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Discussions

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

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RC3, Referee #3 (Remarks to the Author):

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 de-

sign 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

Response: We thank the reviewer for the positive comments and constructive inputs.

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.

Response: We first decided to use linear flux calculations only because we thought that the saturation effects characterised by a plateau after a certain time would be low. However, following your comment and comment from other reviewers, we changed flux calculations in the new version of the manuscript to use exponential estimations.

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?

Response: The standard error approach that we used (Nickerson, 2016) is a first order approximation for the MDF from high-frequency measurements and the "true" MDF is a function of the chamber timeseries fit type as well (i.e. Linear, exponential, quadratic).

Nonetheless, while the use of linear regression resulted in systematically smaller fluxes as compared to exponential regression. It is therefore recommended to initially calculate fluxes with linear regression to determine the threshold for “low” fluxes and to recalculate them using exponential regression (Korkiakoski et al., 2017).

Korkiakoski, M., Tuovinen, J.-P., Aurela, M., Koskinen, M., Minkkinen, K., Ojanen, P., Penttilä, T., Rainne, J., Laurila, T. and Lohila, A.: Methane exchange at the peatland forest floor–automatic chamber system exposes the dynamics of small fluxes, 2017.

Nickerson, N.: Evaluating gas emission measurements using Minimum Detectable Flux (MDF), Eosense Inc Dartm. N. S. Can., 2016.

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.

Response: We decided to use the same quality check for CH4 than for N2O and to only struck CH4 data if the SHORT R2 < 0.8. As you can see in the CH4 figure, the main emission or consumption peak during the 2 minutes measurements are still present.

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?

Response: We regularly checked that no liquid water accumulation occurred in the sampling tubes. Moreover, both analyzers measure also water vapor and its effect on concentration of CO2, CH4 and N2O was accounted for. We also monitored Licor and Picarro inside operating temperature: Picarro temperature remained between 44.99

and 45.01 C and Licor temperature at 51.7 C.

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.

Response: We think that we already included many aspect of quality check and data management and more information can be found in the new version of the manuscript. All the subsequent figure and analysis were done using R software and we would be happy to share the codes upon request.

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.

Response: See RC2 (5)

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?

Response: Setting all chambers as LONG measurements would have led to a maximum of $\sim$ 3 measurements only per chamber and per day. Mixing LONG and SHORT measurements allows to maximise the number of measurements per chamber and per days while ensuring a reliable estimation of the low N2O fluxes and to capture tran-

sient peaks of CH4 and N2O. Moreover, under tropical conditions, a visit once a week is absolutely necessary to ensure a proper maintenance of the setting (removing fallen leaves, branches, etc. . .).

* 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?

Response: If we understood well your question, you are asking why we considered SHORT (not LONG) N2O fluxes as unacceptable. In order to compare the fluxes from SHORT and LONG closure time, we have added a third graph to the Figure 2 (previously supplementary Figure 1). It shows that N2O fluxes estimated using the SHORT closure time are not correlated with fluxes estimated with the LONG closure time (R2 of 0.02) and should not be considered.

* 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.

Response: We realised that, although the suggested reference was in the list of references, it was not mentioned in the text and we now cite this reference in the introduction. In this reference, the authors are measuring CO2 and CH4 but not N2O fluxes. Moreover, the description of the experimental design of the automated soil fluxes measurement system in this paper is very short and does not give the specification of the multiplexer not the exact model of the CRDS Picarro analyser that they used. We therefore still believe that our study is the first one describing in detail the simultaneous

measurement of the three GHGs under tropical field conditions.

* 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 . Response: In the revised manuscript, this figure is now presented in the main text (Figure 2) and also includes the N2O.

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

Response: We agree with the reviewer and, as suggested, we have added some comments about the respective parts of the N2O and also CH4 emissions and absorptions. We have estimated that 28% of our fluxes indicated a sink for N2O and 72% a source for N2O and that 59% of our fluxes indicated a sink for CH4 and 41% a source for CH4.

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