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Biogeosciences Discussions

Interactive comment on "Continuous measurement of soil CO_2 efflux in a larch forest by automated chamber and concentration gradient techniques" *by* N. Liang et al.

Anonymous Referee #1

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General Comments: After I read through the revised ms, I don't see that authors have addressed all my comments and other reviewers' adequately. My main concerns are;

1. Out of focus of this ms. I couldn't find the objectives of this paper. It seems not very clear to me that authors try to compare the two techniques (chamber vs gradient), or try to study the annual sum of soil respiration, or try to study the seasonality of Q10. 2. As I pointed out in my 1st review, there are quite few issues in the methodology used in the research. I don't think authors pay enough attention to all the requirement to make the accurate soil CO2 flux measurement (see review by e.g. Livingston and Hutchinson, 1995; Hutchinson and Livingston, 2002; Davidson et al., 2002; Rochette

and Hutchinson, 2005). 3. Also I am questioned the validity of gradient technique to estimate the soil CO2 flux, although the method gained some popularity recently. The equation (2) (Rs=-DsdC/dz) used by the author assumes no source or sink term for CO2 between the two CO2 probes. But we all know this assumption can't be held in the soil profile, esp near the soil surface. So my question is; how can you account for the source term above the upper probe or between the two probes in the equation (Eq. 2) when you use this method to estimate the soil surface CO2 flux? How do you account for the influence of changing in soil moisture in the field if you measure the air-filled porosity in the lab (Eq. 3)? As we all know air-filled porosity changes as volumetric water content change. 4. Again I still have some difficulties to believe the seasonal change of Q10. I think the variation of Q10 most likely was due to the mismatch of temperature measurement and location of soil respiration or due to different soil moisture content across the whole season, not likely due to temperature sensitivity of microbial activity over the season. See (Davidson et al.,2006) for more on this.

Based on my comments listed above, I would reject this ms.

Reference:

Davidson, E. A., K. Savage, L. V. Verchot, and R. Navarro (2002), Minimize artifacts and biases in chamber-based measurements of soil respiration. Agric. For. Meteorol., 113, 21-37. Davidson, Janssen, Luo. (2006). On the variability of respiration in terrestrial ecosystems: moving beyond Q10. Global Change Biology. 12, 154-164. doi: 10.1111/j.1365-2486.2005.01065.x Hutchinson, G. L., and A. R. Mosier (1981), Improved soil cover method for field measurement of nitrous oxide fluxes. Soil Sci. Soc. Am. J., 45, 311-316. Hutchinson, G. L., and G. P. Livingston (2002), Gas flux, in Methods of Soil Analysis: Part 1. Physical Methods, edited by J. H. Dane and G. C. Topp, 3rd edition, pp. 1159-1182, Soil Science Society of America, Madison, WI. Livingston, G. P., and G. L. Hutchinson (1995), Enclosure-based measurement of trace gas exchange: Applications and sources of error, in Biogenic Trace Gases" Measuring Emissions from Soil and Water, edited by P. A. Matson and R. C. Harriss, pp. 14-51,

Blackwell Scientific Publications, Oxford. Rochette P., Hutchinson, G.L., 2005. Measurement of soil respiration in situ : Chamber techniques. pp. 247-286. In: Hatfield, J.L., Baker, J.M., (ed.) Micrometeorology in Agricultural Systems. Agronomy Monograph No. 47. American Society of Agronomy, Crop Science Society of America, and Soil Science Society of America. Madison, Wisconsin, USA

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