

Interactive comment on “Fluxes of CO₂, CH₄ and N₂O from soil of burned grassland savannah of central Africa” by S. Castaldi et al.

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The paper provides confirmatory data on the small effect that vegetation burning has on greenhouse gas emissions or removals by soil processes in grasslands and savannas in Africa. Since the number of papers on this topic is relatively small, and few are as comprehensive as this one in terms of experimental design, variables measured and techniques, this is a valuable addition.

The main reason this paper need revision is the language. It can be tidied up by having a fluent English speaker edit the text. The fixes are small, but many. Minor grammatical issues are not a serious problem, but sometimes they lead to ambiguities, which are a problem. I have only commented on the latter cases. For instance, I don't follow the intent on pg 4094 lines 8-9; pg 4095 line 2 probably mean 'grazing' rather than

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'breeding', and for 'superficial' substitute 'of low intensity'; line 11 on the same page should be '...area affected every year by fires...'. Line 14 - if the fires are 'superficial', can they also be 'massive'?

pg 4096 ln 22 Technically, this is not an Arrhenius relation - just call it a temperature function.

Scientifically, savannas are never spelled with an 'h', even if your word-processor claims otherwise. In fact, these are grasslands, not savannas, since the woody plant cover is less than 5%. They may well be referred to as savannas by local people, and they may become savannas if fire were excluded, but for now they are tropical grasslands. The construct 'grassland savanna' is a contradiction (since both are nouns) - 'grassy savanna' is OK, but this is not one of them. It is a bushy grassland, if anything.

Why are latin names not italicised throughout? Is this not possible in Biogeosciences?

Water-filled pore space (WFPS) [note, not 'spaces' - this needs correction in the text and all the figures] is used as metric of wetness - correctly, since it does allow comparison of the field soils with the repacked columns. But to calculate WFPS you need the bulk density, and that is nowhere reported. It should be in table 1. Standardise on WFPS throughout, rather than sometimes using gravimetric or other measures eg 4097 ln 17 where you refer to '% of saturation', and in figure 1 you use volumetric %, etc

pg 4093 ln 25 The claim that the Guinean Savanna (which this site is not) is 60-80% burned relies on an old reference, which does not stand up to modern analyses using remote sensing. Very few savannas have more than 40% burned area at a landscape scale, though individual parcels may burn annually.

I have reservations of using ion-specific electrodes for this purpose. Their sensitivity is very low in the range in which soil nitrate and ammonium occurs, and subject to interferences. This may be why the mineral N came out so much lower than the amino

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N.

pg 4099 line 5 the units of biomass N must be specified for this sentence to be valid.

pg 4100 line 27 it would be clearer to present both of these ETo values on a daily basis.

pg 4101 ln 6 to 8 express as g/day, rounded to one decimal place ie 7.7 g CO₂/m²/d to avoid a problem of inferring more precision than you have.

The appropriate test in figure 3 is a homogeneity of slopes test and a test for differences in intercept. Where are these reported? And are the slopes and intercepts in 3 B different from those in 3 A?

pg 4102 ln 15 - which fluxes showed higher variability?

Rather than talking of 'first campaign and second campaign' just say 'in the dry season one month after burning', and 8 months after burning, in the subsequent wet season' (pg 4105 ln 6 and elsewhere).

ln 4109 do you mean that burning trebled the emissions relative to the plot not burned in that year? (not really a control)

Figure 4 what is the top and bottom panel? In Figure 5, what is the left and right panel?

Figure 8 claims to be a rate, but the units have no time dimension.

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