We are very thankful to the anonymous reviewers for their detailed, constructive and positive feedback. We agree with most of their comments and will alter the manuscript accordingly.

Anonymous referee #1:

1) Referee#1: Your title includes the words 'water table gradient', but this is not reflected in the text. You focus on the differences between undrained / drained. Please include the differences/similarities between D1 and D2 in your paper, in particular in the discussion section.

Authors: We agree. The original title was "Gaseous nitrogen losses and mineral nitrogen transformation in drained and undrained black alder (Alnus glutinosa (L.) Gaertn.) forest on organic soils". Therefore the manuscript was more focused on the differences between undrained and drained. However the Editor recommended us to change the title because in her view the terms "drained" and "undrained" alder forests are misleading as all sites are located in a drained alder forest and the undrained parts are due to some wet hollows. She suggested to use "along a water table gradient" or a similar expression instead. However, due to the only small differences in GW between the two drained sites, it seems to be more meaningful to focus on the differences between drained and undrained. Therefore we change the title in "Nitrogen mineralization and gaseous nitrogen losses from waterlogged and drained organic soils in a black alder (Alnus glutinosa (L.) Gaertn.) forest", as the Anonymous referee#3 suggested.

Additionally we include following sentences in the manuscript, to concretize the differences/similarities of the two drained sites.

P19087, line 1: "However, the distinct differences in the C and N contents of the drained sites resulted not in significantly different NNM rates in the present study."

P19089, line 19-24 "However, in the present study, observed differences in the GW levels were only small between the drained sites, resulting in comparable annual N_2O emissions."

2) Referee#1: Field N_2O emissions are supposed to be shown in Fig 1 d, 2d. These figures do not exist. Field N_2O data are only shown in Fig 5. I hope these missing figures will provide the missing information on the regression model you allude to in the discussion section (p 19089 line 19/20). Make sure you will describe them fully in a short paragraph in the result section.

Authors: N_2O fluxes were mislabelled with Fig 1d and 2d. Instead N_2O fluxes were only presented in Fig 5. For describing the N_2O regression model, we include a new figure were the model equation is given in the figure caption as followed:

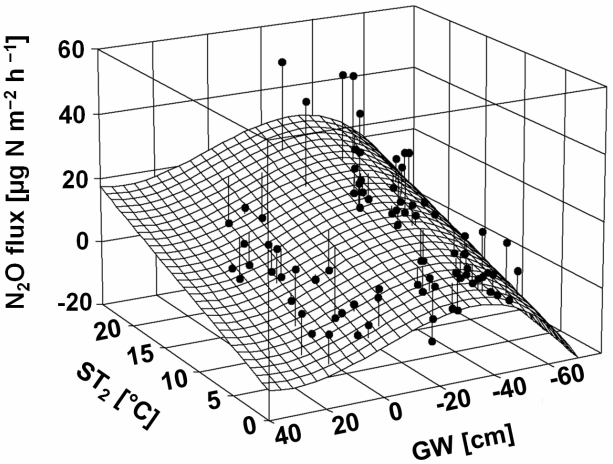

Fig. 6 Relationship of mean N_2O flux (y) to mean soil temperature in 2 cm soil depth ($x1$) and to mean groundwater level ($x2$). Equation formula is y = −4.884 (± 2.409) + 1.128 (± 0.179) · ST_2 + (−0.322) (± 0.086) · GW + 0.0004 (± 0.000) · GW^3 (n = 80; adj-R^2 = 0.37; P < 0.001).

3) Referee#1: In the discussion section you deduce from a study in the black forest on Norway spruce that a mean GW level between those measured at U and D1 would enhance N_2O emissions. Can you really jump to this conclusion? The sites, climate conditions etc don't appear to be very similar (Section 4.2 p 19089 line 21). In addition please include site information for the Jungkunst 2004 publication in line 21 (i.e Bavaria, Norway spruce...).

Authors: We agree that both locations were not similar in respect to their climatic conditions, soil diagnostic properties, etc. but the study of Jungkunst et al. (2004) confirmed the theory of maximum N_2O emissions at a certain GW level, whereas higher or lower GW reduced the emissions. However, our conclusion was mainly drawn due to the observed cubic response of the N_2O fluxes to groundwater level. To concretize our conclusion, we changed the paragraph (Section 4.2 p 19089 line 19) to: "According to Couwenberg et al. (2011) mean annual GW levels below −15 cm show a wide range of N_2O emissions in fens, but Jungkunst et al. (2004) found highest annual N_2O emissions in a Norway spruce stand (black forest, southwest Germany) at a GW level of −20 cm, whereas higher or lower GW levels decreased the emissions. However, in the present study, observed differences in the GW levels were only small between the drained sites, resulting in comparable annual N_2O emissions. However, the observed cubic response to the GW level in our N_2O model reveals maximum N_2O emissions at a certain GW level. Perhaps, it could be assumed that a mean GW level between those measured at the observed sites D–1 and U would result in enhanced N_2O emissions at the present black alder forest."

