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Interactive comment on "N_2O, NO and CH_4 exchange, and microbial N turnover over a Mediterranean pine forest soil" *by* P. Rosenkranz et al.

P. Rosenkranz et al.

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Since we have described the principal set-up of the equipment already in earlier publications, we tried to keep this section as short as possible in the short version. But we do agree that this was obviously to short and, therefore, now have extended this section. We also measured automatically CO2 concentrations in the sample air drawn from the static chambers and used those measurements as a control to check the appropriate closing of chambers. This information is now also given in the Material and Method section.

Yes, we can certainly rule out that N2O concentration measurements are effected by interferences with CO2 and H2O in sample air. We always use pre-columns filled with Ascarite which do not only remove CO2 from sample air, but also most of the water vapor. To ensure that there will be no break through of CO2 or H2O which may

interfere with N2O measurements, we changed the pre-columns routinely on a weekly basis: This information as well as information on the length of a measuring cycle (60 min closed, 60 min opened) are now given in the Material and Methods section.

We agree with the referee that measured NO, N2O and CH4 fluxes at the soil surface have to be interpreted to be the sum of consumptive and productive processes. We know highlight this context by introducing two more sentences: "It should be emphasized that the observed N2O fluxes at the soil surface are always the result of simultaneously occurring production and consumption processes (Conrad, 1996, 2002). In accordance with compensation point concept (Conrad, 1996) our data demonstrate that uptake of N2O can dominate over N2O production in the soil."

We also do agree with the reviewer that the most logical interpretation of the absence of a stimulating effect of soil moistening on N2O fluxes should be interpreted in such a way, that moistening stimulated N2O production as well as N2O consumption process, so that the net effect on surface fluxes was zero. We now include a sentence in the discussion section to highlight this interpretation: "This can be interpreted in such a way, that increases in soil moisture stimulated N2O production but simultaneously also increased N2O consumption, so that the net-effect - as measured at the soil surface as N2O flux - was zero."

Measurement of soil moisture do show that soil moisture at the artificial rainfall sites increased significantly. But this does not really help to interpret observations. What we should have done in the field is to use an inhibitor such as 0.01% of C2H2 to reduce or inhibit nitrification activity. Since this is assumed to have no effect on denitrification activity, one should assume that the uptake of atmospheric N2O should be higher in such case (assuming denitrification is indeed the uptake mechanism and assuming that nitrification as well as denitrification contributed to N2O production). However, we missed to perform this experiment, which would have allowed for further interpretations.

Unfortunately, we have not done any soil air CO2 concentration measurements. CO2

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measurments were only done in the framework of N2O/CH4 flux measurements with static chambers. We agree with the referee, that the findings concerning N2O concentrations in soil depths give a hint, that the organic layer might be most responsible for the net N2O uptake. In the discussion we are mentioning this: "Furthermore, the measurements of N2O soil air concentration profiles at the San Rossore site showed that N2O concentrations below the C rich organic layer and in uppermost mineral soil were slightly lower than atmospheric N2O concentrations. This finding is also in-line with the hypothesis of Wrage et al. (2001), indicating that N2O uptake may be mainly associated with the uppermost C rich soil layers."

We agree with the referee, that it can not be excluded, that also periods with net N2O emissions could occur in this Mediterranean forest type due to intra and inter annual variations. However, we still find it valuable to provide an annual estimate. But we now included some sentences, to address the concerns (with which we do agree) of the reviewer: "However, one needs to be carefully here, since our measurements only cover in total only approx. a two month period. But, short periods with high N2O emissions can significantly bias annual estimates of N2O fluxes, as has been shown for temperate forests for N2O emissions during short-term freezing-thawing events (Papen and Butterbach-Bal, 1999). Therefore, annual estimates should be based on year round measurements."

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