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Interactive comment on "Stem and soil nitrous oxide fluxes from rainforest and cacao agroforest on highly weathered soils in the Congo Basin" *by* Najeeb Al-Amin Iddris et al.

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GENERAL COMMENTS Stem-derived GHG emissions from tropical trees are a relatively understudied phenomena, and research on this topic has only really gained momentum in the last 5 years. The most comprehensive datasets are from organic soils in SE Asia (e.g. Indonesia), South and Central America (e.g. Brazilian Amazon, Panama); much less data is available from Africa or from well-drained mineral soils. The former is important because of the large areal extent which Africa accounts for, representing a major uncertainty in global atmospheric budgets of trace gases. The latter is critical because gas transport mechanisms through trees are thought to differ

for wet, organic soils compared to mineral soils (i.e. arenchymatous transport in wet soils versus xylem transport in well-drained soils). In addition, low redox conditions in wet, organic soils are likely to drive different patterns of trace gas production and consumption compared to well-drained mineral soils, which could affect the composition and magnitude of trace gas fluxes.

This research addresses these knowledge gaps by quantifying tree stem and soil fluxes of N2O from well-drained, mineral soil sites in the Congo. In addition to the emissions themselves, the authors have quantified the effects of land management (i.e. unmanaged tropical forest versus cacao agro-forestry), the influence of key environmental variables, and used stable isotopes to qualitatively assess the contribution of soil-produced N2O to stem emissions. The paper was well-written and clearly argued; the bigger picture context of the research was clearly characterised, and neatly linked to the specific research questions posed in this study. The methods, results and discussion sections were also well-written and easy to understand. Sufficient information was provided in the methods such that other experts could replicate this study in other locations. The description of the statistical approach was thorough, and provided the reader with a complete picture of how the data were analysed. The experimental design was robust and well-replicated, taking care to account for potential site or treatment effects (e.g. edge effects) on the experimental results. The authors' extrapolation of their findings to larger spatial scales was thought provoking, as it provides the wider flux community with a baseline or starting point to discuss how mineral soil forests in tropical Africa could be influencing regional and global budgets of N2O via tree stem emissions (see also my comments in point 8).

Overall, I support this paper for publication, given the rigour of the experimental design, the novelty of this dataset, and the high quality of the manuscript. I did, however, have a few questions and suggestions which I believe could improve this manuscript. First, I was curious if the trees sampled in this study had similar or different functional traits (see points 5 and 6 below)? From the experimental design, the authors indicated that they sampled the dominant taxa in each cover type. I had wondered if the dominant trees were functionally similar to each other or if they were functionally different (e.g. do they fall within a similar "space" along the plant economic spectrum, or do the taxa span different life history strategies)? If the former, then the similarities in stem fluxes among taxa or between cover types may be partially explained by the similarity in the functional traits or ecophysiology of the sampled trees. This could mean that plant communities with very different functional traits could show different flux rates or responses to environmental variables. If the latter (i.e. the dominant trees include a mixture of plants with different functional traits), then the findings from this work could be more widely generalisable across communities at different successional stages or with different species compositions.

Second, I was curious if the authors could use isotope mixing models or other data/techniques to infer how much of the N2O was derived from the soil rather than from other sources, such as plant tissues (see point 7)? For example, if there are data from ex situ experiments (e.g. mesocosm or greenhouse experiments) that indicate how much N2O could be produced from within plant tissues, then it may be possible to conservatively estimate what the potential flux rate was from this source under field conditions. Likewise, if plant-derived N2O has a different stable isotope composition from soil-derived N2O then it may be possible to use mixing models to ascertain how much N2O was derived from each source.

Third, it was not clear if forest age or size structure could pay a role in influencing rates of stem flux. The data presented in Table A1 tends to imply that the forests and cacao agro-forestry have a similar size structure (i.e. see basal area data). However, it is not clear if there could be an effect of stem size on flux rates (i.e. would stem emissions be similar or different for stands with smaller or larger stems?). If there is an effect of stem size on flux this could have implications for stands of different successional stages or ages.

Specific questions are outlined in the section below.

SPECIFIC COMMENTS 1. Lines 68-70: The literature on the effects of soil N availability, fertilizer and farm management practices is relatively well-developed, and I recommend adding a few more references here to add weight to your statement. To keep the referencing concise, you could cite one or two of the excellent review or synthesis papers published by colleagues such as Eric Davidson, Pam Matson or Peter Groffman? 2. Lines 86-94: What techniques can be used to determine the main transport mechanism for N2O for the trees in your study site? For example, are their differences in the isotopic fractionation for N2O transported via arenchyma versus xylem sap? 3. Lines 95-106: For prior stem flux studies on wet soils (i.e. Sunitha Pangala & Vince Gauci's work), wood density was found to be predictor for stem flux rates. Was this a variable measured here, or was wood density thought to be unimportant given that flux is likely to be via xylem transport (rather than aerenchmatic tissues)? 4. Line 109: To give readers a bit more insight into how you selected tree species for study, you may consider adding a sentence or phrase indicating that the trees measured represented the most dominant species in each plot. 5. Line 154-156: The only issue to be aware of here is that the most dominant species may have similar characteristics to each other because they may occupy a similar "space" along the plant economic spectrum and possess similar functional traits (e.g. in old-growth systems, the dominant species tend to show similar traits such as slow growth, high wood density, low tissue turnover times, higher N-use efficiency, shade tolerance, etc.). It's possible that plants with different functional traits (e.g. fast-growing species) may show slightly different physiological characteristics and consequently show differences in stem fluxes. 6. Lines 411-412: I think it is significant that there do not appear to be any statistically significant, species-specific differences in N2O flux in either forest or agro-forestry systems, suggesting that the mean or median N2O flux may be similar for trees growing on well-drained soils. The only potential issue to be aware of is whether or not this may be because the dominant trees sampled in this study possessed similar functional traits (assuming that they may occupy the same "space" along the plant economic spectrum; see point 5 above). This may be something worthwhile discussing further in the paper.

7. Lines 451-460: I understand the logic behind this statement and broadly agree with the interpretation; the soil does seem to be the most likely source of N2O, given that the turnover of N in soil is probably significantly greater than N turnover in plant tissues, on roots (the rhizoplane) or within roots. My one question here is whether or not there is a way to use mixing models to infer how much of the N2O was derived from the soil versus to N2O produced within the plant? Does the isotope value of N2O derived from in-tree processes differ enough from soil-produced N2O that you could estimate how much N2O is coming from each process? If this is possible, this would lend weight to the authors' argument. 8. Lines 493-505: I like that the authors have been bold enough to report annualised, upscaled estimates of N2O flux from their study sites, as not all investigators would have been confident to do so. Given how little data exists for African systems (and for stem fluxes in general), these kinds of upscaling exercises enable the wider flux community to understand how stem fluxes may fit into the bigger picture of regional and global N2O cycling. Even if these numbers are refined or improved upon by future field experiments, we now have a starting point or baseline to compare against. My recommendation here is that it may be worthwhile to briefly expand this section of the text to discuss the other ways this kind of upscaling could be done to derive annualised fluxes. For example, for landscapes that are spatially structured due factors such as agricultural/forestry planting patterns, topography, soil moisture, fertility, differences in soil type) spatially weighted upscaling may be another approach that could be used. This would not only signal to the reader that the authors are aware of the assumptions/potential limitations of their approach, but also provide food for thought for colleagues who might be interested in conducting similar types of studies in other regions.

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