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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.

M. B. Hinojosa et al.

mariabelen.hinojosa@uclm.es

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RESPONSE TO ANONYMOUS REFEREE #3

We thank the referee #3 for his/her feedback and constructive comments. The answer to his/her different comments or remarks are given bellow.

REFEREE#3 GENERAL COMMENT 1 (RGC1): The manuscript "Experimental drought induces short-term changes in soil functionality and microbial community structure after fire in a Mediterranean shrubland" (Hinojosa et al.) concerns an interesting ecological topic. However, experimental design is not persuasive. The authors hypothesize that "drought conditions after fire will reduce microbial biomass recovery and

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modify functionality and diversity of soil microorganisms either survivors or colonizers after fire" (lines 9-11, p.15254). However, the study did not start immediately after fire (September 2009), but about six months later (spring 2010), so that the most critical period was not investigated. Indeed, drought could have a more negative effect immediately after fire, when soil is deprived of litter and plant cover, than later, when probably plants begin to recolonize soil, by protecting it from direct solar radiation and so reducing the water evaporation. However, the authors did not specify if fire totally removed plants and litter neither if, and to what extent, a plant recovery occurred in burned plots during study period. This information is necessary to interpret the results of this study. Moreover, microbial community could be affected by drought immediately after fire more than later, when it probably recovered, at least in part, after direct and indirect action of fire. Also soil chemical properties may be different immediately after fire than later and this may affect the recovery of microbial community. For example, in Discussion (line 25, p. 15262, and following ones) the authors reported that the ammonium increased immediately after fire (data not shown), whereas data set included in this study showed a decrease.

ANSWER TO GENERAL COMMENT 1 (AGC1): We recognize that a greater sampling is always wanted, and a sampling immediately after fire and even another sampling in winter would be desired. However, given the limitations in space in these experiments, we had to choose when it was more appropriate to use the limited area available for a destructive soil sampling, in order to minimize external perturbations in our experimental system. We appreciate the comment and hope to provide arguments to justify our choice of sampling. Here we want to recall the response provided to the general comment 2 of reviewer#1 (AGC2). The problem with sampling right after fire is that only some of the plots (SD) would continue being treated with drought until end of October. Therefore sampling at that time, although it could produce differences for some treatments, these would be temporary and for a short time. It is after the wet period when sampling could take advantage of the different drought treatments. That is, from November 1st 2009 until March 31st 2010, all treatments with rainfall manipulation (i.e.

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HC+, MD+ and SD+) received the same precipitation. Thus, it is later, in early spring, when the treatments started to be differentiated again by manipulating their rainfall. Carrying out the sampling in spring 2010 (May 28th, 2010), we had the HC+ treatment with "full" precipitation, the MD+ treatment with one moth of 25% rainfall reduction and, two months of 45% rainfall reduction in the SD+ treatments. We believe that this is appropriate and will provide in the text additional explanations to justify our choice.

In regards to the effect of fire in plants and litter, it should be mentioned that both plant and litter covers were nearly completely combusted as a consequence of fire. After fire both variables were measured periodically, jointly with a number of other variables. The vegetation analysis is now in the process of being prepared as a manuscript by some of the coauthors of this paper. They are happy to provide some background information that could help the interpretation of these results, obviously, without compromising the other paper.

REFEREE#3 GENERAL COMMENT 2 (RGC2): In addition, to evaluate "the joint effects of both drought and fire" (as authors affirmed at line 16, p. 15255) experimental design must include unburned plots for each rainfall treatment. By contrast, only for experimental condition "without rainfall manipulation" an unburned control was included. The reduction in soil water content and in most biological parameters observed with increase of drought in burned area is expected and obvious, and could be not due to fire. Indeed, the experimental design used in this study does not allow to know if reduction of these parameters was due only to drought or also to fire. In this paper there are two data set that should be not discussed together, one concerning fire effects (EC- vs EC+), another drought effects (EC+, HC+, MD+, SD+) on soil microbial and chemical properties.

ANSWER TO GENERAL COMMENT2 (AGC2): We completely agree reviewer #3 that our experimental set up allows us to test only the effects of drought on different aspects of soil processes in a post-fire environment. And additionally, as a secondary objective we also could compare unburned and burned plots with non-manipulated rainfall, to

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verify burning effects alone. As it has been explained also to the referee #1 (AGC1 and AC3), we were fully aware of it. In fact, and in the way the referee #3 suggest with this comment, in the paper we divided the discussion of our results in two sections, one to discuss about fire effect under non-manipulated rainfall conditions and another to discuss about the effect of drought in a post-fire environment. In any case, we will revise the text to avoid any misterpretation.

REFEREE#3 DETAILED COMMENT 1 (RC1): Lines 17-18, p. 15252: the sentence is not true for K, because this decreased in burned plots (see Fig. 2a and lines 7-8, p. 15260).

ANSWER TO DETAILED COMMENT1 (AC1): Thanks for spotting this contradiction. We will reword the text in the abstract following the main text, as suggested by the reviewer.

RC2: Lines 21-22, p. 15252 and lines 21-23, p. 15261: actinomycetes did not decrease in burned soil compared to unburned soil (see Table 2).

AC2: Thanks again. Text will be reworded to reflect the consideration of the comment.

RC3: Lines 4-9, p. 15256: fresh soil must be used only for measurements of ammonium and nitrate, whereas air-dried soil must be used to determine exchangeable potassium, phosphate and soil organic matter.

AC3: It is true that air-drying is the most accepted procedure of soil sample preservation treatment for long-term storage (Tan, 2005). However, it should be recognized that soil can undergo significant changes under any pre-treatment and storage conditions whether soil are refrigerated, frozen or dried before analysis. To our knowledge, there is no problem in using field moist samples for soil analyses, as long as appropriate storage conditions are used (Dick et al. 1996) and results are expressed in dry weight basis. In our study, field moist samples were stored at 4°C until their analyses, which were carried out within a week after sampling. This procedure is also generally ac-

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cepted (Alef and Nannipieri, 1995; ISO 18512:2007) and we followed it. Nevertheless, in the revised version of the paper we will provide more information about soil storage conditions prior to analysis.

RC4: Lines 23- 26, p. 15261: the sentence is not completely true, because fungi and fungi/bacteria ratio were significantly reduced by MD+ and SD+ treatments only in 2011, whereas, bacteria, Gram+ and actinomycetes were significantly affected only by SD+ treatment and only in 2011 (see Table 2).

AC4: Thanks. This sentence will be reworded for consistency with the results in the revised version of the paper.

RC5: The title of paragraph 4.2 “Effect of fire under manipulated rainfall patterns” is not suitable: data did not allow to know the fire effect because experimental design did not include unburned plots for each rainfall treatment.

AC5: We are happy to modify this heading to preclude any misinterpretation.

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Tan, K H. (2005). Soil sampling, preparation, and analysis. CRC press. 680pp.

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Alef, K., Nannipieri, P. (1995). Methods in applied soil microbiology and biochemistry. Academic press. Pp. 608.

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