

Interactive comment on “Short Communication: Quantifying and Correcting for Pre-Assay CO₂ Loss in Short-Term Carbon Mineralization Assays” by Matthew A. Belanger et al.

Anonymous Referee #1

Received and published: 1 November 2020

This short communication looks at the methodological artifact of soil dry-down on measuring active soil carbon as CO₂ burst. The topic would be of interest to readers of SOIL, although the manuscript may be even better fit for journal like SSSAJ since prevalence of this method in the US. I have some general concerns/suggestions and specific comments to help the authors improve their manuscript.

1. There is considerable research on soil moisture content and biological activity of soils, including dry-down and wet-up. This should be discussed more in Introduction and Discussion. There are several studies that show that the drier soils are in the field, they tend to release more CO₂ upon re-wetting regardless of dry-down C loss. Thus,

C1

there are biophysical mechanisms at play other than dry-down C loss. Figure 3 still shows more CO₂-C from the 30

Borken, W., Davidson, E. A., Savage, K., Gaudinski, J., Trumbore, S. E. (2003). Drying and wetting effects on carbon dioxide release from organic horizons. *Soil Science Society of America Journal*, 67(6), 1888-1896.

Jarvis, P., Rey, A., Petsikos, C., Wingate, L., Rayment, M., Pereira, J., ... Manca, G. (2007). Drying and wetting of Mediterranean soils stimulates decomposition and carbon dioxide emission: the “Birch effect”. *Tree physiology*, 27(7), 929-940.

Liang, L. L., Grantz, D. A., Jenerette, G. D. (2016). Multivariate regulation of soil CO₂ and N₂O pulse emissions from agricultural soils. *Global change biology*, 22(3), 1286-1298.

2. These findings need to be placed in context of the purpose and practicalities of CO₂ Burst test. Drying is often needed, if not necessary, to stabilize microbial activity before analyzing for CO₂ production. Whereas accounting for C loss during drying might not be feasible or possible for commercial and even research labs. This would be very onerous. How much more are we gaining by accounting for this C? This paper has not convinced me that we gain much. Despite finding differences between 30 This paper would be much stronger if it included a comparison of two or more treatments, and showed that measuring CO₂ Burst at different moisture contents obscured our ability to detect treatment differences. There are always experimental artifacts with incubation-based, laboratory measurements of soils. A few studies have shown that using more soil reduces variability. The most important thing is that we treat soils the same across time or space, and that the methodology is not creating confounding effects.

SPECIFIC COMMENTS:

L24. Replace ‘the reintroduction of moisture’ with ‘rewetting’

L33. Delete ‘different’

C2

L38. How was it collected? Shovel, or soil probe? More details are needed here.

L43. So total MAP is 2305? This seems very high.

L49. What was the initial water content of the field soil when you collected it? Was it below 30

L85. This seems overly complicated. Why not use area-under-the-curve to calculate cumulative CO₂?

L110. Why does respiration go back up at 8d? This is interesting and looks like there might be treatment effect?

L131. Why use standard deviation in this graph? Fig. 2 uses standard error. I suggest being consistent. Also, use same colors in Fig. 1 and 2 for consistency. Place letters to abbreviate significant differences among means in both Fig. 1 and 2.

Interactive comment on SOIL Discuss., <https://doi.org/10.5194/soil-2020-55>, 2020.