

## ***Interactive comment on “An unknown respiration pathway substantially contributes to soil CO<sub>2</sub> emissions” by V. Maire et al.***

**Anonymous Referee #3**

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Maire et al present a provocative manuscript showing CO<sub>2</sub> efflux from sterile soils. They then go on to propose a mechanism by which stabilized extracellular enzymes, perhaps even in intact membranes, are able to perform respiration.

Overall, I think this is an elegant study, with important implications. However, I think the writing could be improved. First, I think the authors should be clear in distinguishing observations from speculation. They present strong evidence for abiotic CO<sub>2</sub> flux, and even that extracellular enzymes are involved. However, the mechanism is indeed unknown, and may not even be respiratory. This should be reflected in the title- I would delete the word ‘respiratory’. Structurally, the introduction is too brief and doesn’t really identify the problem that this study addresses- are there cases in which CO<sub>2</sub> flux is not explained by biological processes?

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One aspect where the paper could be improved is in discussing the implications of this proposed mechanism for ecosystem function. In particular, I would ask them to consider how respiration is modeled in ecosystem models, and whether this mechanism is implicit, and whether the models would need to be modified.

The authors state that this can be a substantial fraction of total respiration. The calculations for this finding were somewhat unclear to me. Beyond that, I would question whether this statement is supported- we don’t know how well this process would compete with intact microbes- whether turnover time of these enzymes would increase with biotic activity, etc. Further, although this process is abiotic, it would only be decoupled from biology for short periods of time- it relies on living microbes to produce the respiratory enzymes, and to produce enzymes to depolymerize organic matter. What are the implications for system energy dynamics, DOC production, etc for a molecule respiration through Exomet vs microbes? Can this mechanism explain other observations that have been difficult to explain- perhaps respiration following freeze-thaw or wet-dry?

It would be helpful to offer some comments at the end on what the remaining and follow-up questions are regarding Exomet. I would replace the final sentence of the current conclusion-which is pretty wild speculation, and elicited a laugh from this reviewer- since these enzymes were produced by living microbes to begin with.

Is this novel? You bet. Sure, others including Kemmit have suggested that respiration can be decoupled from microbial biomass or activity, but this takes it to a new mechanistic level. My own lab has observed respiration from yeast extract, but we never followed up on this observation. This is a nice series of experiments carefully testing each mechanism. I think a paper like this is prone to criticism- there is always something that wasn’t measured, always an alternative explanation for results- easy fodder for a skeptic. For example, the enzymes could be directly detected with proteomics, the enzymatic products could be measured. But, the authors are careful in their wording to present this as a proposed mechanism that can be further tested. From a biochemical perspective, it is hard to imagine how this mechanism could persist for any length

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of time. Yet, I found the observations convincing. Just because it is hard to believe given our current paradigm does not mean it is wrong. I encourage the advancement of science by publishing this paper. Time will tell if this was a naïve idea with a simple explanation, or a breakthrough.

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Interactive comment on Biogeosciences Discuss., 9, 8663, 2012.

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