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Biogeosciences Discussions

Interactive comment on "Greenhouse gas emissions from the grassy outdoor run of organic broilers" *by* B. Meda et al.

Anonymous Referee #1

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Review on MS bg-2011-426

Greenhouse gas emissions from the grassy outdoor run of organic broilers B. Meda, C. R. Flechard, K. Germain, P. Robin, M. Hassouna, and C. Walter

submitted to BG

The manuscript (MS) presented describes measurements and modeling approaches leading to a short term greenhouse gas balance (GHG) of an 'organic' poultry rearing system with a large, grassy outdoor run for the broilers.

General comments: The MS speaks of an impressive repertoire of data analysis techniques and competent use of language. The database itself is relatively narrow, but contains valuable information worth publication. Nevertheless, there are some impor-

tant issues that need the authors' attention. Below please find an incomplete list of those most important to me. Please amend mentioned issues before resubmission.

Specific comments: Most importantly, the MS is much too long. Not counting References, Tables and Figures it already has c. 10500 words, with an overall count > 16000. On top of this there are 13 Tables and Figures! Half of that extent should do. The length comes from two sources: First, starting with Mat & Meth, there is both too much unnecessary detail (non-essential information) and the wording is too elaborate. The reader deserves a more condensed account. The second cause touches the scientific content. For example, the Results section 3.3 contains a 1625 words section on temporal gap-filling functions alone. I suggest that a few essential lines of this would suit the discussion well, if the topic of the paper should really be the GHG balance and not a technical note. The Discussion section 4.3 (> 1700 words) contains many explanations and calculations that are completely Mat & Meth and Results material.

Also, I have a hard time with the CO2 part of the MS. The study concludes that the chicken run is a substantial short term sink for GHG. This highly important finding is based on a large ecosystem C gain through assimilation (GPP). Ironically, GPP was not measured in this work. Instead the positive CO2 GHG balance is derived from an estimate of chicken droppings and a short-term change of the soil organic carbon (SOC) content of c. +380 g C m-2 yr-1. This is certainly < 3% of the total SOC. Despite the fact that it is very difficult to measure such small changes in SOC significantly different from zero, there is no information in Mat & Meth or in Results to show how this very important bit of information was gained?! Compared to the ca. 12 kg C m-2 the soil may have accumulated in the last 10000 years, a rate of 0.4 kg C in 1 year demands an explanation. Thus, it takes a good argument why the authors assume a SOC balance other than zero. If that much more conservative approach was chosen instead, the NGHGE would be about zero, which was a surprising result, too. For the above reasons I suggest to separate much more clearly between reliable data from the Results section and some more speculative numbers, sparingly used in Discussion, to put measured data into perspective.

It seems there were no replicate gas exchange measurements per frame/site. No statistical method for data evaluation is quoted. Concentration changes of GHG in the static chambers were measured four times in 30 minutes. That is a very long period, particularly for CO2 that may have similar concentrations in the chamber and in the soil at the end of the measurement period. For that reason it is necessary to use the linearity of the regression used here to assure the quality of the measurement. If the linearity is not close to perfect (> 90%), then the measurement is of no value because the chamber has altered (suppressed) the GHG efflux. This additional information is required in section '2.3 Flux measurement technique'. Publications 'in preparation' should not be in the reference list.

The authors mention the extremely high variability of fluxes between measurement days. I find that interesting, too. But no word is lost in Discussion on apparent inconsistencies that challenge the quality of the measurements. E.g. the maximum CO2 flux (3.1 μmol m-2 s-1) in the WS batch is found at max. soil T (13.9°C) and 100% WFSP. Either WFSP is overestimated or ecosystem respiration at c. 3μmol CO2 flux does not exhaust soil O2 availability. The similarly large max. CO2 flux in the SA batch (3.3 μmol m-2 s-1) is found at low 16.5°C and only 38% WFPS. But five of the other eight measurement dates of SA batch have lower CO2 fluxes despite higher soil temperatures and higher water availability. Similar situation with N2O: the largest flux is five times larger than the second largest flux. Why? Please explain.

Not a single error value on 47 pages of MS. Please change.

Technical corrections: Omitted. Proofreading should not be the reviewers' job and be reserved to MS closer to publication status.

Interactive comment on Biogeosciences Discuss., 8, 11529, 2011.