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Biogeosciences Discussions

Interactive comment on "Fluxes of CO_2, CH_4 and N_2O from soil of burned grassland savannah of central Africa" *by* S. Castaldi et al.

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As required by all the 3 referees soil water content has been expressed as WFPS in all the graphs relative to field data and as a percentage of maximum water holding capacity (WHCmax) for laboratory experiments. Corrections have also been made in the text.

We have always used for field flux measurements 3 gas sampling from each chamber and from my experience on gas sampling from many European groups using the manual approach I haven't seen a group which does by routine more than three measurements. Of course it is different when an automated system is used as in that case many more measurements can be planned. Indeed three is the minimum set that allows to check if the increase of concentration inside the chambers over time follows

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a linear trend, which is what should occur if the measurements are correct. Indeed 3 points do not allow to speculate too much on the R2-value. To double check values when the results is not convincing we do control the rates of increase of N2O, CO2 and increase/decrease of CH4 which are measured all at the same time on the same gas samples. My experience is that when fluxes are not too low the linearity is always reached. Only in volcanic environment I got chamber saturation for both of CO2 and CH4 but fluxes were enormously high. When fluxes are extremely low as in the case of N2O in dry environments, it is easily found that no linear increase is observed and the measured concentration falls within one standard error of the measured atmospheric concentration of the standard gas, with an oscillation which deviate from linearity. In this case then we assume that for the time of incubation we used the flux is too low to be detected with our routine. Interestingly we measure the fluxes of CH4, CO2 and N2O at the same time and many time it occurs that while N2O fluxes must be considered below the capacity of measurement of the procedure CO2 and CH4 fluxes are not and this is always coherent with the type of environment in which we work, indicating indeed that it is really a low N2O flux and not a failure of our ability to store gas samples. Samples were analysed within a month (more or less) from sampling and vials have been tested for 2 months storage and we are confident that they are gas-tight if the vials are further sealed with thermo glue on the top of the butyl rubber lid. Without the thermo glue there are already some losses within few weeks. This is in fact one of the main doubts I have in using automated gas sampler which stores in the field gas in vials such as those we used in this work, leaving them for many days in the field before collection. In our case we glue the vials within two hours from the sampling event. Calibration gases are injected in general every 20 samples.

For laboratory incubations our aim was to have a comparative measurements (among treatments) rather than an absolute value of flux to be compared with field data. We in fact used 24 hours of incubation which in the field would be never used. However we were comparing samples which were sieved, dried and shipped and we were worried that normal incubation time (1-3 hours) would have resulted in too low concentrations

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in the headspace of the incubation vessels. In particular for N2O (again all gases were measured on one single gas sample). Indeed looking at the lab results fluxes were so low that a shorter vessel closure would have not allowed to detect any flux. My opinion is that all the procedure makes these results very different from what could be obtained in the field and so I would prefer to leave the emitted gas per unit of dry weight of soil. The conversion can be easily make by the reader knowing the bulk density and the depth of sampling if there is the need to make some further speculation.

In the text we have added some more indications of the gas measurement technique.

Looking at Fig. 5 the referee can check that in figure 5A where a relationship between CH4 fluxes and WFPS can be observed, CH4 fluxes do not decrease at higher water content. Indeed they increase. The mound density underground was not measured, because we did repeated sampling in the same spots both in this study and in the work which is in preparation on CH4 from termites nests and soil, so we could not make destructive sampling. Around the mounds we have found variable results (emissions and uptake) depending on the mound, which spot around the mound (which direction) and the time of sampling during the year. Given the extremely dry conditions of the soil, the sandy structure and the low organic C content it would be very strange that a microbial process of CH4 production would occur. Very strange. Indeed N2O emission which is also favoured by the formation of hotspots of microbial activity was extremely low, probably mainly produced by nitrifier microorganisms in these conditions. One more paper is in preparation where similar observations have been made also in other savanna sites.

P 4090 line 8 (old version) High frequency burning in this case means every year, and sometime also twice per year (during the short dry season). This has been explicitly indicated in the text.

All the suggestions and been included in the text.

p. 4092 line 20-22. The concept is fully explained by Ansley et al. 1992 . The basic

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concept means that fire tends to maintain the dominance of grasses over shrubs and trees, in particular these fast fires which characterize frequent savanna burning, which are generally of low intensity and burn the above ground biomass without destroying the roots. Roots are characterized by shallower rooting system compared with trees so that more organic C derived from root turnover accumulates in the upper soil centimetres, compared with C distribution which could be observed in tree savannas. Hence fire management somehow influences the distribution of the soil C by controlling the plant community diversity.

p.4094 line 8-9 sentence has been deleted as also indicated by referee 1.

