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Interactive comment on "Characterisation of NO production and consumption: new insights by an improved laboratory dynamic chamber technique" *by* T. Behrendt et al.

Anonymous Referee #2

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This is a very long paper, 56 pages, 23 Figures and 2 tables and supplements. Splitting this paper into a purely technical paper, describing the improved dynamic chamber method, and a second paper applying these methods to the different soils identified in Tables 1 and 2, may be more attractive to the audience. The layout of the paper is not ideal; results & discussion sections both contain result and discussion material, so you may as well join them fully. The conclusion section is far too long, the key conclusions are lost in too much peripheral detail.

The technical part of the paper describes in great detail a sophisticated automated laboratory system for measurements of NO production and consumption, and also VOC, CO2. The design, physical properties of the chamber inc. precision, error analysis are

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Discussion Paper

described and equation for calculating fluxes, error terms etc are provided. This part of the paper merits publication.

What is not terribly clear is the sequence of sampling from the six soil chambers and how the 4 experiments were conducted on one soil sample, rather than different samples. You may like to include a timeline to clarify this point.

The soil samples were taken from different countries, so you should describe how you transported your soils from i.e. Mongolia to Germany in a constant 4 degree environment. Your experimental design would lend itself to test the optimal volume of soil, column width and height and flowrates required for these laboratory experiments. Have you done this? To represent conditions in the field one normally repacks the soil to the bulk density measured in the field. It is not clear if you did this. Failing to do this may influence your results. I am interested to see that you did not convert your soil moisture values to water filled pore space. Why did you choose not to do so?

The different relationship between NO flux and soil moisture for your wide range of soil samples are very interesting and it would be great if you could link these results to the soil physical and chemical properties of your soils, in a second paper. However, speculating about possible microbial pathways based on these very simple experiments and very limited soil data is risky. There are usually several possibilities and just picking one microbial process is misleading and will lead to reference misuse. Here are some examples: a) You state that low nitrate concentrations in the Finthen grassland soil suggest heterotrophic denitrification(p1232 line 20,21). However, low nitrate concentrations may also be due to low nitrification rates, NO3 leaching or uptake by plant. b) You imply that 'high CO2 release rates' point to ' the dominance of heterotrophic processes' (P1232 line 10 -13). However, autotrophic and heterotrophic processes occur simultaneously. Large soil respiration rates are indicative of a general large microbial activity, including organic matter mineralisation. Autotrophs benefit from this supply of mineral N. c) 'High ammonium and nitrate contents of the EGER spruce' are indicative of 'heterotrophic nitrification' p1232 line 13-20). The difference between the EGER

BGD

11, C1655–C1657, 2014

spruce and EGER blueberry soils may be due to differences in N deposition rates if the blueberry soil was collected from a clearing, or simple spatial heterogeneity of soil mineral N content. The large mineral N presence is due to wet AND dry deposition, so please change 'precipitation' to atmospheric N deposition'. d) The statement (p 1234 line 29) that 'Orlando (2012) found low abundance of denitrifiers in dryland soil' is incorrect. They observed a low diversity of denitrifiers. e) Ammonium is an essential substrate for both autotrophic and heterotrophic nitrification. So the sentence (p1235 line 1-2) ' Due to very low ammonium contents, limited NO production within those soils is most likely by autotrophic nitrification' is misleading. f) (p 1239 line 2 onwards) 'Following Dunfield and Knowles (1998), there is evidence that the organic carbon content of soil and the concomitant evolution of CO2 are good predictors for soil NO consumption'. You may have taken this statement out of context. The main variables prediction NO flux are mineral N supply and availability, soil moisture and soil temperature.

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