

Interactive comment on “Decreased carbon limitation of litter respiration in a mortality-affected piñon-juniper woodland” by E. Berryman et al.

Anonymous Referee #2

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General Comments

The manuscript provides revealing results about the CO₂ pulses after rewetting in ecosystems characterised by a drought period, and about the effects of the massive death of Pinus-Juniper woodlands on these pulses. However, I have several concerns about how results are presented and interpreted. I also consider that some of the main conclusions should be taken much more carefully. Several methodological constraints also limit enough time resolution to capture the soil flux dynamics.

Regarding the observed absence of water limitation in mineral soil, in many ecosystems characterised by a drought period, the uneven rain events that occurs during or

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at the end of the dry period are larger than the simulated here. This would have important implications in the results, since it would enhance the mechanisms involved in the release of labile sources of C and, in consequence, the CO₂ pulses: the disruption of soil aggregates and extrusion of microbial metabolites in response to osmotic stress, the collapse of part of the microbial cells and the release of cytoplasm and metabolites, etc. In addition, this release of labile C could promote a “priming effect” leading to the mineralization of more stable sources of soil organic matter in the soil. Thus, the water and C limitation of soil respiration in dry ecosystems might be coupled, making challenging to disentangle the individual water limitation.

I also have concerns about how the authors calculated and interpreted time courses of the ‘limitation factor’. Observing the figures 2 and 3, one may think the water and C limitation decrease along the time since the treatment applications were performed, whereas is just the opposite. This ratio just indicates how different the CO₂ fluxes are in treated chambers with respect to the untreated ones. The use of the expression “limitation factor” should not be used in this case. Instead, use the rate of CO₂ increase with respect to the untreated or water-treated chambers (see Kim et al. 2011)

Kim, D.-G., Vargas, R., Bond-Lamberty, B., and Turetsky, M. R.: Effects of soil rewetting and thawing on soil gas fluxes: a review of current literature and suggestions for future research, *Biogeosciences*, 9, 2459–2483, doi:10.5194/bg-9-2459-2012, 2012.

Moreover, the length of the CO₂ pulses can be influenced by several factors included the labile C pools in the soil. Thus, the selection of the ‘time window’ to calculate the accumulated limitations cannot be arbitrary. This impedes the correct interpretation of the accumulated limitation factors.

Finally, soils usually experiment a fast response to rewetting (fast mineralization of the labile organic pools that become available by the effect of rewetting) and the observed response CO₂ pulses can last from hours to several weeks. For this reason, high temporal sampling is needed to correctly record the temporal evolution of soil CO₂

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pulses. As the magnitude and length of these pulses depend on the amount of water added to the soil and the amount of water added in this experiment was quite small, it may have happened that an important part of the pulse have not been registered, thus underestimation the response to the application treatments.

Specific Comments:

Introduction

Page 14477, Line 6: It would have helped to add that the predominant heterotrophic contribution to these pulses happen because, at these point, most of the annual grasses is dried and the respiration of the roots is not expected to have a great contribution as long as the deep roots of the trees are not willing to access immediately to the water that stays in the superficial soil layers.

Page 14477, Line 14 and along the manuscript: Temperature plays a more important role when moisture and C are not limiting factors, but this does not happen always when moisture and C are in high levels (soil pores saturation and reduction of diffusion within the soil matrix, anaerobic conditions, recalcitrant C, etc). I would change 'high levels' by 'not limiting conditions'.

Page 14477, Line 23: I would add at the beginning of this paragraph a sentence that highlights the relevance of these CO₂ pulses for the net annual CO₂ emissions or for the net C balance.

Material and methods

2.2 Soil litter and properties

Page 14480, Line 18: Where the piñon canopies different at each site?

Page 14480, Lines 15-18: Where the soil moisture measurements coupled to soil respiration, inside the chambers? Differences could be also due to the microstructure and porosity in each site, which affect to the desiccation and time courses of soil moisture

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2.3 Experimental treatments

Page 14480, Line 26 and along the manuscript: Substitute the expressions ‘mineral soil applications and litter applications’ by ‘solution applications to litter or mineral soil’.

Page 14481, Line 4: Was the litter replaced or removed? This would have different implications for the interpretation of the results. If it was removed, why this was done after the treatment applications?

2.5 C and water limitation calculations

Page 14482, Lines 4-5: This detail is not relevant for this manuscript. In this case is not possible to ensure that the treated chambers are in absence of water and labile C limitations

Page 14482, Lines 6-7: Even if a small amount of water is added, physical displacement of soil CO₂ cannot be avoided. Water always would provoke a displacement of the gas that is in the soil pores.

Page 14483, lines 11-14: A more detailed explanation about the selection of time window for the calculation of cumulative limitations.

2.6. Statistical analyses

Page 14484, lines 20-21: Did you try to improve normality and homoscedasticity of the data by transformation before applying a non-parametric Mann-Whitney rank sum test?

Discussion

References to the figures and tables would help to follow the discussion of most relevant results.

The effects of rewetting can decline with successive drying and rewetting cycles. This could be an explanation to the more accentuated soil respiration responses observed on July (Experiment 1) compared to August (Experiment 2). To exclude for this effect

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the experiment should have been performed in different experimental areas for each of these two experiments.

Page 14486, Line 17: I only see a clear limitation of C availability in the case of the litter but not in the case of mineral soil, this has to be specified (Table 1, Fig 1, Fig 7)

Page 14486, Lines 20-21: See general comments, paragraph 2, about coupling effects between water and C limitations

Page 14486, Line 21: The stimulation of respiration of water additions is not significant for mineral soil in August. Here, it must be specified that water additions stimulated litter respiration. The effect of the previous irrigation treatment in the same sites in July can be a possible explanation for the absence of response observed in August (Table 1)

Page 14487, line 1: The reduction of the labile C limitation is restricted to the litter. This cannot be extrapolated to the mineral soil.

Table 1: The discussion or interpretation of figures must be in discussion section and not in footnotes. Instead, use this space to specify the meaning of each parameter.

Table 2: It would be appropriate to see the results of the statistical test in the table.

Fig. 1: Change the colour of symbols for water and water + sucrose treatments. They cannot be visualized clearly. Changing the range of the x axes (up to 8) would also help

Fig. 7: The explanation given in the foot note about the negative values of the limitation factors must be wrong. Should not be 'negative proportion represent a treatment where the water addition yielded minor respiration than untreated treatments'?

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