions rather than merging all into one mean observation that has little value for specific regions. The authors acknowledge the large variability within the data set but choose to simply ignore it. Meanwhile it could be an interesting to understand what factors are driving this variability.

Response: We agree that it would indeed be interesting to identify differences between climatic zones or site conditions. However, the reviewer him/herself already described the study as ambitious, and these further trends and details were beyond the scope of the present study. We also did try to analyse the available data for any trends with region, temperature or rainfall, but there were too few data to draw upon, and no significant patterns emerged. Other past studies looked for regional patterns in some individual factors, such as Kindermann et al. (2008) who developed regional patterns of forest biomass, or de Grosse et al. (2001), who developed a global map of N<sub>2</sub>O fluxes, but no previous studies brought these various factors together into a combined assessment. While being beyond the scope of the present study, we do agree with the reviewer, however, that it would be useful if a more regionally segregated assessment could be developed through future work.

3) The result section does not deliver much new insight in its current form. It is well known from previous studies that changes in biomass are a major driver of LUC effects.

Response: It is known from previous studies that changes in biomass are important during land-use change, but we are aware of very little previous work that would have looked for relativities in the different GHGs. Yes, the loss of biomass is important when forests are deforested, but is it more important than changes in SOC or on-going

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changes in methane emissions? Those are the questions the present work sought answers for. Overall, we believe that the main advance of the present study is bringing the main factors together into a single combined assessment.

Numerous inventories and synthesis studies on SOC exist in the literature whose findings are merely repeated and cited in this study. Fertilizer effects on N<sub>2</sub>O emission are also known. Instead, the importance of the contribution from CH<sub>4</sub> and N<sub>2</sub>O exchange for instance, which is touched on in Table 6, would be much more interesting to elaborate on.

Response: We agree that valuable prior studies of changes in SOC have been carried out. We are not claiming that the present work could substantially improve on those studies per se. But, as for our response to the previous point made by the reviewer, the aim of the present work was to bring these various isolated assessments together into a combined assessment. This is what Table 6 aimed to, but we are unsure what kind of elaboration the reviewer is suggesting. Table 6 already provides the relativities between the importance of the different gases. These fluxes are further elaborated through Figure 2 and fully discussed in respective sections in the (newly created) Results and Discussion Section. We believe that at present, the extent of that discussion matches the extent of available information; any further elaboration would instead be unsubstantiated and move into the realm of unwarranted speculation. We would not regard that as appropriate.

4) The choice of a 100 year time frame is arbitrary and, as acknowledged by the authors, introduces much uncertainty since their results strongly depend on the choice of this time scale. Moreover, 100 years are commonly used to derive global warming potentials are related to the lifetime of gases in the atmosphere, meanwhile, land processes occur at different and variable time scales. I therefore question the usefulness of combining all data into this one specific time frame.

Response: With respect, but we do not believe that the choice of 100 years as an as-

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essment horizon is dictated by the lifetime of gases in the atmosphere. Instead, 100 years is a period that is considered as a period of time over which it is useful to assess climatic impacts. It gives a balance between impacts on the present generation and those for our children and grandchildren. The same rationale underlies the use in the present context. Had we used a shorter assessment horizon, it would have elevated the importance of carbon stock changes and reduced the importance of methane and nitrous oxide. Would that have appropriately reflected the importance of the impacts of these gases on present versus future generations? We believe that the 100-year assessment horizon would have struck that most appropriate balance, and that 100-year choice is, of course, consistent with that made in most greenhouse gas assessments.

5) The historic assessment is highlighted in the abstract but not at all discussed in the discussion section. What is the implication and message from this analysis?

Response: We have added some relevant discussion to the (newly named) Section 3.4 Combined effect on net greenhouse gas emissions

6) The authors create a very biased assessment for wetlands by only including CH<sub>4</sub> data for wetland conversion. There is a body of literature available that reports natural peatland and drainage effects on CO<sub>2</sub> exchange, and peatland biomass data (missing in Table 2) is also available in the literature.

Response: As already stated in an earlier response, we recognise that the full GHG implications of draining wetlands were not fully covered in the present study. It was therefore excluded from (the previous) Figures 2 and 3, but it was still discussed to some extent because it is such an important land-use change. We have now omitted the wetland values from Tables 2 and 5 as well and modified the text in the Discussion to make it clear that the Discussion refer to issues that are not supported through data presented here.

7) The manuscript reads fairly well overall but at several occasions, elements from the method, result and discussion occur inappropriately in other sections, some examples

C2259

are outlined below in specific comments.

Response: We are pleased with the reviewer's positive overall assessment, and hope that with our response to the specific issues raised below, the reviewer would be even more satisfied.

Specific comments: Pg 1054, L 19: The term 'generally' is vague, quantify!

Response: Changed to: "In all land-use changes involving forests, changes in biomass carbon dominated the overall change in net GHG emissions."

Pg 1058, L 17: On what do the authors base the use of the '75% assumption'?

