

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

Anonymous Referee #2

Received and published: 29 January 2012

Evaluation of " Greenhouse gas emissions from the grassy outdoor run of organic broilers" by Meda et al.

This study presents, apparently for the first time, flux estimates for CH₄, N₂O and Reco, and a full GHG balance calculation, of an outdoor area used for poultry production. On the basis of 19 flux measurement campaigns with static enclosures, conducted mainly in connection with two production batches under winter-spring and summer-autumn conditions, the authors calculate annual fluxes of CH₄, N₂O and Reco by different methods, and the full GHG balance is estimated using production data and informations from a variety of sources.

The system studied includes a house where the poultry would spend part of the time, but also an upward slope away from the house. The experimental approach to cover

C5512

this spatial and temporal variability was a stratified distribution of sampling points and spatial integration by geostatistical methods. This approach is commendable, as are the efforts to determine overall fluxes using different methods. The study does have limitations in the sampling strategy and collection of supporting data, and there is certainly scope for further improvements of flux estimates in future studies. Still, the work provides a comprehensive analysis of a little known aspect of poultry production and could be published following proper revision.

Specific comments: p.11534, l. 2 - Please specify sampling time here. It is stated on p. 11545 to be afternoon, which certainly is not the optimal representation of daily mean temperature, and this bias should be clear to the reader (and potential consequences for flux estimates discussed).

p. 11534, l. 13 - It is relevant to comment on the occurrence of any non-linearity in trace gas accumulation. Poultry manure excreted would be deposited at the surface, indicating that sources of N₂O and CO₂ would also be highest near the surface which theoretically should give a high potential for chamber-feedback.

p. 11534, l. 15 - How well did the data on soil WFPS, which were based on TDR measurements at 5 cm soil depth outside the fenced in paddock, represent WFPS inside the paddock? This is a relevant concern condisering the variation in vegetation, animal traffic and elevation. It would have been feasible to install TDR probes near the sampling points for manual verification of soil moisture trends during measurement campaigns, or to estimate soil moisture otherwise. I miss some critical reflection on this source of error. Reference could be made to the extensively studied cattle-overwintering area in the Chech Republic (Simek and others).

p. 11535, l. 20 – Do you mean “top left corner of the map”?

p. 11536, l. 1 – Please change to “both interpolation methods”

p. 11539, l. 18 – Why are CO₂ fluxes reported in molar, not mass units? It would

C5513

be facilitate a comparison with literature on other grazed systems, which is relevant as these are drawn upon in the modeling of NEE.

p. 11539, l. 28 – “is” should read “was”

p. 11541, l. 2 – I believe a discussion of sources of N₂O would be in place (in the discussion section), and it does not appear suitable to deal with the important topic by reference to a general (older) review of processes behind N₂O emissions. A poultry paddock such as this is a highly specialized system with respect to distribution of C and N sources, vegetation cover and, probably, strong gradients in pH with higher pH in the most impacted areas. Both nitrification, denitrification (and nitrifier-denitrification) may be sensitive to pH, and the response of both processes to this particular environment is really difficult to predict, and should be investigated further. At this time I should like to see a short paragraph highlighting the potential importance of C and N concentrating at the surface, and interactions with pH.

p. 11546, l. 20 – The authors calculate N₂O emission factors based on N excreted, and without correcting for NH₃ losses which are stated to range potentially up to 60% of N excreted. This is in accordance with the recommendations of IPCC for the Tier 1 approach. However, the authors also used a “mechanistically grounded gap-filling algorithm” based on soil temperature and moisture (p. 11541, l. 8) to model emissions between measurement campaigns, which is moving into a higher Tier. The potential for NH₃ losses is also, like N₂O, a function of temperature and moisture at the soil surface. It would be useful to briefly discuss the implications of disregarding NH₃ losses.

p. 11547, l. 4 – In the discussion of CH₄ oxidation it appears relevant to make a reference to the fact that mineral N is often shown to inhibit methane oxidizing bacteria.

p. 11547, l. 24 – “more than”?

p. 11548, l. 18 – “a” should read “an”

p. 11549, l. 16 – The argument that microbial processes behind N₂O and CO₂

C5514

emissions were probably different because the former varied 100-fold and CO₂ only five-fold is not valid. It could be true if N₂O was predominantly a product of ammonia oxidation, although this is not very likely. The N₂O:N₂ product ratio of denitrification varies dramatically depending on oxygen availability, pH and distance to the surface, and the difference observed could very well be a result of such mechanisms. I suggest that you either avoid this discussion or address it in more detail.

p. 11551, l. 12 – FN₂O was defined as ng N₂O/m²/d on p. 11541, l. 18. It is not consistent with the definition given here.

p. 11551, l. 21ff – I must admit that I do not find the extensive calculations to estimate NEE for the outdoor area convincing. There are many assumptions made, for example that C deposition during the batch production cycles can be considered as C sequestration, and C leaching data obtained from very different grazing systems. In the present study, due to gradients in elevation and excretal depositions, there were clearly areas with a high potential leaching. Also, there is no description of how the ranges in daily soil and vegetation intake (Table 5) were taken into account in the modeling. In my view, the calculation of a net annual GHG budget of the outdoor run does not strengthen the paper, which is already too long.

p. 11555, l. 8 - The authors point to EC measurements as the most pressing research need. It is difficult to see how such measurements can be adopted in this system where significant spatial heterogeneity in soil conditions and associated emissions is directly linked to the presence of the housing facility. It should be discussed how such measurements could in fact be adopted. I see a much greater scope for improvement of flux estimates by improving the model of emissions by extending the approach adopted here, but including information on pH and actual soil moisture conditions near the soil surface.