Response to the comments on bg-2015-458

Dear Professor Michael Weintraub,

Please find attached our revision of manuscript bg-2015-458 “Greenhouse gas emissions in natural and agricultural lands in sub-Saharan Africa: synthesis of available data and suggestions for further studies”. We thank you and the reviewers for constructive suggestions on the manuscript. We have addressed each of the comments as outlined below.

A) Associate Editor

Thank you for your revised submission to Biogeosciences. Your revised MS has received two more reviews from highly qualified reviewers in this field, and it is clear from their reviews that this revised version is much improved. However, they again offered numerous substantive comments to improve the manuscript, especially reviewer 2. I have now had a chance to consider the reviewers’ comments and your response. Based on these, and my own assessment, I am willing to reconsider a further revised resubmission that addresses the reviewers comments, especially the most substantive comments from reviewer 2 regarding the lack of conceptual framework and synthesis. I encourage you to give strong consideration to reviewer 2’s suggestion of adding a paragraph or two to the introduction discussing the broader ecological context of possible controls and drivers of GHG emissions/associated biogeochemical processes. Also, given the sometimes contradictory results presented, please be sure to discuss what conclusions can be drawn from this analysis, and what further research is needed to be able to draw more substantive conclusions in the future.

>>Response:

We have revised the manuscript based on the general recommendations of the editor and specific comments from the two reviewers. Details of the more substantial changes are summarized below:

1. We have added a paragraph to the introduction to strengthen the conceptual underpinning of the review. This includes much more detail on the mechanisms, possible controls and drivers of greenhouse gas emissions.

2. We have included two new tables (Tables 3 and 5) to synthesize the study results. Table 3 provides summary of environmental factors affecting greenhouse gas emissions for each land use/ecosystem type. Table 5 provides a summary of the current state of knowledge of greenhouse gas emission processes, control factors, estimation and model prediction and clearly highlights the large research gaps that still exist.

3. We have included additional information on how the quality of the studies included in the review affect the reliability of estimates of annual GHG emission rates.

4. Two recently published papers reporting greenhouse gas emissions in African natural and agricultural lands have now been included in the manuscript.

B) Report #1 by Referee #3

General remarks

This manuscript presents a unique and very valuable comprehensive review of studies on greenhouse gas emissions in sub-Saharan Africa. At least to my knowledge, no comparable paper is available so far. Overall, the paper is well-written and clearly structured. Also, I fully agree with the conclusion that this area in understudies and that there is an urgent need to develop and agree on a strategy for addressing this data gap. This manuscript provides a solid basis with information about the status-quo.

Minor remarks

L6-10: It would be good not only to provide the numbers for global warming potential but also for the resulting radiative forcing, which is a better quantity for what really matters in the end.

L19: There are at least two studies that I know that are missing in this analysis:

Ago EE, Serca D, Agbossou EK, et al (2015) Carbon dioxide fluxes from a degraded woodland in West Africa and their responses to main environmental factors. Carbon Balance Manag 10:22. doi: 10.1186/s13021-015-0033-6

Quansah E, Mauder M, Balogun AA, et al (2015) Carbon dioxide fluxes from contrasting ecosystems in the Sudanian Savanna in West Africa. Carbon Balance Manag 10:1. doi: 10.1186/s13021-014-0011-4

Therefore, the Google Scholar search should be extended for the key word "West Africa" and perhaps others, too. It would be a pity, not too include these nice data sets about four more long-term flux measurement sites.

>>Response:

Both Ago et al. (2015) and Quansah et al. (2015) reported ecosystem respiration (CO_2 fluxes) measured by eddy-covariance systems. Therefore their results are not comparable to the results (soil and water surface greenhouse gas fluxes measured with a chamber technique) reported in the current manuscript.

We have, however, included details from two newly published papers reporting greenhouse gas emissions in African natural and agricultural lands:

Bateganya, N. L., Mentler, A., Langergraber, G., Busulwa, H., and Hein, T.: Carbon and nitrogen gaseous fluxes from subsurface flow wetland buffer strips at mesocosm scale in east Africa, Ecological Engineering Ecol. Eng., 85, 173-184, 2015.

Rosenstock, T. S., Mathew, M., Pelster, D. E., Butterbach-Bahl, K., Rufino, M. C., Thiong'o, M., Mutuo, P., Abwanda, S., Rioux, J., Kimaro, A. A., and Neufeldt, H. C. J. G.: Greenhouse gas fluxes from agricultural soils of Kenya and Tanzania, J. Geophys. Res., doi: 10.1002/2016JG003341, 10.1002/2016jg003341, 2016.

C) Report #2 by anonymous Referee #1

General comments

This article summarizes current knowledge about GHG emissions from different ecosystem types and land uses in Africa, and is a useful contribution, particularly by identifying numerous research gaps. The authors have improved the article to address the previous review comments. In particular, the new data quality assessment is very helpful, the results are better organized, and the new analysis of controls on CO2 emissions helps to better link the many disparate studies.