4) Referee#1: In the discussion section (p 19090 line 19 onwards) you use the lack of methane emissions in the laboratory study as an indicator for favourable denitrification conditions. I would draw the opposite conclusion. The fact that you still have methane oxidation at 100% WFPS suggests to me that the system is not anaerobic enough for maximum rates of N_2 production.

Authors: We partly agree. The lack of methane emissions only shows that the redox conditions are not too anaerobic for denitrification, but no conclusion could be drawn if the incubation conditions were favorable for denitrification or not.

The observed slightly uptake of CH_4 at anoxic conditions for both sites in the laboratory experiment can probably be explained by anaerobic CH_4 oxidation which is thermodynamically possible with alternative electron acceptors (Segers, 1998). For example, some microorganisms coupled the anaerobic oxidation of methane to denitrification (CH_4 were used as carbon source) (Raghoebarsing et al., 2006; Zhu et al., 2010). Furthermore, investigations from Bollag & Czlonkowski (1973) showed that CH_4 production started only after NO_3^- was entirely consumed.

However, we delete the sentence at P19084, line 15-17 and change the following sentence accordingly:

P19090, line 17 "We can rule out that redox conditions are too anaerobic or microbial activity were less favorable for denitrification in samples from the undrained site in the incubation experiment because no CH_4 emissions were detected from water saturated soil cores and aerobic and anaerobic CO_2 production were comparable and showed a distinct temperature response in both sites."

5) Referee#1: Section 2.2.2 p 19077 line 28: explain why the first 20 ml of the extract were discarded.

Authors: The mineral nitrogen contents were analysed according to VDLUFA (1997). Therein it is part of the method to discard the first part of the extract. We change the sentence (p 19077 line 26) to "The extracts were filtered through a 4–7 µm filter paper (Whatman 595 1/2) and the first 20mL of the extract were discarded (VDLUFA, 1997).

6) Referee#1: Section 2.3 p 19079 line 4: change to '...for site U were collected...' (not 'where')

Authors: Thank you. We replaced "where" by "were".

7) Referee#1: Section 2.3 p 19080 line 9: Why did you adjust the WFPS of site U to 83% and for sites D1 and D2 to 70%. Does this large difference in WFPS not influence the results?

Authors: At site U, the WFPS in the field is about 100% for most of the year. To avoid shrinking losses in the cores by drying to 70% WFPS, three cubic blocks (dimension 40 x 25 cm, height 20 cm) were collected. From this cubic blocks, six intact soil cores where collected

after the blocks reached the calculated 70% WFPS. However, the calculation of the WFPS based on the bulk density which was determined at 100% WFPS. We suspect, that due to the shrinking of the soil sample blocks the bulk density must have change, therefore the calculation of the required water content was not as accurate as needed to adjust a WFPS of 70%. However, the actual WFPS of the samples could be determined only after the measurements were carried out.
We suspect that the incorrect setting only marginally affect the overall results from the laboratory experiment since no differences could be found in the responsiveness of CO_2 exchange at the lower soil moisture content between U and D2. Additionally samples from site U and D2 show at both WFPS levels the same pattern in the reaction of N_2O fluxes. The reduction from 100% WFPS too 83% obviously affected the N_2 flux rate much stronger than the difference between 83% and 70%. However, the only marginally affect could be found for CH_4 exchange, but this could be accepted as this was not our main topic. Nevertheless, we cannot totally exclude the possibility that the incorrect adjustment lowered the gaseous N losses from samples of site U at 83% WFPS.
To take this into consideration, following changes have been made:
Section 2.3 were divided in three sub-sections and partly rewritten. We include following sentence in section 2.3.1 “We suspect, that due to the shrinking of the soil sample blocks the bulk density must have changed, therefore the calculation of the required water content was not as accurate as needed to adjust a WFPS of 70%.”
We change in section 3.4 the relevant parts to 83% WFPS instead of 70% WFPS.
At P19089, line 27 we include “The overall results from the laboratory experiment lead to the conclusion that the incorrect setting of the WFPS too 83% only marginally affect the responsiveness of samples from site U compared to 100% WFPS treatments as well too samples from site D–2. Nevertheless, we cannot totally exclude the possibility that the incorrect adjustment lowered the gaseous N losses from these samples.”
We change the labelling in Figure 7 and the corresponding Figure caption.

