

Interactive comment on “Effects of extreme drought on agriculture soil and sustainability of different drought soil” by S. M. Geng et al.

Anonymous Referee #1

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In this paper the authors show data of changes in soil microbial carbon in a field experiment under maize subjected to varying durations of drought. Changes in the soil biological response to varying levels of drought have been reported previously (e.g. Meisner et al., 2013). The objective of the experiment was to evaluate how varying degrees of drought impacted soil microbial carbon and its recovery following rewetting of the soil.

The subject matter of the paper would be more suited to a soil science journal than HESS. There are no hydrological implications of the research discussed by the authors and therefore I doubt that the research fits within the scope of the journal. For this reason and those outlined below I cannot recommend publication.

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The research appears to also have some significant flaws that cannot be corrected without repeating the experiment and revisiting the methodology. Principal results of the work include hump-shaped relationships between soil water content and soil microbial biomass that change upon rewetting and differ depending upon the duration of drought. A flaw in the experimental design is that the system is planted under maize and that the three different droughts tested were successive such that the results are likely to be significantly impacted by first the increasing density of roots as the maize plants grow and then the subsequent senescence of the root system due to drought stress and/or anthesis. This effect is even suggested by the authors in the Discussion but not implicated in the interpretation. Soil microbial biomass is strongly impacted by the amount and age of roots. Therefore the hump-shaped relation between soil microbial biomass and soil moisture content could just be dominated by a root effect, rather than soil moisture. In addition, the recovery experiments to assess the effect of rewetting would have been impacted by the amount of remaining roots in the soil. As the shortest drought had wetting occurring when the plants were at their youngest (and likely had most roots) then the greater recovery post wetting would be expected for this treatment on the basis of root biomass alone. These effects could have been accounted for somewhat by the use of appropriate controls, though none were used. For example, a bare soil treatment as well as a constant water content treatment could have been used.

Another significant flaw is the apparent lack of replication. For each of the three treatments there appears to be only one treatment, which opens the potential for an impact by spatial variation within the field to significantly impact conclusions. Finally, the methodology hints at “pooling” of results during the first drying, but apparently not during the second drying following rewetting. Therefore, with one treatment and three different sites the recovery is potentially impacted by significant site differences.

Minor comments The English could have been improved substantially prior to submission and would be required to be corrected for resubmission.

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The Methodology lacks significant detail. For example, it is not clear what the reported % soil moisture refers to, volumetric or gravimetric. No information on site layout was provided. It is not exactly clear how samples were “pooled”, how many from each site etc. From what is described the pooling of samples between different treatments occurred during drying therefore there looks as if there was effectively no replication throughout the experiment. It is not clear what Origin 8 is. It is not clear how much water was reapplied to each site.

The Discussion relies on data not presented. In addition, it is a little speculative about conditions in the soil and causes of the changes.

There is no indication of the variability of measured soil properties in Table 1. Table 2 and 3 are repetitious of text and either could have been omitted or the text reduced significantly. Table 4 perhaps too much for one result and it could instead be inserted into the text. Figure 3 and 1 could have been combined. In relation to Figure 3 no data is presented to illustrate temporal changes in total soil organic carbon content with time / water content. Figure 5 repeats the presentation of a significant proportion of the data presented in Figure 4.

The references are generally up to date and relevant.

References Meisner, A., E. Bååth, and J. Rousk, Microbial growth responses upon rewetting soil dried for four days or one year, *Soil Biology and Biochemistry*, 66, 188-192, 2013, doi:10.1016/j.soilbio.2013.07.014.

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