

Interactive comment on “Soil carbon dioxide emissions controlled by an extracellular oxidative metabolism identifiable by its isotope signature” by B. Kéroual et al.

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Anonymous Referee #2 (Received and published: 17 May 2016)

Referee2: “This excellent study shows the occurrence of extracellular respiration in soils and discusses the involved pathways. Even if addressed in earlier works, the question of extracellular or abiotic production of CO₂ is of broad interest for the conceptual representation of soil organic carbon mineralization. The study is one of the best conducted on this subject. Even if research has to be continued on this question, these are new concepts and ideas in this study, which are worth being published yet. The initial manuscript has been clearly improved in this new version. I therefore consider the manuscript as acceptable for publication.”

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Response: We thank the referee for his support and help.

Referee2: “Concerning section “4.4. Towards a quantification of EXOMET and cellular respiration in living soils”. Results of figure 3e and corresponding equation page 16 that relates δ¹³C of CO₂ to DOC could be explained through two processes of CO₂ release by exomet: one involving (almost) no fractionation and the other highly fractionating, and probably from carbon derived from extracted/heated organic matter. The linear relationship between δ¹³C and DOC concentration might be as well explained by a proportion of the second process in the CO₂ efflux, which is itself correlated with the extraction level of carbon by treatment, as by a reservoir size dependent kinetic expression of the ¹³C fractionation factor. The proposed method to quantify exomet through ¹³C signature thus makes sense, but the equation that relates the isotope fractionation to DOC concentration should not be considered as generic.”

Response: We completely agree with this point which has also been raised by the first referee. We have modified the text to clarify this limit and suggest studies that could be conducted to overcome these limits (page 14 line 15; page 15 line 17).

Referee2: “According to the data, labelled glucose is a source of exomet CO₂, but is not the dominant source. The conclusion that exomet can achieve a respiratory-like metabolism doesn’t exclude the occurrence in parallel of more partial mineralization processes, e.g. involving methoxy or carboxyls etc. Complete mineralization of complex molecules such as glucose would furthermore lead to smaller isotope fractionation than observed.”

Response: We agree with the idea that there are a few questions remaining within the results that highlight that the extracellular metabolism is still in the hypothesis phase. Therefore, page 16 - line [20-23], our terms were moderated: “Collectively, our results tend to sustain the hypothesis through which soil C mineralization is driven by the well-known microbial mineralization and an EXOMET carried out by soil-stabilized enzymes and by soil mineral and metal catalysts.” We have also specified that the causal link

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between the magnitude of fractionation and the DOC content is not certain since the correlation emerges from a compilation of results obtained after different sterilization treatments. Further studies should analyze this causal link in experiments where the DOC content is directly manipulated and the change over time of the isotopic composition of DOC is quantified (page 14 line 15; page 15 line 17).

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