8) Referee#1: Section 4.1 p 19086 line 6: it would be very helpful to the reader if you would put average NNM rates from your data into this first sentence. : '...observed NNM rates (x – y kg/ha/y) are at the high end...'
Authors: We changed the sentence as proposed in “For both drained sites, the observed NNM rates (518–653 kg N ha^{-1} yr^{-1}) are at the high end of NNM rates given in the literature.”

9) Referee#1: Section 4.2 p 19090 line 14: Change 'Obviously' and 'temperate' to: 'Our data have shown that denitrification was limited by temperature ...@...
Authors: We changed the sentence as proposed in “Our data have shown that denitrification was limited by temperature at the drained site,….”

Anonymous referee #2:

1) Referee#2: Chapter 2.3 Laboratory incubation experiment P19079 line 26. Is it correct "He/O2 6.0"?
Authors: This was a mistake. We used He 6.0 as carrier gas with a flow rate of 1 mL min^{-1}.
We change the sentences to “Gas chromatograph settings were: TCD temperature 60 ℃, sample inlet 60 ℃, molsieve capillary column (14 m), oven temperature 60 ℃, carrier gas, He 6.0 (1 ml min^{-1}).”

2) Referee#2: P19080 line 8. Please explain, why was adjusted 83% WFPS for site U instead of 70%?
Authors: See comment 7) Referee#1.

3) Referee#2: Chapter 3.3 Field N_2O fluxes P19083 line 16. There is referred to figure 5c. I assume that authors meant figure 5.
Authors: Thank you. We changed figure 5c to figure 5.

4) Referee#2: P19083 line 20. There is referred to figure 1d, 2d. I assume that authors meant figure 1.
Authors: Thank you. We changed this to "(Fig. 1; Fig. 5)".

5) Referee#2: P19083 line 20. There is said, that undrained site the highest emission of N_2O occurred during the dry period in May. In figure 5 is shown that the highest emission of N_2O in site U occurred on 11th of June. And in figure 1 is shown, that water level started to rise at the same time. I suggest to change ": : :during the dry period in May" to "..at the end of dry period at the beginning of June".
Authors: Referee#2 misunderstood the X-Axis scaling of figure 1 and 5. Scaling starts at 1st of December 2010 (Dec-10). Thereafter always the 1st of each second month is shown for the year 2011 (-11).
We ad a corresponding remark too the figure caption from figure 1,2 and 5.

Anonymous referee #3:

Specific comments:

1) *Referee#3:Title suggestion: Nitrogen mineralization and gaseous nitrogen losses from waterlogged and drained soils in black alder forests.*
Authors: See comment 1) Referee#1.

2) *Referee#3: Authors may add information (ref) about previous findings on the relationship between temperature and soil denitrification rate and its product stoichiometry.*
Authors: We include following sentences:
P 19074, line 5 "Furthermore, soil temperature is known to be a key variable affecting both processes (Firestone and Davidson, 1989; Smith et al., 1998)."
P 19089, line 29" This was in line with investigations from Scholefield et al. (1997) who found a greater than 50-fold increase in denitrification activity with increasing WFPS from 70% to 90%. Beside soil moisture, the dependency of denitrification activity on temperature, as found for field N_2O fluxes, becomes even more apparent regarding the results from the incubation experiment, particularly for samples from site D–2. Additionally our results reveal that the production of N_2 increased more with increasing temperature than the production of N_2O, which was also found by Maag & Vinther (1996). Higher denitrification activity with increasing temperature were also reported by several other studies (e.g. Dobbie and Smith, 2001, Schindelbacher et al., 2004; Schaufler et al., 2010). Butterbach-Bahl et al. (2013) reported that the Q10 of denitrification exceeds the Q10 of soil respiration and attributed this to a tight coupling between the microbial C an N cycles. Additionally, denitrification is indirectly effected by the temperature induced respiratory depletion of soil oxygen concentration (Butterbach-Bahl et al. 2013)."
However, as written at P19091, line 1-4 For N_2 fluxes no comparable values from the field are available in the literature but observed N_2 fluxes in the incubation experiment are in the range of other studies from drained and undrained fen ecosystems (e.g. Teiter and Mander 2005; Wray and Bayley, 2007; Mander et al., 2008; Roobroeck et al., 2010; Soosaar et al., 2011).