Response: We recognize the issue raised by the reviewer and have now based the numbers for biomass carbon stocks of primary and secondary forests on information provided in WBGU (1998) where a compilation of relevant research on just that question has been provided. We stated it in the revised section in 2. 2. 1. Quantifying changes in biomass carbon stocks.

Pg 1059, L 10-15: This belongs into the discussion section

Response: We have moved this section to the (newly created) Results and Discussion section.

Pg 1059, L 22: What does 'one-off carbon stock changes' mean? Clarify.

Response: Upon land-use change, carbon stocks change only once. Hence, the climate-change impact of the change in carbon stocks is independent of the length of time that one considers. Upon deforestation, a site might lose 100 tC ha<sup>-1</sup>, especially if biomass carbon is burnt. That loss remains at a 100 tC loss whether one looks at it over 1 year, 10 years or 100 years or longer. In contrast, a change in methane or nitrous oxide emissions is effective over each year that a land use remains under the new land use. So, a changed methane emission by, say, 10 kg ha<sup>-1</sup> yr<sup>-1</sup> translates into a climate change impact of 10 kg ha<sup>-1</sup> over 1 year, 100 kg ha<sup>-1</sup> over 10

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years and 1000 kg ha<sup>-1</sup> over 100 years. So the relativities between the importance of carbon-stock changes versus on-going emission changes of methane and nitrous oxide change very much with the length of time that is considered. To make that clearer in the manuscript, we have added some explanation in the text as follows: "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. Upon land-use change, carbon stocks change only once. Upon deforestation, a site might lose carbon, especially if biomass carbon is burnt. That loss remains is a one-off change regardless of the length of time that one considers. In contrast, a change in CH<sub>4</sub> or N<sub>2</sub>O emissions is affective effective over each year that a land use remains under the new land use. So the relativities between the importance of carbon-stock changes versus on-going emission changes of CH<sub>4</sub> and N<sub>2</sub>O change very much greatly with the length of time that is considered."

Pg 1062, L 9-10: Provide reference

Response: We added a relevant reference as below: Pre-revision "However, because globally, grazing is dominated by sheep and cattle, we used the same average CH<sub>4</sub> emission rates for all grazed lands."

Post-revision "However, because globally, grazing is dominated by sheep and cattle (FAO, 1996), we used the same average CH<sub>4</sub> emission rates for all grazed lands."

Pg 1065, L 19-20: Why do greater C contents make smaller differences? Clarify the logic of this sentence.

Response: We have changed the sentence to: "The average C stocks of grasslands were also greater than the C stocks of cropland (Table 1), thus also making a small difference in any conversions between grasslands and croplands".

Pg 1066, L 21-22: what does 'mostly' mean? Quantify.

Response: The sentences (Pg 1066, L 20-23) were intended to briefly explain the

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quantified results reported in Table 5. This whole section has been revised and re-ordered.

Pg 1067, L 1-4: move to discussion

Response: This data set and the associated description have now been removed from the manuscript.

Pg 1067, L 6-7: method section element

Response: With respect, but this sentence is not a description of methods, but a one-sentence introduction to the results that are presented next. We feel that we would do a disservice to our readers if we removed this introductory statement here.

Pg 1067, L 9: Results are often referred to several figures and tables. This indicates that some tables and figures are redundant, see also further comments on this below.

Response: We recognize that there was some duplication in the presentation of our data and have now omitted Figure 2 from the manuscript.

Pg 1067, L 11-14: by how much?

Response: This Section discusses the results that are presented in Table 6 and Figure 2. The exact numbers are presented in the table and figure. This text tries to discuss the important trends in the data, the main findings. There is no point in repeating the same detailed information that is already in the table and figure. The text tries to convey information that is not already given in exactly the same form in the table and figure.

Pg 1068, L 10-16: what is the point of this paragraph?

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

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Pg 1069, L 4-8: This is not new information and its presentation does not develop the current analysis and findings.

Response: This section discusses the reversibility of biomass stocks. We think it is important to point out that carbon losses are not simply and immediately reversible. A mature forest can be cut down and burnt with all carbon lost within a short period of time. If the forest is allowed to regrow, it may eventually reach a similar amount of biomass as it had before being cut down, but that may take 100s of years, and over that period, the carbon stocks will be lower than in the mature forest. This has important carbon implications, and it seems important to us to point those out.

Pg 1069, L 16-17: don't repeat results in the discussion section

Response: The Results and Discussion sections have now been merged to address the problem pointed out by the reviewer and related issues.

Pg 1070, L 23: Quantify the term ‘more common’

Response: To clarify this further, we revised the sentence as below: Pre-revision “The present study combined all the data summarized in these previous studies and this combined data confirmed the result of the more recent analyses in showing that carbon gains following reforestation of pastures are more common than carbon losses.”

Post-revision “The present study combined all the data summarized in these previous studies, and these combined data confirmed that the results of the more recent analyses in showing that carbon gains following reforestation of pastures are more common than carbon losses.”

Pg 1070, L 25ff: don't repeat results in the discussion section

Response: As mentioned in a response to a previous comment, the Results and Discussion sections have now been merged to address this and other identified problems.

