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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.

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General comments: This manuscript presents original results on N2O, NO, NO2 and CH4 fluxes from a Mediterranean forest in Italy. The results are interesting from three points of view : (i) few such measurements have yet been published under Mediterranean conditions, (ii) more especially dealing with these different trace gas fluxes in the same time and (iii) they evidence continuous deposition of N2O and CH4. The conditions and the methods are well described, the experimental setup was sound and most relevant variables were measured, even if additional measurements could help interpreting the results. The subject is well within the scope of BioGeoSciences I consider that this work is a significant contribution to our knowledge on trace gas fluxes when one consider both the data and the discussion that arises some basic question on processes in forest soils and soils with low N input. However I would like to make three comments of this paper: - the measured N_2O fluxes are low and most often negative. Their values are not far from the detection limit with chamber methods. On figures 1 and 2, they are given with error bars, whose way of calculation is not really given. I assumed that it was the standard deviation of the measurement over the 5 chambers. Nevertheless, due to the weakness of the flux, a more precise error analysis should have been done to include in this error bar the uncertainty in measurement over each chamber by considering the uncertainty in gas analysis and other possible sources of errors. - the discussion arises several arguments to explain N_2O deposition under aerobic conditions. All are possible explanations, but none of them gives direct experimental evidence. I am not really convinced by the long discussion on inorganic N concentrations. As N_2O and NO emissions are linked to N transformation rather than to N concentration, the concentration is not a completely relevant variable, even if a correlation exists. Moreover, in a sandy soil, nitrate can be lost rapidly by leaching. For the relation between fluxes and soil water content, I agree that “no positive effect Ě could be demonstrated”. But it is neither possible to conclude that their was no effect. I suggest that the authors put this discussion into perspective and discuss the additional measurements that should be made to draw firm conclusions to explain these fluxes. - the results on N_2O and NO fluxes, with soil N determination make a consistent set of data. In contrast, the results on CH_4 are largely decoupled of that on N species and are not discussed at the same level. I consider that the results on CH_4 are not really necessary and could be only mentioned in the discussion.

Specific comments: - the soil description is short: the authors should give more information on the soil type and texture, the depth of the organic layer and the organic content of the different layers. - Table 2: the concentration differences do not exceed 1.2%; nothing in the Material and Methods section gives the uncertainty of the method, including sampling and analytical determination - Considering the possible effect of inorganic nitrogen on methane oxidation, as far as I know it is linked mainly to NH_4^+ and not NO_3^-. - Conclusions: I was surprised that the conclusion is only on N_2O, while original results were also obtained on NO

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