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Comment

Interactive comment on “Wet-season spatial variability of N₂O emissions from a tea field in subtropical central China” by X. Fu et al.

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COMMENTS: I.1) Please elaborate in detail the major novelty of this paper compared to the study from Li et al., 2013. Simply referring to dry season in one paper and to wet season in the other is not sufficient. I.2) In addition, comparing dry and wet season should be done carefully as half an hour sampling cannot represent an entire season. I.3) It is suggested in the abstract that soil properties should be included in interpolation methods. To my opinion, such an achievement would be required before final publication. II) The paper should refer to and discuss process-based biogeochemical modeling approaches (e.g., DNDC). Which implication do the results of the paper have for existing models and process understanding (see e.g., Butterbach-Bahl et al., 2013)? III) Provide more details on the size of the static chamber (e.g., volume). Please also

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explain why the chamber volumes were not ventilated (air mixing to avoid concentration gradients)? Could this potentially cause random sampling errors that have influenced the spatial distribution of the emissions? IV) It would be important to see the spatial distribution of the error of the interpolation (kriging variance) on a map.

RESPONSES: I.1) In subtropical central China, the temperature and precipitation are very different between wet season and dry season as well as subsequent field management, e.g., fertilization and weeding, which may result in strong seasonal variations of N₂O emissions from tea-planted soils with totally different spatial structures and controlling factors. Therefore, this wet season study is a companion investigation of the dry season study (Li et al., 2013) for looking at the spatial variability of N₂O emissions from tea field soils. Compared to Li et al. (2013), the major novelties in this study may include the following three aspects. Firstly, the sampling chambers were redesigned (please see P8-9 L162-168 of Section 2.3 in the revised manuscript) to be more practical. Basically, the bases and the sampling chambers are separated. When in field operation, the chambers are clipped on the bases with sponge seals in between to stop the gas leaking. Thus, the volume of each sampling chamber is kept almost the same. Secondly, the sampling grids of this study (15 m S-N x 15 m E-W) were slightly denser than the dry-season study (15 m S-N x 20 m E-W). Finally, although we found that the spatial structures of N₂O emissions were comparable during both the dry and wet seasons (with an effective range of approximately 28.0 m and 25.3 m, respectively), the influencing factors of N₂O emissions were quite different, of which soil properties (soil ammonium, soil nitrate and soil organic carbon) and elevation had significant impacts on N₂O emissions during the wet and dry seasons, respectively. In addition, the total amount of N₂O emissions in the wet season was almost seven times more than that during the dry season for a 30-min snapshot (10:00-10:30 a.m.) over the studied 4.0 ha tea-planted catchment.

I.2) Yes, we agreed that a 30-min measurement cannot fully represent the N₂O emissions for an entire season. However, the main objective of this study was to investigate

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the spatial structure and controlling factors of N₂O emissions from tea-planted soil, while the seasonal variation of the amounts of N₂O emissions was regarded important, but less concerned.

I.3) Yes, we totally agreed. In the cokriging interpolation approach, soil properties such as soil organic carbon, soil ammonium-N and soil nitrate-N (which had significant correlations with N₂O fluxes) were chosen as the co-variables for estimating the spatial distribution of N₂O emissions (see Sections 3.3 and 4.3 in the revised manuscript).

II) The main objective of this paper was to explore the spatial structure and controlling factors of N₂O emissions from tea-planted soils over a catchment. The outcomes of such a research may help us to design the static chamber positioning scheme when a continuous measurement of N₂O emissions from tea fields is carried out, and provide a more accurate estimation of N₂O emissions at regional scales. Although the main controlling factors of N₂O emissions from tea fields found in this study were helpful for the relationship development, there were few direct contributions to understand the biogeochemical processes simulated in the ecosystem models such as DNDC and DAYCENT.

III) The static chambers were divided into two parts: base and chamber. The base was 0.15 m in diameter and 0.05 m high. The chamber was 0.15 m in diameter and 0.15 m high. In the field operation, the base was gently inserted vertically into the soil, and the chamber was clipped on the base with the sponge seals in between to stop gas leaking. Therefore, the effective static chambers volume was equal to the chamber volume of 0.002651 m³ (please see P8-9 L162-168 of Section 2.3 in the revised manuscript). Theoretically, these static chambers should be ventilated for balancing air pressure at operation. However, our pre-experiment of gas sampling had proven that the ventilation of the chamber had little impact on the N₂O concentration gradients during a short incubation time period of 30-min. Therefore, to avoid the unnecessary complexity of the gas sampling chamber, the ventilation device was not applied in our study.

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IV) Yes, we agreed. A new kriging variance map (Fig. 10) for Fig. 9 was produced and its relevant description was also made in the revised manuscript (please see P14 L289-290 in Section 3.3 in the revised manuscript).

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/12/C1158/2015/bgd-12-C1158-2015-supplement.zip>

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