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Comment

Interactive comment on “Rainfall patterns after fire differentially affect the recruitment of three Mediterranean shrubs” by J. M. Moreno et al.

J. M. Moreno et al.

josem.moreno@uclm.es

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This paper is a relevant contribution to the understanding of the interaction of disturbance events with climate variations, particularly rainfall. Fieldwork studies on the interactions between different factors are complex and extremely useful to provide sound scientific basis for modeling the effects of climate change, and surely the interaction of rainfall patterns and fire disturbance regime is a critical topic for the Mediterranean areas. The paper should therefore be presented in Biogeosciences.

C3.1: The main issue concerning the paper is the lack of data relative to the community composition before the fire events, both in the sampled plots and in a buffer area around them. Indeed species abundances within and around the plots are likely to strongly affect the relative abundances of the emerging seedlings. If data on soil seed bank

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were not available, however data on species abundances in the plots and in a buffer area around them could have been used to analyze the number of seedlings of the considered species, partly controlling for seeds availability.

R3.1: We can but agree that information about the soil seed bank would have been very valuable. However, as commented earlier, obtaining such information was a project on its own right. We then had to assume that, at the beginning of the experiment, plots did not differ in size of the seed bank, and that any subsequent effect was due to the time elapsed since the first burning. The area chosen for study was very homogeneous. Yet, variability exists. We sampled a relatively large area to account for this and minimize the possible effects for the heterogeneity of the vegetation. We will include additional information on the abundance of each of the main species. In addition, a sentence declaring that the soil seed bank was not known, that the effects found are assumed to be due to our treatments and that the possible variability among plots was accounted for by our experimental design and sampling effort.

C3.2 Another issue that may have been solved through the sampling of the vegetation before fires is the role of the other species occurring in the shrubland, namely *Erica arborea* and *Phyllirea angustifolia*. In fact even if their emergence was nil, as stated at the beginning of the materials and methods section, these species are resprouters and are provided with well-developed root systems. Within the sampled quadrats of 50x50cm the occurrence of individuals of these species could have caused hypogeal competition especially affecting the data relative to recruitment. I suggest to the authors to describe in detail the characteristics of the vegetation before the fires and to mention this information also in the discussion.

R3.2: See comment above. We will provide further details about the vegetation of the study site. Nevertheless, we feel that competition between resprouters and seedlings was not an important factor in the early establishment due to the fact that plant cover is very low during the first few years, more so during the first spring, when most mortality occurs. Experiments in which resprouts were removed did not find any significant

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effect on seedling establishment in California chaparral (Tyler 1996; Ecology 77:2182-2195).

Other comments:

C3.3: Figure 6 and 7 are wrongly cited within the text.

R3.3. Agree. Corrected.

C3.4: In the discussion the authors state that fall-winter rainfall have the potential to change plant demography and community composition, while summer drought may not be as critical as usually assumed. However the authors related seedling emergence only with fall-winter rainfall and no relations are shown concerning summer rainfall. I would avoid this statement or support it quoting data not shown if this statement derives from authors' explorative analyses.

R3.4: Our statement is based on the fact that a large percentage of the variance in recruitment was explained by emergence, and this is true at the end of the third year after fire. Furthermore, mortality occurred early on, not later on, and this was already described by Quintana et al. 2004. We have changed the text to indicate this, but have eliminated the reference to "intensification of the summer drought" because what we know is about the past, not the future.

C3.5: I agree with Referee #2 that figures showing all plots (not average values) would be more informative for the reader.

R3.5: See the response above.

C3.6: I found difficult to understand the meaning of year 1-2-3-4, in relation to rainfall patterns and species sampling: are the authors considering a year starting in January or in the summer, this doubt raises especially from figure 1, this should be stated in the materials and methods section.

R3.6: We have introduced a new graph that is self-explanatory. For clarity, we set all

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years to start in October, coinciding with the end of fire season, so that the various years (hydrological, and experimental) coincide.

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