p. 4095 lines 21-23. The concept has been clarified. What we mean here is that we wanted to create a gradient of soil water content adding water to the soil plots at different time length from gas sampling but in particular we wanted to have the soil quite dry before adding water (rain simulation 1 day before sampling) in order to evidence any eventual pulse of gas generated by the wetting of the dry soil. The tent served to maintain the soil relatively dry and to avoid that a "last minute" rain event could cancel the created gradient of soil water content. We are aware that such a tent at 2 meters would not shade completely the soil from rain but we hoped to limit any eventual rain input. Indeed no rain occurred before the measurements so that the problem was in any case overcome.

p.4099 line 10. The volume of each gas sample taken from the chamber is 20 ml while the volume injected in the GC is 2 ml (2 ml injection loop).

p.4100 lines 26-28. As suggested by the referee the ETo were left out as indeed it was not used afterwards to clarify other concepts.

p. 4102 line 16 - What is meant is flux size classes.

p.4105 line 8-10. Concept has been better clarified.

p. 4106 line 4, we mean unburned plots, the reasoning is that we are in the middle of

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the dry season but in the unburned plots where the grass is drying we can still observe a 20% of living green biomass, despite the extreme dry conditions. In the burned plots all the above ground biomass is burned.

p. 4109 line 23 Looking at numbers from fig. 9 and Table 1 it can be calculated that the amount of NO evolved per gram of soil was much less than the amount of available mineral N in the soil. This means that the pulse is not simply controlled by the amount of substrate in the laboratory where also the water content was constant. The emissions observed is something more complicated probably supported by chemico-physical specific conditions which are created by the drying-wetting cycles. I could not say precisely what would happen in terms of NO emissions if I would rewet the soil again. Probably it would also depend on how dry the soil gets before rewetting, the temperature, for how long, UV irradiance and many other parameters. I prefer not to make speculations on this quite unclear topic and simply report the observations we made, the conditions of which are clearly specified in the method section. On the other hand, also other papers where soil is rewetted to stimulate gas pulses, do not indicate if and how much rain occurred at the site in the period before the wetting experiment. We all assume from their description of DRY season that no rain occurred, but it is just an assumption which cannot be deduced from the paper in many cases.

p. 4111 lines 1-3 I assume that the release of CO2 from burning of biomass is considered to have a null contribution to the increase of atmospheric CO2, as it derives from fixed atmospheric C-CO2. Probably a more complicated CO2 balance which needs further considerations is the one in ecosystems affected by strong fires which consume the litter layer, destroy roots and alter the organic C content in the first soil centimetres.

All minor corrections have been made in the text and in the figures.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/7/C1931/2010/bgd-7-C1931-2010-supplement.pdf

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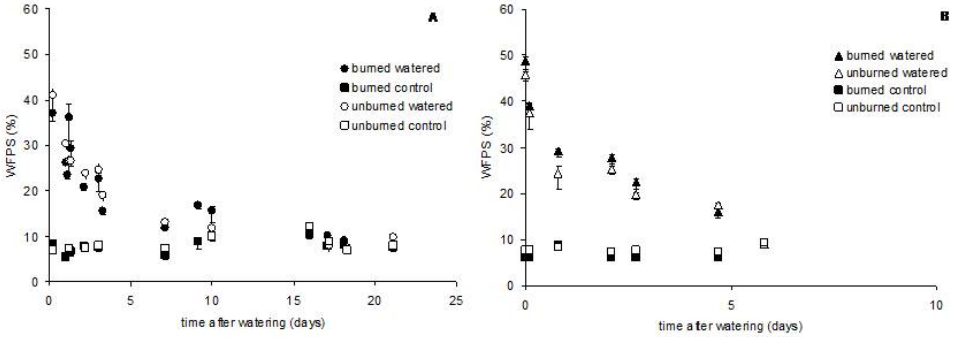

Fig. 1.

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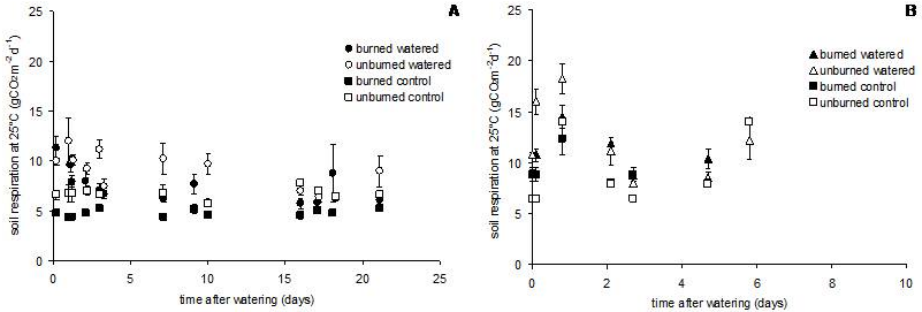

Fig. 2.

