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Interactive comment on "Soil Denitrifier Community Size Changes with Land Use Change to Perennial Bioenergy Cropping Systems" *by* K. A. Thompson et al.

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The manuscript from Thompson et al. is generally well structured, concise and informative. Results from their research, with higher biomass production from miscanthus but lower N2O flux, have great potential for soil science, agriculture, economics and climate change mitigations if their results can be further validated in future studies. As the authors mentioned in Conclusions, future measurements of N2O fluxes and other relevant N cycling processes is critical in linking microbial communities to actual N2O mitigation benefits during land use change. N2O fluxes are highly variable, which raises my concern on how to interpret the information from soil denitrifier community size. Is 4 time samplings (May 9th, 2011; October 30th, 2011; May 2nd, 2012 and

October 20th, 2012) enough to represent the link between soil denitrifier community size and N2O fluxes, to differentiate seasonal changes?

Thank you for this comment. We believe that the timing of our sampling encompasses both seasonal changes and changes that may occur due to cropping system/management practices. Our objective was not to directly link N2O fluxes to these communities, but rather to assess whether biomass cropping systems and their management influenced the size of the denitrifier communities (ie the functional potential of these communities). Our sampling approach gave results showing significant changes in these communities based on cropping system and their management, validating our sampling choices.

Specific comments: 1, lines 61-65, confusing, need to clarify 2, lines 61-72, would it be better to add the reason why focus on N2O?

Wording of lines 61-65 have been edited to clarify (comment #1) and a sentence at the start of this paragraph has been added to provide linkage between N2O and denitrification (comment #2): "N2O is a potent greenhouse gas with a global warming potential 296x that of CO2 (IPCC 2007). However, measuring N2O directly in the field is often difficult with chamber methods in cropping systems that produce large aboveground biomass. Additionally, including multiple field treatments (eg: RCBD design) make micrometeorological methods of N2O flux impossible to obtain. Instead, relative abundances of denitrifier genes can be used to assess a soil's potential to produce (e.g. nirS or nirK) and consume (e.g. nosZ) N2O via denitrification, representing a qualitative proxy of relative N2O emission potential of a soil (Butterbach-Bahl et al., 2013; 71Hallin et al., 2009; Morales et al., 2010; Petersen et al., 2012; Philippot, 2002). Denitrifier community size has been correlated with denitrification process rates (Hallin et al., 2009; Wu et al., 2012), and denitrification potential (Attard et al., 2011; Cuhel et al., 2010; Enwall et al., 2010). Potential denitrifying activity and denitrifying community size have also been shown to be correlated with each other in some studies (Hallin et al., 2009; Morales et al., 2010; Szukics et al., 2010; Throback et al., 2007); suggesting

community size may indicate potential differences in soil N processes after LUC. Particularly, the nosZ-bearing community may act as a N2O sink and counter high N2O production rates (Braker and Conrad, 2011; Philippot et al., 2011), therefore influencing N2O emissions (Cuhel et al., 2010; Morales et al., 2010; Philippot et al., 2011)."

3, line 97, there are only two N fertilization rates, 0 and 160 kgN ha-1, “multiple” is not appropriate Within the overall field trial, there are 4 N fertilization rates (0,80, 120 and 160 kgN ha-1). We chose two (unfertilized and 160N) for assessment within our study.

4, line 115, add . after) Thank you, done.

5, lines 119-120, N fertilization rates are confusing, “46-0-0” and “34-0-0” need further explanation Thank you, we will add in “N-P-K” to denote chemical make-up.

6, line 127, capital words in subtitles are not coherent Thank you, we will address this.

7, lines 155-156, strange position under 2.2 Soil sampling, suggest relocate to 2.1 Site Description We agree, we will move this section accordingly.

8, lines 234-235, no context for Ho Thank you, we will address this.

9, line 249, please explain “S. Ontario” Done, we will write out “Southern Ontario”.

10, lines 243-256, authors refer to Roy et al. 2014 for result of environmental conditions instead of Fig.1. Are precipitation and temperature taken from Roy et al. 2014 ? If so, it would be better to also mention it in the Figure caption. If only soil moisture is measured, it would be better to descript soil moisture conditions instead of only mention that soil moisture “could also impact soil N and soil bacterial communities”.

Thank you, we will edit for clarification. We use Roy et al. (2014) in text for 30 year average data, and whereas the data in figure 1 (precipitation and temperature) was collected from the Elora Research Station over the 2year study.

11, line 275, . after $p<0.05$ Thank you, done.

12, line 339, “years 2 and 3”, please specify what 2 and 3 refer to Thank you, we will clarify this in the text – we are using ‘year 2 and 3’ in reference to years after LUC, so year 2 and 3 of miscanthus/switchgrass growth. We will fix this accordingly.

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