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Biogeosciences Discussions

Interactive comment on "Reviews and syntheses: Greenhouse gas emissions in natural and agricultural lands in sub-Saharan Africa: synthesis of available data and suggestions for further studies" *by* D.-G. Kim et al.

Anonymous Referee #1

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General Comments

This article is an interesting, novel review of greenhouse gas (GHG) emissions from natural and agricultural ecosystems in 22 countries in sub-Saharan Africa, compiling published data on CO2, CH4, and N2O emissions. The authors summarize knowledge of the baseline (current) emissions from this region. They report measured emissions from a range of different ecosystem and land use types, and management practices. The variability in measured emissions is large, and the authors highlight important research gaps and the need for further studies to elucidate environmental and man-

agement drivers of emissions at multiple spatial and temporal scales.

This paper fills an important knowledge gap. However, I think the authors could improve upon several aspects of their review. Both the results and summary sections might be improved by including a framework that organizes or summarizes the suite of complex direct (e.g., oxygen and carbon availability) and indirect (e.g., root and microbial respiration, soil texture, temperature) controls on emissions across the studies – and how those controls are affected by management (e.g., tillage, fertility inputs) and ecosystem state factors (e.g., parent material, climate, vegetation). Related to this, the review should also include more synthesis, if possible, such as quantitatively summarizing findings regarding controls across studies. As currently written, the results read as an inventory or list of emissions rates and key findings from individual studies (rather than a "synthesis," which is in the title), depending on which factors individual studies addressed (e.g., temperature, moisture, vegetation type, pH, dynamics of C and N availability, etc.). The current presentation of results makes it difficult to discern – on average or in aggregate for different ecosystem types or management systems – the state of knowledge regarding relative importance of different drivers of variation. Statistical analysis was performed on agricultural studies to fit models for emissions as a function of N inputs. I wonder what additional statistics might be performed on these data to understand the aggregate effect of controls on emissions rates across multiple studies or ecosystem types (i.e., how emissions vary with these different factors)? Are there consistent effects of soil texture across the studies? Such information (if available) would better direct future research efforts. For example, the authors could highlight whether more is known about some controls than others, or if there is a lack of information about interactions between different controls, etc. It seems that a key point from the findings is that there is a need for more studies that address questions about how interactions between management (fertility practices, tillage) and environment (soil texture and type, etc.) drive GHG emissions, but this discussion could be strengthened.

Second, more attention should be given to the disparate methods within the studies. The authors are clear that they selected only in situ studies, but then note that a wide range of methods was used in the studies they synthesize. Could this be accounted for in some way in the analysis (e.g., analyze emissions by measurement method)? Are some of the results presented likely more robust than others? More information could be added to the supplementary tables; for example, duration of the study (whether emissions were measured for one year, one growing season, multiple years, etc.), frequency of sampling events within a year, capturing major weather events, etc. Were any of the measurements for agricultural systems on actual farms, or were they in experiment stations? A methods column could also potentially list chamber type or other relevant information.

Third, the overall coherence would be improved by stronger links to theory, and by including broader discussion/interpretation of the summarized findings. For example, the authors could draw upon N saturation theory from N deposition studies in forest ecosystems (N surplus is mentioned in the discussion on page 16496, but might be better mentioned up front as a guiding framework for understanding a key driver of losses in systems with N inputs, and then woven throughout). For example, the finding that N2O emissions increased exponentially when fertilizer applications exceed plant uptake (for the very high rates) is in line with N saturation theory. Another option is to link findings to an ecological nutrient management framework in the agriculture section, which aims to couple C and N cycles (e.g., by adding a C source such as a cover crop together with an N source, or using organic N sources) to reduce N surplus and balance N inputs with harvested exports. Finally, the paper would improve with brief discussion throughout regarding why and how reported emissions for the different ecosystems might matter for current sustainability concerns, particularly regarding land use change. Linking emissions rates to crop productivity (the yield-scaled results) is an important start, but what other trade-offs are there? Vegetable systems with high emissions, for example, are likely a small proportion of total land use, and may contribute high nutritional value per area. Table 2 with the impacts of different management practices

gets at this, but it would be useful to identify some potential tradeoffs more generally and better synthesize across studies.

Specific Comments

Page 16483, lines 7-13: How do these numbers compare to countries or regions with highly industrialized agricultural systems and higher average N fertilizer rates? This would help to place these figures in a broader context.

Page 16484, line 9: How many total papers did the initial search yield (from which the authors distilled the papers that met the criteria for inclusion)?

Page 16485, line 6-11: Is there any reason to narrow your selection criteria? Can the authors analyze the results for different ecosystems by measurement method or frequency? Adding more information to the supplementary table on methods would help.

Page 16486, lines 7-10: Can the authors analyze the effect of soil moisture and temperature across the forest studies (e.g., more of a meta-analysis approach)? Or find ways to lump studies that measured or reported data on similar categories of controls?

Page16487, lines 8-25: A mass balance, or C budget, perspective would help frame this paragraph. How do emissions relate to above- and belowground C inputs?

Page 16492, lines 7-27, Page 16493, lines 20-29; Page 16494, lines 7-9: Here are examples of where drawing on a mass balance perspective (and N saturation) would help provide a framework within which to interpret this list of results from individual studies. For example, in the case of the green beans, which did not increase emissions, much of the fixed N is harvested and exported from the system. There is also a need to understand relationships between management, N surplus, and emissions, which will depend on how loss pathways are partitioned (leaching v. gaseous losses).

Page 16493, line 3: I thought the review didn't include incubation studies. Or was this in situ?

Page 16495, lines 22-24: The C isotope result comes a bit out of context here. Briefly explain why this was measured/objective of the study.

Page 16497, line 6: What is meant by output here? Harvest, leaching, or gaseous losses?

Page 16497, lines 12-21: In the agroforestry/maize systems, were fertilizer rates adjusted (reduced or eliminated) based on the N input from the legume trees? It seems that for some of these studies the N balance perspective would allow the authors to say whether there may be potential to reduce emissions (in line with theory, if N surplus is reduced), but may require better management (i.e., reduced inputs) and rotation planning.

Page 16498, line 5: Again, the discussion of incubation experiments is a bit confusing. Were these included in the selection criteria? Are they in situ rather than lab incubations? Perhaps clarify in the methods.

Page 16499, line 8: Could place results in a broader sustainability context: soil CO2 emissions are only one component of emissions from agricultural systems, which also have all of the CO2 emissions from tillage, fuel use, and embodied emissions in chemical inputs, etc. (if used).

Page 16500, lines 10-24 and Figure 5: Part (a) Can the authors separate the total N input by emissions graph by N source (e.g., manure, fertilizer, legume, or some combination of these)? It would be most interesting for part a, which is in a more realistic range of N input rates. For parts (b) and (c) it might be helpful to explain why these studies used such unrealistically high N rates, far outside of what would make economic sense for any farmer. What was the context of these studies?

Page 16502, line 6: And N source (whether organic or inorganic)

Page 16502, line 15: Yes, and link new knowledge of microbial communities (e.g., functional gene abundance) to emissions rates (when talking about importance of identify-

ing mechanisms/driving processes)

Technical corrections

Page 16484, line 4: spell out AFOLU the first time

Page 16488, line 11: typo “this mechanisms”

Page 16503, lines 22-23: two typos (advanced and higher)

Interactive comment on Biogeosciences Discuss., 12, 16479, 2015.