3) *Referee#3: Second objective needs to be revised as authors did not test N_2O emissions and the factors regulating the N_2O emissions along a soil moisture gradient.*
Authors: We agree. We change the sentences to "and (ii) N_2O emissions and the factors regulating the N_2O emissions and N_2O/N_2 ratio from waterlogged and drained black alder forest on organic soil."

4) *Referee#3: 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).*

Authors: We move up the paragraph at P19075, line 19–22 to line 10 due to the more general character of this information. Further we change the sentences in line 12-14. To avoid redundancy and to simplify the manuscript structure, physico-chemical properties of the sites were only presented in Table 1 and Table 2.

5) *Referee#3: I should remind that N_2O measurement intensity (every second week) was very low. As authors also indicated, they most likely missed number of important N_2O peaks, as extreme N_2O peaks normally lasts very short (a day or two). Therefore, I recommend to avoid strong conclusions about the low annual N_2O emissions.*
Authors: We agree that the N_2O measurement intensity was low in the present study. However, several actual studies (e.g. Guckland et al., 2010, Eickenscheidt et al., 2012, Beetz et al., 2013, Leiber-Sauheitl et al., 2014) use the same measurement frequency for N_2O measurements.
N_2O peak emissions are known to occur mainly during frost-thaw or distinct dry-rewetting periods. Those periods could not been found during the measurement period. Nevertheless, we can not rule out that we have missed some N_2O peaks.

6) *Referee#3: For me 120 min chamber closure time (for tall chambers) sounds unreasonable. Can you please discuss about the linearity of the N_2O and CO_2 measurement (or show an example).*
Authors: 120 min chamber enclosure time was only conducted in case of 927 L chamber volume at site U. At this site, expected N_2O fluxes are low and thus a longer enclosure time is necessary to receive a significant concentration increase. For example at the measurement dates with the highest observed N_2O fluxes at site U (2011.05.18 & 31), the mean R^2 was 0.96 and the mean slope was 0.597 for 120 min (71.64 ppb difference from t_0 to t_{120}) (see also Fig. 1 as example for the increasing N_2O concentration and linearity).
We further think that scattering of N_2O concentrations due to random errors during sampling and measurement (GC accuracy is at least ±13 ppb for N_2O detection) were much larger than the effect of the chamber on the gas exchange and possible biases due to linear regression. Carbon dioxide fluxes were not used in the present study.

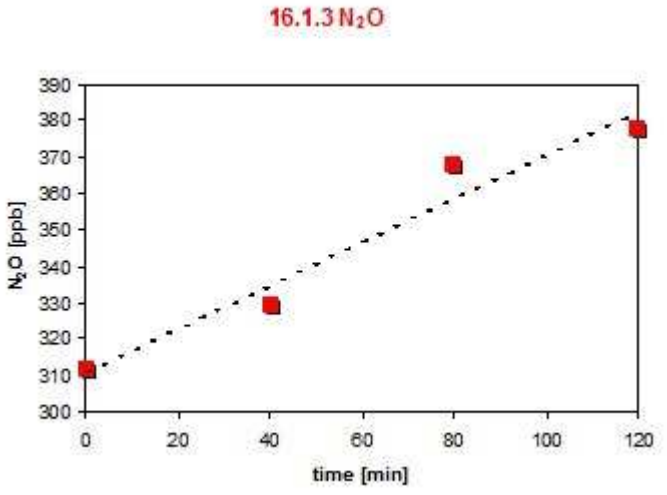

Fig. 1 Increasing N_2O concentration during 120 min chamber enclosure time at site U. Slope = 0.597 ± 0.10; R^2 =0.95.

7) *Referee#3: 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.*
Authors: We agree. We divide this section into three sub-sections: "2.3.1 Soil core sampling and WFPS adjustment", "2.3.2 Determination of gas fluxes" and 2.3.3 "Estimation of available N_{min} and NO_3^- consumption". Furthermore the first section was partly rewritten too become more specific in the sampling description.

8) *Referee#3: Can you please give information about the background N_2 concentration and its variation?*
Authors: Background N_2 concentration vary between 3.5 and 4.5 ppm (ca. 3 ppm originate from the artificial He/O_2 gas mixture and 1 ppm from diffusion into the incubation measuring device). However, the variation in N_2 background concentration is negligible as the actual gas concentration was detected at each single measurement.
The lowest detectable flux rates were 200 µg CO_2-C m^{-2} h^{-1}, 0.2 µg CH_4-C m^{-2} h^{-1}, 0.5 µg N_2O-N m^{-2} h^{-1}, and 0.04 mg N_2-N m^{-2} h^{-1}.
We include the upper paragraphs in the new section 2.3.2 of the manuscript.

