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SOIL
2, C656–C659, 2015

Interactive
Comment

Interactive comment on "Burning management in the tallgrass prairie affects root decomposition, soil food web structure and carbon flow" *by* E. A. Shaw et al.

Anonymous Referee #2

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This is a nicely designed study to assess the effects of contrasting fire treatments on the decomposition of 13C-labeled root litter and subsequent movement through the soil microbial and nematode communities. The topic is interesting and the approach is relatively novel. However, I found the results and conclusions somewhat hard to follow, due primarily to some confusing text and some inconsistencies in the stated results and accompanying figures. Some of these issues should be relatively easy to address, while other may require some additional analysis or redrawing of figures to clarify. I don't think the manuscript is ready for publication in its current form, but a revised version may be. I provide both general and specific comments below.

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Although it is true that fire is "managed" today in most areas of tallgrass prairie, fire was historically an important natural factor driving ecological processes in these grasslands. Naturally occurring fires are also important in many other productive grasslands globally. As a result, I question why the authors appear to discuss fire only in the context of a management practice in this manuscript (beginning with the title "Burning management in the tallgrass prairie...", and continuing throughout the manuscript). Wouldn't the study have broader appeal by referring to the effects of fire per se, whether the fire is prescribed or naturally occurring? For example, line 23 could be altered to read "This is especially important in grasslands where fire is common and removes aboveground litter..." rather than "This is especially important in grasslands where fire is a common management practice and removes aboveground litter..." Likewise the words "management practice" could be removed from line 45, and elsewhere, without altering the meaning of the sentence and making the results more relevant to grassland fires in general. If you then want to note that fire, as a management practice, can affect soils and soil biota, you could do that with the text as written on page 4 (lines 66-70). I expect that many of the effects of prescribed and natural fires are similar (both remove aboveground detritus, both alter the soil microclimate, etc.) One might argue that some effects of prescribed and natural fires could vary, based on timing, intensity, etc., but that could be brought up in the Discussion, if the authors feel that is relevant.

The use of 13C-labeled plant root litter to follow detrital C through both microbial pools and consumers (nematodes) is novel, and a valuable approach for assessing how fire alters soil food web and associated C flux. In that regard, this paper contributes some novel data and insights.

Line 46 – stating that fire affects the soil community and root decomposition in "Konza Prairie LTER soils" seems too limiting and site-specific. Why not broaden this to tallgrass prairies soils, or something similar?

Lines 126-127 – Could the authors be more specific with respect to hypothesized differences in soil and nematode communities between the contrasting fire treatments. The

SOIL

2, C656–C659, 2015

hypothesis that will be "different" is OK, but are there more specific predictions that could be made based on what is known about effects of fire of organic matter inputs in these grasslands, changes in soil microclimate, etc.?

Line 136 – remove extra "the" before Long-Term Ecological Research site. . .

Lines 275-276 – There appears to be different notations used in the formula (fR) and in the corresponding text (fr). Fix this so that capitalization is consistent.

I found portions of the Results section to be confusing. I think this is because some of the conclusions drawn in the text are not apparent in the figures that are referenced to support them. In addition, I had trouble interpreting some of the figures/figure legends. Some specific examples follow:

Line 310 – Authors state that PLFA abundance was significantly lower for the AB than for IB treatment and refer the reader to Fig. 2. However, Fig 2. does not explicitly include comparisons of either individual PLFA groups or total PLFA for all groups among fire treatments (i.e., panel A vs. panel B). Do the authors mean that PLFA averaged across all functional groups was significantly lower for AB than for IB treatment? If so, that should be explicitly stated in the text. Same comment applies to reference about bacterivores being more abundant in AB and plant parasitic nematodes being more abundant in IB (lines 314-315). This is not readily apparent in Figure 3. Note: "abundance" in line 314 should be "abundant".

There are also some issues/inconsistencies in the figures and figure legends. For example, the caption for Fig 2 indicates that data are based on n=3, but that's not consistent with statements in the Methods that there were 4 replicates per treatment/harvest date (see lines 187 and 203). Why were only 3 replicates used in Fig. 2? The legend in Fig. 3 uses lower case 'a' and 'b', but should be upper case to be consistent with the figure labeling and with other figure legends. The x-axis in Fig. 4 is in units of months, while other figures with a temporal scale are in units of days. In addition, because the time between collection intervals is not evenly spaced, the figures that have a temporal

SOIL

2, C656–C659, 2015

Interactive Comment

x-axis should have those points scaled/spaced to reflect the actual time between collection intervals (i.e., in Fig. 5, the interval between 3 and 10 days is presented as the same as the interval between 90and 180 days, resulting in very misleading temporal patterns of C incorporation into the biota). This may or may not affect the authors' discussion of temporal dynamics of litter C movement.

Lines 324-328 – This section of the Results refers to changes in the nematode community driven by the addition of litter, and the time since litter addition. The statements about temporal changes following litter addition reference Fig. 3. However, it appears that Fig. 3 includes only data from the litter addition treatment! How can we know then that the temporal changes are due to the litter addition, and not just changes in the community over the course of the 180-day incubation? In order to demonstrate that the changes in nematode are a response litter addition, you would need to compare the temporal dynamics of nematodes in the litter-addition vs. the non-addition soil cores. Why was that not done here? In fact, I don't see any data from the non-addition cores in any of the figures, except Fig. 1. It seems to me that comparisons of changes in microbial and nematode communities over time in soil cores with and without litter additions would be a key part of this story, especially if the authors wish to attribute temporal changes to the addition of litter. Can the authors add these data, where appropriate?

Line 422 – The Johnson and Matchett reference seems out of place here. I don't think that reference deals at all with the effects of pyrogenic material. In fact, there are other ways that burning can promote N limitation besides adding pyrogenic OM, such as by increasing inputs of detritus with a wider C:N ratio. There are many references to support that in tallgrass prairie.

Fig. 1 – It is difficult to read the small font used to highlight groups with top 'species' scores on these graphs. In addition, the groups are not really 'species' right? Might be better to refer to them as functional groups or something similar?

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SOIL

2, C656–C659, 2015