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Interactive comment on "Sources of nitrous oxide emitted from European forest soils" *by* P. Ambus et al.

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I'd like to congratulate the authors to this very interesting manuscript and the attempts they made to compare N2O production activities among different forest sites. The methods involved are scientifically sound and the results are clearly presented. I find the manuscript acceptable for publication in Biogeosciences.

However, one can still find arguments, suggestions, how the scientific outcome can be improved:

1. I suspect, that removing of the litter layer changes the N transformation processes in the corresponding soils quite dramatically (see comment from Referee 1). A discussion on it, would be very helpful in this regard. 2. How representative are the results obtained from relatively small soil profiles (= cores)? Do the results obtained in the lab somehow correlate with outdoor flux measurements of N2O? I think these data should

be available for some of the investigated forest sites? Is there the same ranking of N2O production for example: Hyytiälä $<$ Höglwald $<$ Schottenwald 3. A rather technical comment: The authors are saying that all soils showed N2O production. This can hardly been seen from some of the data in Fig. 2 (eg. Hyytiälä) - I think it is a matter of scaling and needs better visualisation. 4. Subjecting soils to water-logging conditions you can get create in almost every soil N2O or even CH4 producing conditions (see also Wachinger et al., 2000 SBB, Peters & Conrad, 1995 AEM). So for me there is no wonder that you could detect N2O in all samples. The more interesting question for me would have been, how long does it take until N2O production is detectable in the various soils?

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