9) *Referee#3: 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:*
9.1) *I assume that soil NO_3-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 NH_4 and NO_3 levels prior to the experiment).*
Authors: Refree#3 probably misunderstood the intension of the incubation experiment. The main focus was to assess the potential denitrification rates and especially the N_2-losses under conditions, which should reproduce field conditions as close as possible. Thus different NO_3^- contents were taken in purchase, as it reflects the natural site conditions. It was not our commitment to adjust equal N_{min} contents due to its low meaningfulness for the undrained and drained sites.
However, to concretize the intension of the incubation experiment we include following paragraph P 19078, line 17 "To examine the magnitude of potential total denitrification losses from the undrained and drained alder forest, a laboratory incubation experiment was performed. Therein, the experimental conditions should reproduce field conditions as close as possible in respect to their temperature range and mean WFPS."

9.2) *Referee#3: Secondly, soil NO_3^- 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 NO_3^- content will differ drastically at each stage where temperature effect was tested (please note that soil NO_3^- 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 N_2 or N_2O emission from U soil.*
Authors: We agree that the NO_3^- content deplete in course of the incubation time. Several studies (e.g. Schindelbacher et al., 2004; Schaufler et al., 2010;) use a comparable laboratory design to determine the effect of temperature and/or soil moisture on greenhouse gas emissions. However, it was not our intension to derive a functional response between denitrification activity and NO_3^- content.
Storage temperature of soils cores were 4℃, which could be assume to reduce microbial activity and therefore N transformation processes and NO_3^- consumption to a low level. In course of the incubation period, the temperature in the first 44 hours was set to 0℃ or rather 5℃, also low microbial activity and related NO_3^- consumption can be expect. The low microbial activity is also reflected in the low observed CO_2 flux rates at 0 and 5℃ temperature level at each WFPS level and both site types.
In the present study, significant amounts of gaseous N losses are only expected in case of complete denitrification with N_2 as end product (observed N_2O-N losses are quantitatively not important). Thus only samples with 100% WFPS were potentially affected from NO_3^- limitation at higher incubation temperatures. However, as the rough estimation in Table 1 shows, it can be assumed that sufficient NO_3^- is available in the soil cores from site D–2.
At site U, NO_3^- could mostly not detected in the field. Therefore it could be assumed that NO_3^- is limiting at all temperature levels at 100% WFPS as would additionally be observed in the field. However, as the calculation showed, the presumably produced gaseous N-losses distinctively exceed the potentially available NO_3^- from soil cores at 100% WFPS. This

probably indicate an immediately denitrification of nitrified N (coupled nitrification-denitrification), as you assumed (see comment 13 Page 19090 L4). Moreover it shows that nitrification if it happens at all under (slightly) anaerobic conditions is on a very low level and resulted not in large N_2 emissions.
For soil core samples with 70% WFPS it has to keep in mind that not only N consumption but also nitrification take place. To discuss about the amount of extra nitrified N is hypothetic and not meaningful but the field studies demonstrate that very high net nitrification rates occurred in the present alder stand.
However, for clarification we include Table 1 into the manuscript and include a subsection 2.3.3 "Estimation of available N_{min} and NO_3^- consumption".

Table 1 Estimation of available N_{min} and NO_3^- consumption during sample storage and incubation time.

	time [h]	site U	site D2
mean bulk density from soil samples 9–16 cm soil depth [g cm^{-3}]		0.13	0.56
Estimated NO_3^- content [mg N kg^{-1}]*		5.90	68.33
Estimated NH_4^+ content [mg N kg^{-1}]		23.30	0.00
Soil mass 250 cm^3 soil core [kg]		0.03	0.14
Estimated amount of NO_3^- per soil core [mg N]		0.19	9.57
Estimated amount of NH_4^+ per soil core [mg N]		0.76	0.00
Surface soil core [m^2]		0.00407	0.00407
Estimated mean N_2 flux during storage at 4°C [mg N m^{-2} h^{-1}]		1.40	0.82
mean N_2 flux at 0°C [mg N m^{-2} h^{-1}]		1.55	1.03
mean N_2 flux at 5°C [mg N m^{-2} h^{-1}]		1.24	0.60
mean N_2 flux at 15°C [mg N m^{-2} h^{-1}]		1.36	1.37
mean N_2 flux 25°C [mg N m^{-2} h^{-1}]		1.27	6.02
Estimated mean N_2 exchange from soil core at 4°C [mg N]	264/432#	2.4527	0.8757
mean N_2 exchange from soil core at 0°C [mg N]	24	0.1514	0.1006
mean N_2 exchange from soil core at 5°C [mg N]	20	0.1009	0.0488
mean N_2 exchange from soil core at 15°C [mg N]	20	0.1107	0.1115
mean N_2 exchange from soil core at 25°C [mg N]	20	0.1034	0.4900
sum N_2 exchange during sample preparation and incubation [mg N]		2.92	1.63
Proportion from estimated available NO_3^- [%]		>100	17

