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Interactive comment on "Stand age and tree species affect N_2O and CH_4 exchange from afforested soils" *by* J. R. Christiansen and P. Gundersen

Anonymous Referee #1

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General comments The paper by Christiansen and Gundersen presents interesting results on the effect of tree species and stand age on the exchange of nitrous oxide and methane between forest soil and the atmosphere. The study is well planned and in most parts the interpretation of results is sound. However, I have concerns about some of the conducted statistical analyses that have to be addressed and I would like to suggest refocusing parts of the paper (see below).

One of the objectives of the study was to "identify the environmental factors responsible for the differences in GHG exchange", which is approached by regression analyses. However, from my point of view several of the regression analyses are not appropriate.

Firstly, the regression between the annual N2O emissions and the %NO3 (percentage of NO3- of total mineral N) is inappropriate as the %NO3 was only measured once at the end of the two year flux measurement period. Given that the concentrations of mineral N as well as the ratio between NO3- and NH4+ can vary during the course of a year as well as between years, the representativeness of the measured %NO3 can be questioned. Secondly, to support the relation between N2O fluxes and %NO3 the authors further conducted regressions between the annual N2O fluxes and NO3- leaching as well as sub-root NO3- concentrations measured over a 3-yr period by Hansen et al (2007). However, these data came from the period 2000-2002, while N2O measurements were conducted 2008-2010, questioning the appropriateness of the regression analysis. It should be noted that the authors acknowledge the weakness of these regressions. However, from my point of view all the mentioned regressions should be removed from the paper.

Regarding the result section I would like to suggest an alternative structure of the subsections as follows: 3.1 Abiotic soil properties: This could include the text presented p. 5739, line 22 – p. 5740, line 7 and p. 5741, line 8 – p. 5742, line 14, with potentially shortening of some parts. 3.2 N2O exchange: containing p. 5738, line 24 – p. 5739, line 10 and p. 5740, line 9 – 18. 3.3 CH4 exchange: containing p. 5739, line 11 – 21 and p. 5740, line 19 – p. 5741, line 3. 3.4 Relation between GHG exchange and soil properties: containing p. 5742, line 15 – p. 5743, line 3 (but excluding the above mentioned regressions!) as well as p. 5740, line 15-16 and p. 5741, line 4-7.

Please check the paper carefully for phrasings that indicate that the N2O emissions or CH4 uptake are by the trees (e.g. CH4 oxidation in oak), as the exchange is between soil and atmosphere.

The GHG fluxes were measured in each stand in three plots, that each consists of three chambers. It is not clear if for statistics an n=3 (for plots) or an n=9 (for chambers) was used. It is, moreover, somewhat confusing that results were partly presented as stand averages, but partly as chamber fluxes, which also resulted in some repetitions. I suggest focusing on the stand average fluxes and discuss individual chamber fluxes only in respect to spatial variability.

Specific comments p. 5730, l. 17: could you specify which physico-chemical properties? p. 5731, l. 4-6: suggest moving this sentence to after the next paragraph p. 5732, l. 1ff: I think the cited study from Höglwald (Papen and Butterbach-Bahl, 1999) is specific as the spruce forest exhibit about four-time higher NO than N2O fluxes (see Gasche and Papen, 1999). Consequently, the total NO+N2O emission is higher from the spruce compared to the beech site. This may deserve mentioning here. p. 5732, l. 10-12: needs rephrasing p. 5732, l. 12: write "deciduous" instead of "hardwood" p. 5732, l. 23: replace "so" with "in order" p. 5732, l. 25: it is not clear if you mean that N2O emissions increase or decrease. Please rephrase. p. 5736, l. 10: Unclear what is meant by "due to limitation of concentration data points during enclosure". p. 5736, l. 20: How many soil cores were taken? p. 5737, l. 18: How did you deal with negative fluxes when log-transforming the data? p. 5739, l. 5-7: repetition from p. 5738, l. 27ff. p. 5739, l. 11: should read "μg CH4-C"? p. 5739, l. 12: ". . . only above zero during winter and early spring" seems to contrast with line 16-17 "Emission of CH4 was observed . . . predominantly in spring and autumn" p. 5739, l. 18: as maximum emission a value of 66 μg CH4-C m-2 h-1 is given, but line 11 reads that "CH4 fluxes ranged between -30 and 9". And why was not maximum CH4 uptake discussed, as more often uptake then emission was observed? p. 5739, l. 24-25: Suggest: "Soil water content ranged from 44 to 6 vol%". I don't think that much information is provided by mentioning in which plot the extremes were measured, as it seems that it is just by chance that highest moisture was in O-70 and not O-93, as well as lowest moisture in S-69 and not S-97. p. 5739, l. 26-29: Suggest deleting whole sentence from "Only . . ." p. 5740, l. 11: Suggest "average values" for consistency. p. 5741, l. 2: Delete "and" after significantly p. 5741, l. 17: The statement "even though soil temperature increased with stand age in oak and decreased in Norway Spruce" appears to be incorrect, as Fig. 3b indicate that both these changes were non significant! p. 5741, l. 19: Delete "marginal" p. 5744, l. 7: "nitrous oxide reductase" p. 5743, l. 9: Suggest to only pro-

vide mean and SE. p. 5744, l. 11ff: One additional aspect of the lower diffusivity in the younger stands is that the retention time of N2O in the soil is prolonged, which in turn increases the likelihood for a reduction to N2 and hence can reduce the N2O emissions. p. 5745, l. 5ff: The difference in C/N ratio between the stands at Vestskoven is rather small (10.6 – 14.6) compared to the range for European forest (13.4 – 37.7; Pilegaard et al. 2006) as well as organic forest soils (13 – 90; Klemedtsson et al. 2005). Moreover, the C/N ratio at Vestskoven is for all four stands in the lower range of the European compilations, indicating the potential for significant N2O emissions. As the C/N ratio is only a general proxy for N2O emissions, one can question its applicability for comparing the plot N2O fluxes. p. 5745, l. 8: Suggest "higher net nitrification" p. 5745, l. 13: It is not clear if you mean net or gross nitrification rates. Moreover, I do not see the functional relationship between C/N ratio and nitrification (particularly not for autotrophic nitrification). Why should nitrification be higher at lower C/N ratios? I also think that gross nitrification may actually be lower in the younger stands, as the competition for NH4+ may be higher. p. 5745, l. 17ff: I think that the paragraph on changing plant N demand after afforestation should be more highlighted, as from my point of view this may be at least as important as the changes in physico-chemical soil properties. p. 5748, l. 18-19: Suggest: "CH4 uptake increased in oak but remained constant in Norway Spruce stands" (see statistics). p. 5748, l. 24: Suggest adding that low N availability is due to high plant N demand. p. 5749, l. 5: You have not talked earlier about the N cycling, hence it should not be included in the conclusions. However, it may be helpful to discuss earlier your results in the light of differences in N transformations between coniferous and broad-leaf forest soils, which was the subject to several studies (e.g. Brüggemann et al., 2005; Christenson et al., 2009; Staelens et al., 2011; Ste-Marie and Houle, 2006; Zeller et al., 2007).

Fig. 5: Irregardless of my general doubts to perform this regression (see above) it appears that the correlation between N2O flux and soil water NO3- is almost exclusively due to one of the twelve plots. Therefore, such a regression has to be interpreted with care. Table S-1: Plots are here labelled A – C, while in Table 1 by numbers (1 – 3).

Please be consistent.

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Interactive comment on Biogeosciences Discuss., 8, 5729, 2011.