Dear Reviewer,

We appreciated your comments which certainly helped us to improve the manuscript. As following these suggestions, we have revised the manuscript carefully. Our responses to the comments one by one are attached directly to the following text. Please don't hesitate to contact us if any open questions do remain. Thanks a lot!

Best regards,
Sincerely yours,
Chunyan Liu and coauthors

Anonymous Referee #2

The authors of this manuscript performed two years of field work on N_2O 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 N_2O 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 N_2O 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 N_2O 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 N_2O 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.

Thanks a lot for this suggestion. In our studied region, the application of urea for

tea plantations is the local farmers' conventional and common fertilizer practice. But in terms of our survey, the compound fertilizer (NPK) or synthetic nitrogen fertilizer combined with organic fertilizer was also applied for tea plantations in some regions of China. In the future studies, we need to consider these management practices.
I am sorry for that maybe our indistinct description about fertilizer treatments misleads the reviewer. According to this suggestion, we have reworded this sentence to make it clear. That is "one with additions of urea (UN) that is the local farmer's conventional and common practice for this region…"

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.

Thanks. As a result of the young stand age of tea plants in this study, the farmer did not start leaf harvest thoroughly but seldom conducted it during the growing season. Thus, we did not measure leaf yields of tea plantations in the present study.
As following this suggestion, we have addressed this information in the conclusion section. That is "The results from this study, however, may not necessarily indicate the feasible fertilizer management option in the tea plantations, as a result of only presenting two nitrogen-trace gas species (i.e., N_2O and NO). Therefore, when we finally provide a complete evaluation of nitrogen fertilizer practice in tea plantations from an integrated agronomic and environmental point of view, future field measurements are necessary to include the climatically and environmentally important carbon- and nitrogen- trace gas fluxes (i.e., CH_4, CO_2, NO, N_2O and NH_3) as well as plant qualities and yields."

Specific remarks:

P11628, L16: suggest "still very few data available"

Thanks. Revised.

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)?

Yes, here we want to express the GC system is very stable when we analyze gas samples.

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!

The hourly chamber fluxes were determined by the nonlinear (exponential) or linear method using the N_2O concentration data during each chamber enclosure.

First, several criteria were applied to detect significant nonlinear cases. The criteria were as follows: (a) all the five N_2O concentration data were valid; (b) the concentration-time relationship could be significantly ($p < 0.05$) fitted with not only the linear function ($C = a0 + a1t$, where C is the measured concentration, a0 is the intercept, a1 is the slope of the fitting line, and t is the time), but also the nonlinear (exponential) function following Valente et al. (1995) and Kroon et al. (2008) ($C = k1/k2 + (C0 - k1/k2)\cdot\exp(-k2t)$, where C0 is the concentration at the beginning of the enclosure, and k1 and k2 are the fitting parameters); (c) the correlation coefficient of the nonlinear regression was at least 0.001 greater than that of the linear regression; and (d) the initial slope of the nonlinear fitting curve ($dC/dt|t=0 = k1 - k2C0$) was larger than the slope of the linear fitting line ($dC/dt = a1$). If these criteria were satisfied, the hourly chamber flux was determined based on the initial slope of the nonlinear regression; otherwise, the slope of the linear regression was used to calculate the flux ($p < 0.05$ and concentration number ≥ 3).

The above detailed information was described by our group's previous publication (i.e., Wang et al., 2013), so here we only cited it and did not repeat this information. That is "The N_2O flux was determined by the linear or nonlinear change of gas concentrations during the time of chamber closure, as described in detail by Wang et al. (2013)."

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.

Thanks. If the reader needs our raw data, please feel free to contact us and we would like to provide them.

P11634, L6: would prefer: "Therefore, it has to be noted that: : :"

Thanks. Revised.

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!

The air temperature measured in the chamber enclosures and air pressure obtained from the meteorological station were directly utilized in the flux computations to calculate the gas density during the sampling conditions by using the ideal gas law.

P11643, L13: suggest change to: "background N_2O emissions revealed by present and previous studies: : :"

Thanks. Revised.

P11645, L2: “Based on twoyear field measurements: : :”

Thanks. Revised.

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)?

Thanks. As following this suggestion, we have added the description about the importance of temporal variations and their controlling factors. Also, we added the information about open research questions which should be tackled in future studies.
That is “Clearly, both N_2O and NO emissions varied substantially within a year and between different years, which was chiefly driven by the fertilization events and the distribution and size of rain events.”
And “In total, the substitution of conventional urea by organic fertilizer in tea plantations significantly increased N_2O+NO emissions, and this stimulation effect should be taken into consideration when evaluating soil carbon sequestration strategy of organic fertilizer.”
And “The results from this study, however, may not necessarily indicate the feasible fertilizer management option in the tea plantations, as a result of only presenting two nitrogen-trace gas species (i.e., N_2O and NO). Therefore, when we finally provide a complete evaluation of nitrogen fertilizer practice in tea plantations from an integrated agronomic and environmental point of view, future field measurements are necessary to include the climatically and environmentally important carbon- and nitrogen- trace gas fluxes (i.e., CH_4, CO_2, NO, N_2O and NH_3) as well as plant qualities and yields.”

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.

Yes, we would check the references carefully and try to shorten the number of them.

Table 2: Please also consider the ancillary data shown in Fig. 2 here.

Thanks. For the ancillary data shown in Fig. 2 (i.e., NH_4^+, NO_3^- and DOC), we have described them in detail in the Section 3.1 Environmental variables. So we did not show them in Table 2.

Figure 1-3: I would omit the “15” which indicates the middle of the months.

For this study, we started our field measurements in mid of September, 2012. Accordingly, we took “September. 15” as the first label in X-coordinate in order to make our experimental results more clear.