Biogeosciences Discuss., 12, C4661–C4664, 2015
www.biogeosciences-discuss.net/12/C4661/2015/

Biogeosciences
Discussions
Open Access

Interactive comment on "Organically fertilized tea plantation stimulates N_2O emissions and lowers NO fluxes in subtropical China" *by* Z. Yao et al.

Anonymous Referee #2

Received and published: 24 August 2015

The authors of this manuscript performed two years of field work on N2O and NO fluxes and their controlling factors from a subtropical tea plantation by investigating the impact of two organic fertilizer types. Comprehensive manual flux measurements (3 or 5 times per week, 5 gas samples per chamber closure, tea plants were accommodated) were conducted by means of vented closed chambers and subsequent GC and NOx analyses. Underlying standard methods and statistical analyses were convincingly performed. Uncertainties associated with the NO method were adequately discussed. This work adds valuable information to the few existing N2O data sets existing so far from tea plantations, and provides for the first time NO fluxes from such intensively managed systems. Therefore, I think that content and scientific quality of this study meet the requirements for publication in BG. However, I'm convinced that the authors could even do a better job. I have two major concerns associated with the

study design which should be clarified prior to acceptance. Some further specific or technical suggestions are of minor importance.

In the introduction, the authors state that "organic fertilization systems have been shown to substantially affect N2O emissions compared with conventional management practices…". While reading the manuscript, I asked me again and again why they have not also investigated a conventional mineral fertilizer treatment in the current study. It is not surprising that organic fertilization stimulates N2O production compared to the background of a control treatment. The more interesting question is how the organically fertilized treatments would perform in comparison with treatments fertilized with conventional mineral fertilizer. Therefore, based on their experimental design, the authors are not able to recommend one most appropriate fertilizer (in terms of N2O mitigation), because they ignored the most applied in practice. I know that additional chamber measurements are laborious, but measuring an additional treatment could have been achieved by reducing the measurements during background flux periods to one measurement per week. To address this mineral fertilizer issue, I strongly recommend expanding the discussion. Results from the literature should be discussed more specifically in the light of differences in emission levels between organic (this study) and mineral fertilization. The authors should be able to bring the community one step further regarding the question which fertilizer type would be desirable in terms of reducing nitrogenous emissions from tea plantations. This cannot be done if mineral fertilization is a priori ignored.

Second, I was wondering why the authors have not included plant yields in their analysis. If one tests different fertilizer types, it is very likely that yields will also be affected. This cannot be ignored, since the requirements of the market have to be met in such highly productive tea plantations. It will depend on the yields (and may be on quality of the tea leaves) whether an alternative fertilizer type, that potentially helps to mitigate N-fluxes, can be used in practice. Furthermore, accounting for yields would also enable calculating yield-based emission factors. If the yields for the measuring period are

available, please consider and discuss them! If not, this important aspect should be at least addressed in the conclusion section.

Specific remarks:

P11628, L16: suggest “still very few data available” P11632, L15: I guess the stability of the GC was checked by measuring these standard gas samples. Avoid “calibrated” here. Instead, you should indeed give information on the calibration procedure: Which and how many gas standards were used? How did you handle the non-linearity of the ECD (which kind of regression was used for the calibration)? P11632, L19-20: I like a flexible approach which allows for applying linear or non-linear regression for flux estimation. Which criterion did you use to decide among regressions? Please add! P11632, L20: The Wang et al. study used the method proposed by Kroon et al. (2008), which is an exponential regression. I agree that this approach prevents systematic underestimation of real fluxes compared to linear regression. However, it might be prone to large uncertainties in certain cases and it is not recommended by the guidelines of the Global Research Alliance on Nitrous Oxide (De Klein and Harvey, 2013). I therefore suggest the following: please report all the GC raw data, corrected for temperature changes in the chamber headspace, in an electronic supplement. This would offer the possibility to re-calculate the fluxes with alternative, may be future advanced flux estimation approaches and will ensure transparency of your study. Because of the great range of fluxes measured in this study, the raw data-set can provide valuable information for exercises with different flux estimation methods. Publishing the raw data would surely increase the value of the paper as well as the number of citations. P11634, L6: would prefer: “Therefore, it has to be noted that. . .” P11634, L10: I appreciate that you have measured the temperature inside the chambers. But how did you proceed with the recorded data? If you corrected mixing ratios according to temperature changes, please describe this method! P11643, L13: suggest change to: “background N2O emissions revealed by present and previous studies. . .” P11645, L2: “Based on two-year field measurements. . .” P11645, L1-18: The conclusions should be considerably

improved, since this section more or less appears in the style of a 2nd abstract. I would like to see more general conclusive remarks and still open research questions which should be tackled in future. Some ideas: the importance of temporal scales: Do you think that your work is representative in the long run? How will emissions be affected by changes in soil carbon stocks due to organic fertilization? Will organic fertilization be a feasible management option besides mineral fertilization considering demands of the market (yields, plant quality)? P11645-11654: You cited > 80 papers. Avoid too much multiple citations. Use only the most appropriate ones in order to reduce the number a references. Table 2: Please also consider the ancillary data shown in Fig. 2 here. Figure 1-3: I would omit the “15” which indicates the middle of the months.

Reference: De Klein, C. and Harvey, M. (2015): Nitrous oxide chamber methodology guidelines. Version 1.1. available at: https://www.mpi.govt.nz/document-vault/3687

Interactive comment on Biogeosciences Discuss., 12, 11625, 2015.