

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.

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?

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.

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 304 peer-reviewed papers.”

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.

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.

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.

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

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

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

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.

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

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

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

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

p. 17, line 9: Need to re-write this sentence (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?

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.

p. 22, line 8: Is this a typo? Someone actually applied $2700 \text{ kg N ha}^{-1} \text{ yr}^{-1}$ 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.

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

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.

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

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