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Biogeosciences Discussions



# *Interactive comment on* "Detailed regional predictions of $N_2O$ and NO emissions from a tropical highland rainforest" *by* N. Gharahi Ghehi et al.

**Anonymous Referee #3**

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GENERAL COMMENTS

Despite the fact that tropical forests are thought to be the largest natural sources of N2O and NOx globally, we have a an incomplete understanding of the magnitude and range of N2O and NOx fluxes across different tropical habitats. In particular, upland/montane ecosystems are particularly under-represented in process-based studies, bottom-up emissions inventories and modelling studies. This study is therefore is interesting because it seeks to model and extrapolate N-trace gas fluxes from upland tropical ecosystems in Africa where we no little about biosphere-atmosphere exchange or ecosystem N dynamics.

However, despite the great promise of this paper, it suffers from a few critical problems. First and foremost, the modelling dataset has not been adequately validated against field flux measurements. While the investigators have attempted to parameterise their model using laboratory-based incubation data, this alone is not sufficient, given the scaling issues associated with trying to link laboratory- to field-based measurements. While laboratory measures may be sufficient to calibrate response curves of N2O and NOx flux against soil moisture/WFPS (as described in the lead author's 2012 paper), it is often unclear - a priori - if laboratory incubation data will provide an accurate estimate of the mean, median and variance/range of field fluxes without direct measurements to back them up. Another problem with laboratory measurements is that they do not always account for switches and lags in biogeochemical processes, associated with changing weather, soil or other environmental conditions. A combined laboratory/field measurement approach is often required in this kind of context to ensure that the lab and field measurements are in good agreement. While I fully appreciate that it can be difficult to collect field data in some tropical locations, at the very least if the investigators were able to conduct at least a few campaign-based measurements to validate/groud-truth their lab and modelling data, this would greatly help the manuscript and study as a whole.

Second, the model results need to be stated more cautiously and with less certitude, given that they are not adequately validated against field results. This is particularly prominent in the Discussion section, where the authors make much more sweeping statements about N2O/NOx exchange than I believe that results warrant. For instance, on page 1496, lines 19-24 rather than saying with certitude that "On average the entire Nyungwe forest emits..." it would be better to couch the model findings more cautiously; for example: "On average the entire Nyungwe forest is likely to emit on the order of 439 t of N2O-N...etc."

Third, the authors have identified that their model is highly sensitive to variations in bulk density (BD) and pH, yet readily admit that BD is one of the key variables missing in



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their legacy dataset (page 1489, lines 8-11). They have indicated that they estimated BD using some kind of transfer function, but it is not immediately clear how robust this approach is for estimating BD of the soils in question. For example, did they include some kind of error propagation calculation to account for uncertainty in modelled estimates (rather than direct measures) of BD. While the authors have cited a previous publication (Gharahi Ghehi 2012 SSAJ 76:1172-1183) that discusses this method, I think - given how critical BD is to explaining the variability in their results - that they launch a more robust defence of this approach, given that there seems to be no ready substitute for empirically-derived BD numbers in my mind.

Fourth, one of the key findings of this study is that the model is unable to adequately simulate 'hotspots/hot moments' in N-trace gas fluxes, as evidenced by the poor fit between measured and simulated results for very high N2O/NOx fluxes. This tends to suggest that the model as a whole is likely to underestimate N2O/NOx fluxes, as the model isn't able to capture these 'hotspots/hot moments'. The investigators need to explore this issue more deeply in the Discussion. For example, it would be useful to know how frequently these 'hotspots/hot moments' occurred in their incubations. Can the investigators use this frequency/probability information to estimate (roughly) how much they may have underestimated N2O/NOx fluxes?

Fifth, I would like to see more comparison of the investigators' work with studies by other groups working in upland/montane tropical environments, e.g. Ed Veldkamp, Michael Keller and Whendee Silver's work in other parts of Latin America, Africa and SE Asia. The authors have tended to cite their own work, and the work of Peter Vitousek and Pam Matson, but have not been fully comprehensive in their citations or inter-comparisons.

Last, the authors need to be more careful in the editing of the manuscript, as there are a large number of typographical and grammatical errors (see SPECIFIC COMMENTS below). Other remarks or observations echo the other referees' comments (e.g. Is chemo-denitrification explicitly modelled in this study? Concerns about circularity of



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the reasoning, etc.)

SPECIFIC COMMENTS

1. PAGE 1485, LINES 8-10: "The importance of tropical forests as sink and source of carbon GHGs is relatively well known..." This is overstated; I would suggest deleting or rewriting this sentence. While we have a better understand of the balance of CO2, our understanding of CH4, CO, volatile organic C (VOCs), black C, organic aerosols and halocarbons is very poor, possibly even worse than our understanding of N2O and NOx, so this statement is inaccurate.

2. PAGE 1485, LINE 23: Line should read : and denitrification when nitrate is used as..."

3. PAGE 1485, LINE 27-PAGE 1486, LINE 3: This sentence needs to be edited as it is grammatically incorrect.

4. PAGE 1486, LINE 22: Should read "no reliable spatially explicit predictions..."

5. PAGE 1489, LINES 8-11: See comment above. How robust is this 'pedo-transfer function'? What does it actually do? Given how important BD is for the sensitivity of the model, this approach needs to be defended more robustly.

6. PAGE 1490, LINES 11-12: Another typo/grammatical error; this sentence needs to be finished!

7. PAGE 1497, LINES 6-8: Poor grammar/sentence construction. Are these data supported by field studies from the Nyungwe region, or from other tropical field sites? This is left ambiguous.

8. PAGE 1501, LINES 11-12: Grammatical error; this sentence should read "Although there is still considerable uncertainty associated with our emissions estimates, our results provide the first spatially explicitly predictions..."

Interactive comment on Biogeosciences Discuss., 10, 1483, 2013.