However, while it is clear that rigorous statistical synthesis (such as meta-analysis) is not possible with this dataset, the article still neglects to synthesize the available data. The authors have compiled and "reviewed" a large number of papers, but there is little synthesis *that draws conclusions across the studies. That means that for readers the review is still challenging to read: the authors present a long list of (often contradictory) results that is difficult to interpret. The tables are easier to follow, as a reference for quickly locating studies and emissions rates for different ecosystem types.*

>>Response:
We agree that a clearer synthesis of the results would improve the paper. In this revision we've tried to do that by providing two new tables (Tables 3 and 5) as well as additional text. Table 3 provides a summary of the environmental factors affecting greenhouse gas emissions for each land use/ecosystem type. Table 5 provides a much clearer summary of the current state of knowledge of greenhouse gas emission processes, control factors, estimation and model predictions in SSA and as a result we provide much more clarity on the large research gaps that still exist. We hope that these inclusions, along with the additional text, have enabled us to strike the right balance between providing data on SSA GHG emissions and information on the broader patterns and controls on emissions.

The addition of the data quality assessment is helpful. However, that information should be integrated into the results and discussion rather than just being summarized in a separate results paragraph, particularly in cases of land uses or management strategies based on just a couple of studies. How robust are particular conclusions about emissions rates or their sources and drivers?

>>Response:
We have added additional text where appropriate on how the data quality will affect estimates of annual emission rates. It is not that many of the studies were done incorrectly; it is just that many of them were established to answer specific research questions and not with the aim of generating annual GHG emission estimates. Nevertheless because there are so few studies (and even fewer of the highest quality and duration) in many regions, on many land uses and for some gases, data quality is a serious issue affecting GHG emission estimates from SSA. Our decision was to be clear about this lack of data but also to use what information we had to generate best estimates of GHG emissions. Were short-term, or relatively poor quality data have been used we have tried to be explicit about it and to be clear about the reliability of the estimates.

Specifically, we have made the following alterations:

In 3. 1. 1 CO_2 emissions,

Vegetable gardens were the largest sources of CO_2 emission largely due to the excessive C inputs. However, this was based on two studies that used photoacoustic spectroscopy, which has been found to be unreliable due to cross sensitivities between the various GHG and water vapour (Rosenstock et al. 2013), suggesting that these production systems need to be studied further. The next largest sources of emissions were agroforestry, cropland and rice production systems (Table 1 and SI Table 2).

Many of these estimates are based on short-term, infrequent or poor quality sampling (Table S1) suggesting that the uncertainties are likely much greater than implied with the provided standard error. This is not meant as a critique of these studies, as many of them were specifically designed to answer specific research questions about the effects of various biogeoclimatic factors on emission rates rather than determining the cumulative annual emissions. However, given the lack of other data, these still provide the "best guess" for cumulative emissions.

In 3. 1. 2 CH_4 emissions,

As with studies on CO_2 emissions, many of these studies used only infrequent or poor sampling methodologies (Table S1), and there is a high degree of uncertainty surrounding the estimates.

In 3. 1. 3 N_2O emissions and emission factor (EF),

The number of studies on N_2O emissions in Africa is, however, particularly low (n=14), with some questions regarding the quality of the methods (Table S1) in some of these studies, and there are significant regional gaps leading to large uncertainties in the conclusions that can be currently drawn.

In 3.1.5 Data quality assessment,

As mentioned earlier, many of these studies were undertaken to address a specific research question, not determine annual, cumulative emissions, however the result is that the degree of uncertainty around cumulative emissions is likely higher than what is indicated in Table 1.

The review continues to lack a conceptual framework. This could be addressed by adding a paragraph or two to the introduction discussing the broader ecological context of possible *controls and drivers (proximal and distal, or direct and indirect) of GHG emissions/associated biogeochemical processes. The authors now do a better job of discussing and analyzing environmental controls in the results section (where possible with the limited data available), but still do not place the review within broader conceptual frameworks. That makes reading the list of findings confusing and difficult to follow. In some places potential biogeochemical mechanisms are discussed within the results (e.g., page 12, lines 7-11), but the review would be much improved by starting with a framework to set up the discussion of findings that span a highly variable set of ecosystems and management strategies. Section 3.3 starts to address this issue, but at the very end of the article. Some of this text could be moved to the introduction to help set up the findings. Then, these findings should be better synthesized.*

>>Response:

In the introduction, we have included a paragraph to provide a conceptual framework with information on the mechanisms, controls and drivers of GHG emissions as below:

