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Interactive comment on "Earthworm impact on the global warming potential of a no-tillage arable soil" *by* M. Nieminen et al.

Anonymous Referee #2

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This study investigates the effects of earthworms on GHG emissions by combining field and laboratory measurements of areas with and without earthworm activity. Three gas emissions (CO_2, N_2O and CH_4) have been monitored together with some physic-chemical and abiotic parameters. The main strength of the paper is the fact that part of the experimental work has been performed under field conditions, since the majority of the evidence has been derived from laboratory incubations. Indeed, one of my main objections to Lubbers et al. 2011's paper is the fact they have only included in their review results from plant-free (!) laboratory incubations.

This paper, therefore, contributes to the current debate of whether earthworms can actually be responsible for global warming when in the past they have been considered the good players in ecosystem functioning. For this reason, I think any new conclusions to be added on the matter should be sound and based on good experimental approaches to avoid more controversy on the issue.

Firstly, I would like to highlight that the paper focuses on just one species, L. terrestris, and this should be clearly reflected in the title, otherwise it gives the impression that it sells more than it actually does. Secondly, another concern is that that the non-midden area was not worm free, i.e. although significantly more worms were collected from the midden area, a good number of worms were also present in the reference treatment (Table 4 and P6337 L17-18). This makes the comparisons with the laboratory experiment, which included a treatment without L. terrestris, more difficult.

Specific comments regarding the different sections: - Introduction: For non-specialists readers, it would be useful to clearly state that the work by Lubbers et al. 2011 was actually based on laboratory results (P6327). This would add more value to the research presented here. A reference is needed to support the statement given in P6328 L4-5. If L. terrestris is the second most abundant earthworm in arable fields in Finland (P6328 L7-8), which one is the first one and how can you then justify the use of L. terrestris in this study?

- Methods: I do not understand why two sites (A and B) were selected when the results for the site effect are only shown for CH_4 emissions. What is the relevance of this? Were these two areas different in relation to earthworm densities? What do you mean by 'they had no obvious difference in soil properties' (P6330 L1)? A reference is needed to support the statement given in P6329 L25-26. One important concern regarding the gas measurements performed in the field is that they were taken immediately after the rings were inserted into the soil; this is known to decrease CO_2 fluxes due to the 'lost' autotrophic flux (root-derived) component (Heinemeyer et al. 2011). In addition, when fitting the chambers an important amount of CO_2 gets trapped inside (this also applies to the laboratory measurements). Finally, air temperature was used to correct gas fluxes; but was this parameter measured inside or outside the chamber?? Finally, midden and straw samples were stored (freeze at -18oC) for 7.5 months which seems to me to be a very long period... In my opinion, all these aspects should be mentioned in the discussion since they could have affected the results. In relation to the laboratory incubation experiment, my main concern is that the mesocosms were filled with sieved soil which is known to increase substrate availability for microbial populations (Hartley et al. 2007) and again making the comparisons with the field experiment difficult. Additional questions regarding this experimental set-up is whether you achieved the same bulk density recorded in the field by compacting the soil in the mesocosms and whether the selected incubation temperature mimics the one recorded in the field site during the autumn period. Finally, the statistical analyses of the data seem to include excessive complications. The experimental design is a full factorial layout with two factors (treatment and sampling date) in which time is a repetitive factor. Performing a Repeated Measures of ANOVA is the appropriate method to analyse the two data-sets (field and laboratory results) and therefore, I cannot understand the need for developing a model to estimate gas fluxes from the different treatments when the real data could have been presented instead.

- Results: This section is very difficult to read because the graphs and the majority of the tables are based on model estimations instead of on real data. For example, looking at Table 4 it is very surprising that the mean values with overlapping 95% CI are truly significant (see for example, soil moisture). In contrast, how can the mineral N content of the straw be so different between midden and non-midden areas? Another important concern is the number of slugs in the midden treatments; this aspect is totally ignored in the discussion and although, I am not an expert on the role of these molluscs in SOM decomposition, it makes me wonder whether, for example, the mucus they secrete could also promote microbial activities and be also responsible for the 'worm' effect. The fact that no information is provided about microbial populations in these treatments makes it difficult to establish whether the treatment differences are due to the worms or to their indirect effects on microorganisms. One way to tackle this would have been to subtract the control fluxes from those derived from the worm treatments to calculate the contribution of the worms. In the case of the laboratory experiment,

the main aspect I would like to highlight is the higher flux values; especially those for CO_2, when compared to the results obtained in the field (see Table 2). I think this can be explained by the fact that the soil was sieved which facilitated microbial access to C and N substrates. On the other hand, the observed decreases of these fluxes over time (P6338 L9-10) could be explained as a result of microbial acclimation or substrate depletion (another topic hotly debated in the literature). All these aspects should be considered in the discussion section.

- Discussion: Overall, I found this section too long and difficult to follow. I think it can be greatly shortened by summarising and combining paragraphs. For example, the first 25 lines in P6342 can easily be summarised and integrated in previous paragraphs. I would advice to follow a logic structure and discuss all those aspects regarding a particular flux together and not scattered throughout this section. I would also like to clarify that the changes that earthworms produce in their environment are actually 'direct effects' (P6342 L1-3). The fact that when you remove the worms from the system, the effects still persist proves that they are direct. Indirect effects are, for example, when their impact on gas fluxes is mediated by a third (e.g. microorganisms), which I think is what happens here, that microbial effects are more important than the direct effects of the worms.

In conclusion, I think this paper could make a good contribution to the journal if the fact that the whole study focuses on just one earthworm species is made clearer both in the title and discussion and consequently, the description of the relevance of the results is toned down. In my opinion, this study does not resolve the issue but highlights the importance of performing field measurements and that laboratory incubations tend to magnify the results.

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