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Interactive comment on "Automatic high-frequency measurements of full soil greenhouse gas fluxes in a tropical forest" *by* Elodie Alice Courtois et al.

Anonymous Referee #3

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In this manuscript, the authors detail a field-deployed and field-tested system for measuring soil greenhouse gas (GHG) emissions (CO2, CH4 and N2O) from a tropical wet forest; the system leverages a commercially-available automated flux chamber system with a commercially-available CRDS analyzer. More specifically, the authors (a) outline the technical protocol for implementing such a system, (b) report the mean fluxes and variability of CO2, CH4 and N2O observed over the four-month deployment period and (c) test two chamber closure lengths to determine the most effective experimental design for capturing fluxes above minimum detectable levels. Successfully implementing such a system in the tropics is both difficult and has only been done rarely, so a technical note detailing how to do so is absolutely a contribution to the literature.

I have two general/more broad comments about the manuscript, which I detail below, and also include several more specific comments at the end of this review.

1. CONCERNS REGARDING FLUX CALCULATION PROTOCOLS

In these automated, high-frequency GHG systems, one important set of experimental design and protocol decisions govern how to calculate flux rates and screen for acceptable data points. The authors lay out fairly transparent information about how they calculated their flux rates for each gas, but I wonder if more discussion of the implications of their calculation choices is warranted. I have a few specific questions. Am I correct in understanding that the authors only calculated flux rates for all three gases based on a linear model (Pg 4, Ln 27)? If so, I wonder why they didn't consider also fitting exponential models to the CH4 and N2O fluxes, if not the CO2 fluxes. The authors themselves note on Pg 5, Ln 17 that using certain chamber closure times (which, of course, this paper is very interested in) in combination with a linear flux fit can lead to flux underestimation. Couldn't the "optimal" chamber closure time that the authors attempt to find also include some experimental designs with different closure lengths but non-linear flux fits?

Additionally, the Picarro G2308 records numerous diagnostic variables alongside GHG concentrations, including measures of moisture, temperature, pressure, etc. From methods section 2.6, I am under the impression that fluxes were only struck from the dataset if they were (a) below the MDF, (b) had an R2 for CO2 < 0.9, or (c) only struck N2O data if the SHORT R2 < 0.8. First, the authors might consider including a supplemental figure that justifies their decision not to have data quality rules around CH4, as they do for N2O. Second, like the other reviewers, I was curious as to how humidity was dealt with, since it appears that the moisture-related diagnostics weren't used to evaluate data quality. Was there a water trap that isn't marked in the instrument set-up diagram?

More broadly, as this paper aims to outline best practices for setting up this kind of experiment in the tropics, fleshing out the data management aspect of things would improve the paper, in my opinion.

2. TECHNICAL NOTE VS. DATA EXPLORATION PAPER

The aspects of this paper that serve as a technical note are novel and helpful. That said, the results and discussion section, in which the observed GHG fluxes are analyzed, is perfunctory and is relatively focused on a methodological question: what chamber closure time should be used in this system, and how can others determine what chamber closure time to use in their analogous system? I found myself wishing that there was a more robust analysis of the GHG data itself and the ecological implications of their various findings. See also my comment below about Table 2.

SPECIFIC COMMENTS

* Pg 7, Ln 30: The authors ultimately recommend a sampling protocol that rotates between short and long closure times. What is preventing them from recommending always doing LONG chamber closures and only using the first two minutes of chamber closure time to calculate the CO2 flux, thus decreasing the amount of human labor needed to swap out the system program once a week?

* Pg 7, Ln 23 / Figure A2: This figure is used to justify why LONG chamber readings weren't reliable for N2O flux estimation, but these data don't indicate that the variable fluxes are unreliable, only that they are variable and considerably larger in magnitude than the SHORT N2O flux estimates. Can an additional supplemental figure be added showing the R2 values for the LONG vs. SHORT N2O fluxes? Or some similar figure that shows why the LONG fluxes are considered unacceptable?

* Pg 7, Ln 31: "Our unique system. . ." and Pg 8, Ln 12: "this is the first time that this experimental set up is described and tested under tropical field conditions." I believe an analogous system was described in Puerto Rico (O'Connell et al 2018, Nature Communications, https://doi.org/10.1038/s41467-018-03352-3), though not as a technical

or methods paper.

* Appendix Figure A1: Might be worth including the N2O comparison just as the CO2 comparison is included even though the authors discard the LONG CO2 flux estimates.

* Table 2: A number of the authors' chambers reported mean N2O fluxes below 0. This seems worth mentioning in the results and/or discussion.

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