"Interpretation of GHG emissions from soils and terrestrial water bodies is complex because of the multiple, sometimes competing, biological, chemical and physical processes affecting fluxes. Spatial and temporal variability in GHG fluxes is also high and challenging to capture with direct measurement. This in turn makes reliable annual GHG flux estimates from different soils, land uses and regions quite rare in SSA. Net soil CO_2 flux is largely a product of autotrophic respiration derived from plant roots and heterotrophic respiration of soil organic matter. Soil CO_2 flux provides an integrated result of biological CO_2 production throughout the soil column, changes in soil CO_2 diffusivity in the soil profile, and in some areas geological processes (Raich and Schlesinger, 1992). Net CH_4 flux is the result of the balance between methanogenesis (microbial production under anaerobic conditions) and methanotrophy (microbial consumption) (Dutaur and Verchot, 2007). Methanogenesis occurs via the anaerobic degradation of organic matter by methanogenic archaea within the archaeal phylum *Euryarchaeota* (Thauer, 1988). Methanotrophy occurs by methanotrophs metabolizing CH_4 as their source of carbon and energy (Hanson and Hanson, 1996). Soil N_2O in produced through three main processes such as nitrification (Kowalchuk and Stephen, 2001), denitrification (Knowles, 1982) and nitrifier denitrification (Wrage et al., 2001). Identifying controlling factors and their effects on GHG fluxes is a pre-requisite to enhancing our understanding of efflux mechanisms and accurate quantification of GHG emissions. Environmental factors such as soil properties (e.g., soil type, carbon and nutrients; Pelster et al., 2012), climate characteristics (e.g., temperature, rainfall, drought; Dijkstra et al., 2012) and vegetation type (e.g., crop or forest types; Masaka et al., 2014) can affect GHG fluxes. Management practices can also play important roles in controlling GHG fluxes. The controlling management practices include land-use change (Kim and Kirschbaum, 2015), logging (Yashiro et al., 2008), changing water discharge (Wang et al., 2013), soil compaction (Ball et al., 1999), tillage (Sheehy et al., 2013), removal of crop residues (Jin et al., 2014) and N input (whether organic or inorganic) (Hickman et al., 2015).

Specific comments:

Page 5, line 17-19: Re-write to say: "Data were acquired by searching existing peer-reviewed literature using the names of the sub-Saharan countries and the GHGs (i.e., CO2, CH4, N2O) as search terms (using Web of Science and Google Scholar; 1960-2015). These criteria yielded 307 peer-reviewed papers."

>>Response:

We have made the revisions as suggested.

P. 8, line 11: Explain what is meant by "large C inputs" to vegetable systems. Is this manure? Also it looks like this is a case where the broad conclusion that vegetable gardens are the largest source seems to be a stretch: this is just based on two studies of "marginal quality". This is an example of where it would be helpful to comment on the quality of those two studies in with the results.

>>Response:

We have revised the sentence as suggested:

"Vegetable gardens were the largest sources of CO_2 emission largely due to the excessive C inputs. However, this was based on two studies that used photoacoustic spectroscopy, which has been found to be unreliable due to cross sensitivities between the various GHG and water vapour (Rosenstock et al., 2013), suggesting that these production systems need to be studied further."

p. 9, line 15- It would be useful to interpret this finding in terms of land area occupied by vegetable gardens vs. annual grain crops, for example, in order to determine overall contribution at a landscape, country, or regional scale.

>>Response:

Since information on the area of vegetable gardens is not available we are not able to interpret the finding as suggested.

p. 9, line 21: Yes, but why? This is an example of where the authors could link to broader understanding about biogeochemistry, which should be set up in the introduction. Why do inputs correlate with losses? It also seems worth mentioning that N source seems to matter (in Figure 3A), though it is hard to tell how many data points there are. The studies with organic N sources apply lower total N inputs, and it also looks like they fall below the line. If so, this relates to general understanding that applying N together with C improves the capacity for microbes to process and store the N in the ecosystem. In contrast, large, pulse additions of inorganic N that exceed the capacity for plant and microbial sinks to store the N internally likely push loss pathways of the N cycle.

>>Response:

We have added a brief summary of mechanisms and control factors for all greenhouse gas (CO_2, CH_4 and N_2O) fluxes in the introduction as suggested.

The types of N source (ex. inorganic fertilizer, manure, compost, etc.) can affect N_2O emissions. However, due to lack of available data in African agricultural lands (only one point data in Fig. 3) it is not able to discuss it in the current manuscript.

p. 11, line 1: Should be "Twenty three" - "...were categorized as having poor to very poor methods" would be clearer.

>>Response:

We revised as suggested.

p.12, line 5: It's not clear what is meant by soil "rewetting" here as compared to just soil moisture.

>>Response:

'Soil rewetting' in this example means 'rewetting of dry soil' . We have revised the sentence as below:

"Increased GHG emissions following rewetting of dry soil were observed in various regions in Africa."