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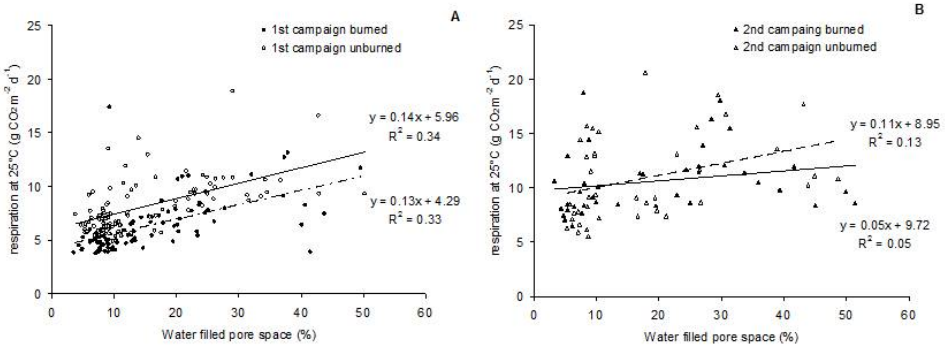

Fig. 3.

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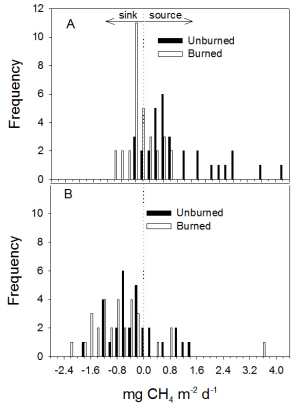

Fig. 4.

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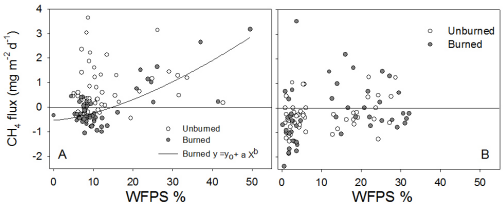

Fig. 5.

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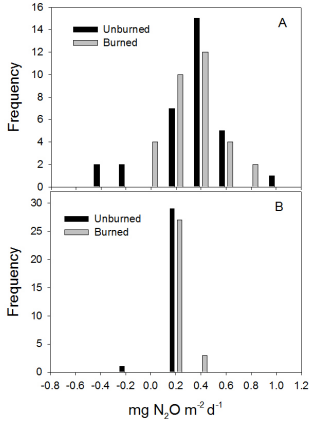

Fig. 6.

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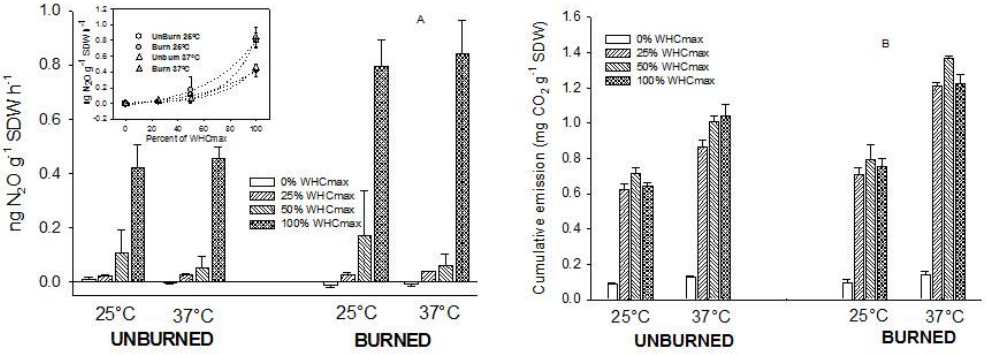

Fig. 7.

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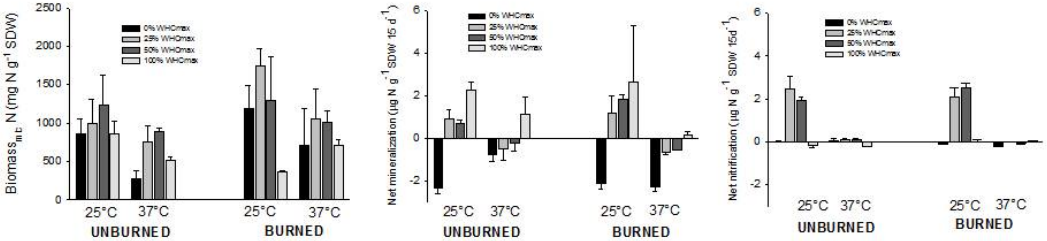

Fig. 8.

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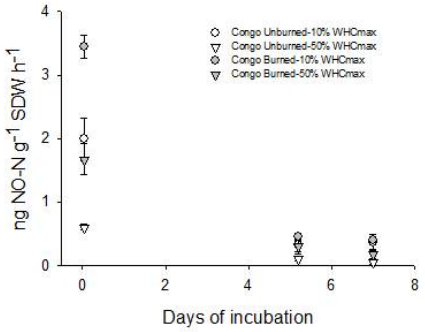

Fig. 9.

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