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Interactive Comment

Interactive comment on "Experimental drought induces short-term changes in soil functionality and microbial community structure after fire in a Mediterranean shrubland" *by* M. B. Hinojosa et al.

Anonymous Referee #1

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Review of the manuscript bg-2014-488

General comments: the paper presented by the authors addresses the interaction between fire and drought on Mediterranean soil, which is an important issue as explained in the introduction.

The condition to test the hypothesis is hence to verify the effect of fire, the effect of drought and the interaction of the two effects answering to the question if the two together led to something more than the sum of the two alone. The experimental design however is not built to support the possibility to test the interaction, or at least to conclude that it is the interaction which causes the observed variations of soil biological, chemical and physical endpoints. We miss the unburned plots under drier conditions. I see this as a limit to drive conclusions which are at least coherent with the goal of the paper, at least as introduced by the authors in their premises.

The second important point which weakens the study is the fact that the experiment has not been followed from the beginning. All the papers on fire experiments indicate that the most important moments are just after during the first weeks after burning, at least for nutrients dynamics, then some long terms effect can be see also later on, on biological parameters such as the ration fungi/bacteria. So it is not clear why the study does not report data from the start after burning. Also data are mentioned as personal communication but not shown, which is even weirder. Also, if the control doesn't exist for all the treatments (unburned) the other alternative (not preferred) would be to have dynamics before and after burning and not only several months after burning. There are then several unclear points which are also relevant that I explain in more details in the specific comments in attachment.

Specific comments:

1. I would like to see a graph with the annual average rainfall values (the multiannual mean of total monthly precipitation of each month of the year) of historical rainfall which the authors have used to set this level of rainfall (1948-2006) plotted together with the total monthly mean of the environmental control.

2. The choice of reducing the rainfall during the summer is due to climatic trend evidence or simply to the willingness to see a harshening of summer aridity? Isn't already the summer in Spain sufficiently dry to limit microbial activity in the way we are discussing? Are the wetter months like winter, autumn, spring any important for the discussion? Does a reduction in rainfall occur also during these months in the climatic trend of climate changes in Spain?

3. It is not clear the experimental set up. From the description it seems that only the environmental control was split in two to have a comparison between burned and

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unburned. It is not clear to me then how can we compare the effect of drought and burning on soil endpoints if I do not have for the different treatments the control unburned. How can I be sure that what I see is due to the interaction and not simply to more extreme drought conditions? The authors themselves indicate that their objective is: we hypothesise that drought conditions after fire will reduce. . ...". How can I know if a reduction in microbial biomass for example in burned and drier condition compared with undisturbed is due simply to one factor or to combination of both? Do I have an unburned very dry plot to say if it already affects BM?

4. The combination of fire and drought would have been surely more interesting starting measurements after fire in 2009 as the peak of mineralization occurs just after fire and most of it might be already fading away after a whole autumn, winter, spring, when rainfall is higher. Could the author comment on this and justify their choice?

5. The dates of sampling should be specified also to understand how they fit compared with the period of induced drought during the discussion of results (right at the beginning, after xx days, at the end. . .)

6. The extracellular enzymes tend to be quite resilient to environmental conditions compared to intracellular enzymes (like ATPases or Dehydrogenases, etc) and might not be representative of the microbial dynamics during "periods" of unfavourable conditions if they are transient and not perennial (like in a desert). Could the author discuss and justify their choice of microbial endpoint indicators compared with other available in the range of possible endpoints?

7. Why the authors measured C mineralization and not N mineralization as well given that one of their objectives is to see if the availability of minerals is reduced in response to draught and fire?

8. Why the authors used a derived measure of microbial biomass (from ELFA) and not a direct official measure of BM

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9. The author state that the used one-way ANOVA to test the effects of burning and rainfall pattern at each sampling event. In theory when the influence of two interacting factors is analysed a 2 way anova should be used. The fact that one-way was used is due to the design which indeed doesn't allow testing the interaction, which gives less strength to the premise of the study.

10. The fact that the authors did not find significant differences in soil water content in summer and in winter brings us back to comment 2. I assume that summer in Spain is very arid and so it is difficult to reduce significantly the rainfall in such a period. I assume also that most of the rainfall occurs between late autumn up to early spring, centred on winter. Thus again if the sheltering systems works from May to October (at its best) it is clear that no differences could be seen in winter. So in order to understand how meaningful is the experimental set up a satisfactory answer to point 2 should be given and summarized in the paper somewhere to justify the choice made.

11. I think that physic-chemical parameters could be discussed in one single paragraph, idem for biological analysis, given that especially for the former points are few in time and not that much can be discussed.

12. Mineral N: it is not always true that ammonium N was lower in burned plots. The same is true for NO3. Moreover it should be underlined that both NH4 and NO3 were very low both in burned and unburned plots (EC). I don't see any clear reverted effect in winter for the drought treatments for NO3, they all seem to flat down.

13. The samples used for C mineralization where incubated at the field conditions of water content? I mean as they were sampled without adjustment of water content reflecting the field conditions or all at the same water content as typically done for potential measurements? It is not specified in the methods basically are you testing the potential or effective mineralization?

14. Fig. 5 Looking at figure 5 can we say that "further drought would have a negative effect on burned soil"? can you say that? Can you exclude that further drought would

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not have a negative effect on BM anyway, even if the soil is not burned? I don't think so as you miss the unburned drier condition. So which indication can we give here?

15. Fig 6 is not clear and tables A1 and A2 do not help to have a picture of which microbial groups are most affected. Something more explanatory and ecological would help.

16. It is not clear why the authors discuss the dynamics of nutrient immediately after fire, which indeed is the most interesting phase when really most of the nutrients can be leached and so a lower rainfall input would make the difference, but then they do not start the study immediately after fire and data are not shown and is cited as personal communication. I don't think this make sense.

17. The authors continue to mention their results as immediately after fire (spring summer 2010) when actually the immediately after fire, technically speaking are the first weeks after burning, in autumn 2009.

Interactive comment on Biogeosciences Discuss., 11, 15251, 2014.