Section 3.2.1 is difficult to follow. Can the authors synthesize these studies rather than reporting a list of what different studies found?

>>Response:

To improve and clarify the synthesize aspect of the review we have included two new tables (Tables 3 and 5). Table 3 provides summary of environmental factors affecting greenhouse gas emissions for each land use/ecosystem type. Table 5 provides summary of the state of knowledge of greenhouse gas emission processes, control factors, estimation and model prediction and serves to highlight the research gaps.

p. 13, line 5: Vegetation type/litter quality, or C input, should also be discussed in the intro as a potential control on GHG emissions.

>>Response:

We have added a brief summary of mechanisms and control factors for greenhouse gas CO_2, CH_4 and N_2O fluxes in the introduction as suggested.

p.14, line 20: Says "references" rather than including the actual references

>>Response:

We have now added the references as below:

"Termite mounds are known sources of CH_4 and CO_2 (Nyamadzawo et al., 2012; Brümmer et al., 2009)."

p. 14, line 24: Termite mound units are difficult to interpret.

>>Response:

We agree! The unit is actually CH_4 emission per a termite mound per second. Because the value itself was not that important (compared to the message) we have amended the sentence as below:

"In Cameroon, the mounds of soil-feeding termites (*Thoracotermes macrothorax* and *Cubitermes fungifaber*) were point sources of CH_4, which at the landscape scale may exceed the general sink capacity of the soil (Macdonald et al., 1998)."

p. 16, line 9: should be "those" not "these"

>>Response:

We have revised as suggested.

p. 16, lines 10-12: Re-write this sentence

>>Response:

Revised as follows:

"Methane fluxes were higher in river deltas (~103 mg CH_4 m^{-2} d^{-1}) compared to non-river bays (<100 mg CH_4 m^{-2} d^{-1}) in Lake Kariba (Zambia/Zimbabwe) (DelSontro et al., 2011)."

p. 17, line 9: Need to re-write this sentence (linked to environment and water quality?)

Revised as follows:

" Studies found the concentration and flux of GHGs are strongly linked to environment and water quality"

p. 17, lines 23-25: This could be set up earlier to link to broader ecological concepts: either here, or in the introduction. What is known, generally, about drivers of GHG emissions in agroecosystems? Why would crop type matter, etc.? Without conceptual framing, this is a challenging-to-interpret list of what other studies have found. What general factors have researchers explored (N rate, N fertilizer form, adding crop residues, etc.)? Why are they expected to matter (if at all)? Are there key findings the authors can point to, rather than listing several contradictory ones in a row?

>>Response:

We have added a brief summary of mechanisms and control factors for greenhouse gas CO_2, CH_4 and N_2O fluxes in the introduction as suggested.

p. 18, line 11: Were these crop residues grown elsewhere and added as an amendment? Or are the authors talking about tilling in crop residues following harvest? Be clear what residue application vs. residue incorporation (which are both used in this paragraph) mean.

>>Response:

By 'incorporation of crop residue' we mean tilling in the crop residue from the preceding harvest. We have clarified this in the text and used the phrase 'residue incorporation' as an alternative to ‘residue application’.

p. 22, line 8: Is this a typo? Someone actually applied 2700 kg N ha-1 yr-1 to a vegetable garden?! If so, of course emissions to the environment (either leaching or gaseous losses) were very high- that is an outrageous N application rate, far exceeding the amount of N that any crops would need, and shouldn’t be used to generalize about how that management or land use type relates to emissions.

>>Response:

This was reported in the paper by Lompo et al., (2012). According to the paper, the experimental design was to reflect common urban gardening (small size and intensive farming) practiced in Bobo-Dioulasso, Burkina Faso. Although the application rate is outrageous it really does reflect a real situation and we believed it to be worthwhile reporting in the manuscript.

p. 23, line 8-9: should say "reduced inorganic fertilizer inputs, accounting for N input from the legume trees;…"

>>Response:

Revised as suggested.

p. 24: Some of this content could be moved to a new paragraph in the intro explaining which edaphic and management controls are expected to be important.

>>Response:

We hope the new information we've provided on the mechanisms and control factors for greenhouse gasefluxes in the introduction will be sufficient.

p.25, line 24: the excessive N inputs in vegetable gardens should be mentioned earlier, if that number was correct

>>Response:

We have now revised the sentence (Vegetable garden in 3.2.2.) to:

The high N_2O EFs may be attributed to the excessive amount of applied N (2700 – 2800 kg N ha^{-1} yr^{-1}) to get high yields in vegetable gardens since surplus N will stimulate N_2O production and also indirectly promote N_2O production by inhibiting biochemical N_2O reduction (e.g., Shcherbak et al., 2014; Kim et al., 2013).

p. 27, line 14: "implication of social scientists" does not make sense

>>Response:

We agree and have deleted the sentence.