Pg 1071, L 3ff: The section on soil erosion appears very random and excessive. Why

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do the authors highlight this specific type of disturbance? There are many other disturbance types that could be included and discussed.

Response: Clearly, there are other types of disturbances that could have also been discussed here. However, we focussed on the impact of soil erosion because it still remains difficult to confidently assess the overall effect of soil erosion on carbon exchange with the atmosphere. While it is well known that erosion can cause large C losses on affected sites, its effect on the global carbon budget is less clear as carbon lost from a site may not necessarily be lost to the atmosphere. We thought it would be important to mention in this context that there is still incomplete understanding and quantification of the erosion processes and carbon exchange.

Pg 1071, L 1ff: The discussion sections on N<sub>2</sub>O and CH<sub>4</sub> changes is weak and does not provide much new insight. Moreover it underlines the weakness of the assumption and data set underlying this study.

Response: Without reference to specific examples, we are left unsure as to what exactly the reviewer is objecting to and what could or should be modified. We have now merged and revised the Results and Discussion sections dealing with these gases and hope that the revised version would be judged more favourably.

Pg 1073, L 1ff: Very little data is presented for wetlands. Thus, a whole discussion paragraph on this topic seems excessive. Remove or improve this section with more data from temperate peatlands and a more robust analysis.

Response: As stated above, since there are so many issues and data requirements to fully deal with wetland conversion, we could not adequately deal with it within the present work. We have therefore clearly and consciously omitted wetland from consideration in the present manuscript and also excluded it from Tables 2 and 5. However, it was still discussed to some extent because it is such an important land-use change, but we have made it clear that this discussion refers to issues that are not supported through data presented in the present work.

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Pg 1074, The section 4.7 entitled 'comparison with other assessments' is really only an overly detailed comparison with one other study from one of the authors.

Response: The reviewer is correct in that assessment, but it was because we are unaware of any other studies that could have been compared with the work done here. We actually believe that this also indicates the value of the present work in providing a comprehensive combined assessment of the relevant changes in all greenhouse gases following land-use change that has not previously been done, or been done only in the one study referred to, which, however, mainly used a modelling approach.

Pg 1075, L 4-7: but doesn't this study use the GWP of N<sub>2</sub>O when comparing and stating its small importance?

Response: Yes, this study used the GWP of N<sub>2</sub>O. That has been clearly spelt out in the Methods (2.2.4 Quantifying changes in soil CH<sub>4</sub> and N<sub>2</sub>O emissions). As this should have been clear from the description, we are unsure what the reviewer is referring to.

Pg 1075 The conclusion section is very weak. It merely repeats results and does not provide any novel insights and implications.

Response: Insights and implications of the present study have now been stated in the Results and Discussion section, and the Conclusion section has been omitted.

Tables and Figures: There is some potential in making the presentation of Tables and Figures more concise. Table 2: Biomass carbon stock data is already presented in Table 1. Include biomass data for wetlands.

Response: Table 1 includes the biomass of only four land-use types, and Table 2 uses not only data for these four types but also shows the calculations for biomass differences and then the conversion to changes in atmospheric CO<sub>2</sub>. So, the data in these different tables is sufficiently different that it would not be possible to omit one or the other. As stated above, consideration of wetland conversions has now been removed from the manuscript.

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Table 3: define Kij and Sij

Response: These variables are defined in equations 5 and 6 in the section in the Materials and Methods that deals with changes in soil carbon.

Table 5: Define CH4 and N2O; font size differs compared to other tables.

Response: We defined CH4 and N2O in the caption of Table 5. The different font size is a type-setting issue. We produced all tables in the same font size, but the larger tables were reproduced in a smaller font to fit into the available space in the type-setting of the paper. We will endeavour to ensure that all tables are in a consistent and legible font if the paper gets accepted for final production as a Biogeosciences paper.

Table 6: How is it possible that the contributions of biomass C, SOC, CH4 and N2O exceed 100% in some conversion scenarios?

Response: There were some calculation errors and the percentages should have all added up to 100% in all conversions. We thank the reviewer for having spotted that mistake. That has now been corrected.

Figure 1 could be moved into the supplementary section.

Response: Tables 3 and 4 rely heavily on the information from Figure 1. So we think it is better to retain Figure 1 as it is.

Figure 2: change 'd' to the 'delta' symbol in legend

Response: Changed as suggested.

Figure 3 looks nice but is a repetition of previous table and figure information. Remove.

Response: As stated above, Table 6 and Figure 2 did present similar information. We have therefore omitted the old Figure 2 from the text. We have opted to retain Figure 3 as this figure presents the main information in a useful visual form.

Figure 4: Typo in legend. Include the number of studies representing each region.

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Response: We corrected the typo in legend from "Forst" to "Forest". The total net GHG contribution of LUC from forest to cropland or grassland from the areas estimated to have undergone different LUCs between 1765 and 2005 (Meiyappan and Jain, 2012) and total net GHG emission rates associated with those respective LUCs as determined in the study (see section 2.3). Therefore, it is not relevant to ask for numbers of studies representing each region.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/11/C2254/2014/bgd-11-C2254-2014-supplement.pdf>

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