* Values present start concentrations from net N mineralisation experiment, which starts at the 7th of June 2011. Soil core samples for incubation were taken at the 10th of June 2011.
# Samples from site D2 (100% WFPS) were stored for 264 hours before the incubation experiment starts. Samples from site U (100% WFPS) were stored 432 hours before the incubation starts.

10) *Referee#3:* Please report soil NO_3^- and NH_4^+ content before and after the incubation period.
Authors: A rough estimation of potentially NO_3^- and NH_4^+ contents could be found in the calculation (Table 1) above. Further, section 2.3 of the manuscript was extended by a sub-section "2.3.3 Estimation of available N_{min} and NO_3^- consumption". After the incubation procedure we decide to prove the accuracy of the adjusted WFPS rather than the NO_3^- contents from the soil cores. As the determination of WFPS is a destructive method (drying for 24 hours at 105℃) NO_3^- and NH_4^+ contents could not be test at the same samples.

11) *Referee#3: I assume that soil from D-2 had much higher NO_3^- content during the course of incubation (therefore higher N_2O emission was not surprising). If possible please show time course data.*
Authors: We agree that the samples from site D-2 most likely had much higher NO_3^- contents which was intended and one of our hypotheses! Nevertheless it has to keep in mind that the measurement of N_2 is not trivial due to the very high concentration of N_2 in the ambient air and the resulting strong diffusion gradient. As a consequence, in order to get accurate N_2 flux data care must be taken to guarantee absolutely gas-tight incubation vessels. Therefore a continuously record of additional parameter (e.g. NO_3^-, NH_4^+) is not possible during the measurement procedure in the used incubation device at the ZALF lab.

General comments:

12) *Referee#3:* 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.*
Authors: We agree. However, as written, differences in NNM rates between 0-10 cm and 10-20 cm were only small likely as temperature differences also. However, aeration is an important key factor as you mentioned. We change the sentence to "The observed minor reduction in N turnover with increasing soil depth furthermore reflects the influence of aeration and temperature on mineralization processes".

13) *Referee#3:* Page 19090 L4: *I do not agree with the conclusion that losses of N_2O are only of minor importance compared to N_2 losses under water satured conditions as latter depending strongly on NO_3^- concentration in soil solution. Even under complete anoxic conditions, N_2O/N_2 ratio may be reasonably high.*
Authors: We partly agree. Following sentence were changed; P19090, line 2 "…indicating that in the present alder stand losses of N_2O are only of minor importance compared to N_2 losses at water saturated conditions."
Additional we include following paragraph at P19090, line 6 "Indeed several studies reported that high contents of available NO_3^- result in the inhibition of N_2O reductase activity (Blackmer & Bremner, 1978; Firestone et al., 1979; Weier et al., 1993; Regina et al., 1996). However, for the present study it is not possible to give a conclusion to what extend the probably higher NO_3^- availability inhibits the conversion of N_2O to N_2 at samples from site D–2."

14) *Referee#3:* Page 19090 L28: *I also assume that N_2O losses may be negligible at the undrained site during wet seasons (due to low NO3 concentrations). However even in wet seasons, large N_2 emissions may occur due to complete denitrification (as NO_3^- 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).*
Authors: We do not agree that large N_2 emissions occurred in time periods were waterlogged conditions exist at site U. As written to comment 9.2, the incubation experiment gives a hint that nitrifier denitrification or a fast denitrification of produced NO_3^- occurred under waterlogged conditions. But obviously the nitrification is strongly hampered, resulting in very low nitrification rates and thus no substrate (NO_3^-) for denitrification were available for large N_2 emissions. However, further investigations are necessary to prove both statements.