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Interactive comment on "Smallholder African farms in western Kenya have limited greenhouse gas fluxes" *by* D. E. Pelster et al.

Anonymous Referee #2

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The paper presents a set of flux measurements measured using traditional chamber methods from an area not well accounted for in current GHG measurement budgets (rural Africa). It is true that data is required from these areas to better account for gas fluxes in the region; however the broad approach and limited number of measurements used in this paper to estimate annual flux values is unlikely to represent well the complexity of the processes occurring at such a large scale. I believe that several of the methods in the paper are flawed and interpolation of the data points is too simplistic to provide reliable tier two annual estimates without large uncertainties, which are missing throughout the study. I would encourage the authors to defend their methodologies and improve upon their uncertainty estimates before publication.

Although I have several concerns with the methodology used, the data set may still

prove valuable as a starting point to those investigating fluxes in this region. The paper is generally well written with a few grammatical errors creeping in. If the authors can justify their methods and improve on describing uncertainties in their measurements and interpolation of data, then I would consider the manuscript worthy of publishing.

Comments: Introduction 15302 L26 A reference to IPCC values/estimates may be helpful

P15303 L3 replace 2/3rds with 66 %. All other numbers in the text refer to %

Smallholder farms may account for 80 % land coverage, but do they account for 80 % fertiliser use?

What is the difference between industrial/subsistence farming inputs?

In N2O studies the N (kg ha-1) content of the fertiliser is very important. Without this it is impossible to tell the EF% of the fertiliser events for N2O. 20 Kg ha-1 of manure will have a very small nitrogen content compared to typical European fertiliser applications.

Table 1 Units should be consistent in the table.

There is no mention of the large amount of agricultural goods produced industrially in Kenya. Tea, coffee, cabbages, onions, mangoes etc. . . Although these areas are not covered in the study, the introduction describes Kenya as a sub-Saharan area with low fertiliser input and low productivity. This is not the case across the entire country.

Methods

Incubation study

The soil cores were air dried for 2 days at 30 degrees Celsius. Is this to replicate realistic environmental drying conditions or is this beyond what can be considered natural? Removing the cores from the soil and then drying and placing them in jars would change the oxygen content and aeration of the soil significantly (among other physical and biological changes). This would have a serious impact on the gas producing

BGD

12, C7757–C7761, 2015

microbiological processes which may nullify the validity of any results obtained using this method. If water was added to the samples in a single step, this does not reflect a rainfall event well and the fluxes measured immediately after will not either. If the soil incubation work is to be included in the article, the method and any assumptions made would need to be better described.

Field study

Pooling separate air samples from chambers can only reduce representation of spatial variability from the plots. With individual chamber measurements the spatial variability can be assessed statistically (or at least attempted). If GC sampling costs or time were an issue, then decreasing the sample number may have been acceptable; however, in pooling the samples any information on linearity of regression in the individual chambers or spatial variability within the plot is completely lost. Rather than deal with several large sources of uncertainty in scaling the data (spatial variability and regression), using this methodology the uncertainty in the sampling methodology becomes completely incalculable and ignored. Although this method has been published and peer reviewed it is my opinion that it cannot be used to scale fluxes spatially due to large unaccountable uncertainties and possible statistical bias. This method prevents the propagation of any kind of uncertainty in spatial interpolation when calculating cumulative fluxes.

The assumption that a loss of CO2 represents leaks in the chamber may work in general, but the pooling of samples and the inability to determine which chambers leak, or if the different gases behave differently in each chamber is a real weakness of the method.

Throwing out data with lower values than the precision of the instrument is not good practice. What is the instrumental detection limits for each gas? Is this consistent on a day to day basis or does it change? How is it calculated?

All data should be included and thus the instrumental precision is then un-biased as it is equally positive and negative. In literature CH4 uptake is generally believed and N2O

BGD

12, C7757–C7761, 2015

uptake is not. In this paper all N2O fluxes below zero seemed to have been removed while CH4 is allowed (perhaps because it is generally accepted in literature?). This can bias the results. In Figure 2 I see nothing in the CH4 measurements that can prove anything other than instrumental noise is being measured. For N2O it is impossible to tell as the axis has been cut off at zero. Were no negative fluxes of N2O recorded during the study?

How can detailed regression analysis be used if the chamber samples are already pooled? Each chamber has its own linearity for each gas type depending on conditions.

How was temperature change accounted for within the chamber? 45 min long chamber times may result in very large temperature changes during the enclosure times, especially during hot days. This can change the physics within the chamber in a way that can affect flux calculations. (Air density, expansion of air, large pressure changes, etc..)

No uncertainties were calculated for the entire flux process. This is a real weakness in the method which should be addressed if the data is to be published. Any uncertainty in scaling up at the plot scale is lost in the pooling sample method and no estimate of temporal uncertainty in linear interpolation of the measurements is even discussed. At the end, all of the plots are averaged out to give a range of uncertainty, but each of these numbers should also have a very large uncertainty associated with it. This number should then propagate through. From the results presented in this paper it is impossible to tell how precise the study has been in its estimates of annual fluxes. Is the method even fit for purpose?

Results

Figure 3 No error bars are included in any of the measurements.

Table 2: should Bulk density have units of mass per volume?

More information is required on what the CO2 measurements are actually measuring.

BGD

12, C7757–C7761, 2015

Do they contain some plants or just soil? Why measure CO2 from the chambers? What do these measurements tell you?

The weekly measurements are likely miss peaks in N2O emissions from fertiliser events which can last less than a few days. Any attempt to do a cumulative annual budget for N2O emissions should do more regular measurements at least around fertilisation dates.

Diurnal effects are not observed due to the manual chamber method being used during the day. Nocturnal emissions will have different temperatures (and light for CO2) which may affect processes in the soil. Differences in night/day fluxes in Africa may differ from those observed in the more commonly measured areas (Europe etc. . .).

Figure 4 Explain what the box plots represent. Quartiles of 59 points?

Discussion

15321 L15 Assuming that the measurements scale to a continental scale is highly optimistic.

Interactive comment on Biogeosciences Discuss., 12, 15301, 2015.