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Interactive comment on "Gaseous nitrogen losses and mineral nitrogen transformation along a water table gradient in a black alder (*Alnus glutinosa* (L.) Gaertn.) forest on organic soils" *by* T. Eickenscheidt et al.

Anonymous Referee #3

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This study looks at gaseous nitrogen losses and mineral nitrogen transformation in a black alder forest. Authors combined a field and a laboratory experiment in order to estimate NNM and gaseous N emissions from soils where water table varies significantly. Additionally, in the laboratory experiment, they also aimed to measure the effect of temperature and soil water content on denitrification and its product stoichiometry in two soil types (U and D2). The study is competent and has great value to understand the effect of land-use change on environment and the subject fits well to the journal. However as it stands, the paper has number of serious problems that needs to be im-

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proved before being accepted for publication. Especially interpretation of the data and discussion is very weak. Specific comments: Title suggestion: Nitrogen mineralization and gaseous nitrogen losses from waterlogged and drained soils in black alder forests Introduction: Authors may add information (ref) about previous findings on the relationship between temperature and soil denitrification rate and its product stoichiometry. Second objective needs to be revised as authors did not test N2O emissions and the factors regulating the N2O emissions along a soil moisture gradient. Matherials and methods Section 2.1 needs to be better structured. Please first give general information about the site and ii) introduce each field sites and describe their physico-chemical properties separately (you may add sub-sections for each site, e.g. 2.1.1 U). Please be precise and try to avoid unnecessary wordings. I should remind that N2O measurement intensity (every second week) was very low. As authors also indicated, they most likely missed number of important N2O peaks, as extreme N2O peaks normally lasts very short (a day or two). Therefore, I recommend to avoid strong conclusions about the low annual N2O emissions. For me 120 min chamber closure time (for tall chambers) sounds unreasonable. Can you please discuss about the linearity of the N2O and CO2 measurement (or show an example). Section 2.3: Laboratory incubation experiment: It was difficult for me to understand how many soil cores were taken for each analysis. Please divide this section to two or three part, e.g. you may first introduce sampling for WFPS determination and adjustment. Please explain soil sampling for incubation trial and procedure of incubation experiment seperately. I still don't know how many replication (incubation vessels) have been used for the trace gas fluxes during the incubation experiment. Can you please give information about the background N2 concentration and its variation? Most importantly, I don't think that the method authors used can enable them to compare the effect of temperature and soil types in the incubation trial properly, because of two important reasons: âĂć I assume that soil NO3- content differs significantly among soil types at the beginning of the experiment: Therefore, soil type comparison (their potential denitrification and product stoichiometry) is very weak (unless they washed soils with water, or adjusted soils NH4 and NO3 levels

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prior to the experiment) . âĂć Secondly, soil NO3- content may deplete significantly at the microsites where denitrification occurs. During the course of the experiment, temperature was increased gradually (every 24 hours) and high soil temperatures were tested 3 or 4 days after onset of treatments. Therefore, direct comparison of temperature effect on denitrification process is not possible as soil No3 content will differ drastically at each stage where temperature effect was tested (please note that soil NO3 content is one of the most important variable that affect denitrification rate and its product stoichiometry). Latter may explain why authors did not observe significant N2 or N2O emission from U soil. Please report soil NO3 and NH4 content before and after the incubation period. Five day is a long period, soil NO3 can be depleted fast especially at the microsites where denitrification occurs. Thus, it is difficult to justify the effect of temperature on denitrification and its product stoichiometry. I assume that soil from D-2 had much higher NO3 content during the course of incubation (therefore higher N2O emission was not surprising). If possible please show time course data.

Discussion Page 19087 L21: I do not expect big difference in soil temperature when comparing 0-10 and 10-20 cm soil layers. However aeration may be key for explaining low N turnover. Page 19090 L4: I do not agree with the conclusion that losses of N2O are only of minor importance compared to N2 losses under water satured conditions as latter depending strongly on NO3- concentration in soil solution. Even under complete anoxic conditions, N2O/N2 ratio may be reasonably high. Page 19090 L28: I also assume that N2O losses may be negligible at the undrained site during wet seasons (due to low NO3 concentrations). However even in wet seasons, large N2 emissions may occur due to complete denitrification (as NO3 is low). Latter may be due to significant nitrification activity at the soil surface (which is difficult to detect as it may be immediately denitrified).

Interactive comment on Biogeosciences Discuss., 10, 19071, 2013.