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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

Anonymous Referee #3

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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 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 stud-

ies 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.

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.

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. Numerous inventories and synthesis studies on SOC exist in the literature whose findings are merely repeated and cited in this study. Fertilizer effects on N2O emission are also known. Instead, the importance of the contribution from CH4 and N2O exchange for instance, which is touched on in Table 6, would be much more interesting to elaborate on.

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.

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?

6) The authors create a very biased assessment for wetlands by only including CH4 data for wetland conversion. There is a body of literature available that reports natural peatland and drainage effects on CO2 exchange, and peatland biomass data (missing in Table 2) is also available in the literature.

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 are outlined below in specific comments.

Specific comments:

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

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

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

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

Pg 1062, L 9-10: Provide reference

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

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

Pg 1067, L 1-4: move to discussion

Pg 1067, L 6-7: method section element

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.

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

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

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

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

Pg 1070, L 23: Quantify the term 'more common'

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

Pg 1071, L 3ff: The section on soil erosion appears very random and excessive. Why do the authors highlight this specific type of disturbance? There are many other disturbance types that could be included and discussed.

Pg 1071, L 1ff: The discussion sections on N2O and CH4 changes is weak and does not provide much new insight. Moreover it underlines the weakness of the assumption and data set underlying this study.

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.

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.

Pg 1075, L 4-7: but doesn't this study use the GWP of N2O when comparing and stating its small importance?

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

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.

Table 3: define Kij and Sij

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

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

Figure 1 could be moved into the supplementary section.

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

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

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

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