Biogeosciences Discuss.,
doi:10.5194/bg-2016-474-AC1, 2017

Interactive comment on "Interactions among temperature, moisture, and oxygen concentrations in controlling decomposition rates" *by* Carlos A. Sierra et al.

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Received and published: 23 January 2017

We thank reviewer 1 for his comments and for actively engaging in the discussion motivated by this manuscript (see comments to Reviewer 4). Here we quote his comments in *italics* and provide our answers below each major comment.

This paper reports a relatively simple factorial experiment of soil respiration response of moisture, temperature and oxygen. This is an important topic if we are to accurately model respiration of soils in temporally and spatially variable environments. One might think that these relationships have been well constrained already but when trying to find specific examples in the literature it is not easy to find many examples. A simplification of the DAMM model is used to explore data. A nice addition is inclusion of an

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oxygen treatment to distinguish between the role of water in controlling oxygen supply and carbon diffusion. The paper is easy to read and follow and I generally have few comments. I am not really expert in modelling side of soil carbon dynamics and will limit my comments here.

We think one of the reasons there's relatively little work on this subject is because the difficulties in controlling three factors simultaneously. Full factorial experiments involving more than two factors are not so common in ecology even though they can help us to better understand multiple factor interactions among environmental drivers.

Specific comments 1. While a high C content soil was supposedly selected to avoid carbon limitation during the incubation this does not mean that the labile fraction of C would not be depleted. This is important as it is possible for depletion of labile C occurs faster at higher temperatures. The authors can check whether this might have occurred by examining the timeline of CO2 production – if carbon supply was not limiting then respiration rates should be linear and not reach a plateau. Do authors have this information? Currently reporting only the total CO2 after 35 days.

We do have this information. Cumulative CO_2 production from all treatments is presented in the Figure 1 below.

The treatments with the highest amounts of respired CO_2 showed almost linear increases in respiration and provide no evidence of a depletion of labile carbon. Treatments with low water filled pore space (WFPS) and low oxygen levels show a tendency to reach a plateau, but this is likely due to a strong decrease of respiration rates due to water and oxygen limitation and not due to substrate depletion. The soil is the same for all treatments so we expect labile carbon to be the same in all cases.

The results from the model optimization shows that the fraction of fast 'labile' carbon is around 56% of the total initial carbon in the incubations (Table 1 in new version). This is common for these highly organic soils and it is highly unlikely that this labile carbon is depleted during a 35-day incubation.

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2. Alternatively, a rise in rate through time would indicate adaptation and/or microbial growth during the incubation. Are the authors confident during the 35 days that microbial adaption to constant moisture, temperature and oxygen conditions has not occurred? If this does occur then the model fitting data between different microbial populations. The authors need to acknowledge these possibilities and present some information or rationalisation to overcome them.

The cumulative respiration data in Figure 1 below, presents clear and distinct trends for each of the treatments. Treatments with high levels of temperature, moisture and oxygen show near linear trends, which is an indication that the microbial communities are growing at an almost constant rate and are not experiencing any resource limitation. In treatments with low resource levels (moisture and oxygen), microbial growth declines during incubation time suggesting depletion of resources necessary for growth. We do not believe that the linear trend in high-resource treatments is an indication of microbial adaptation that somehow would invalidate our results. On the contrary, this is a strong indication that growth, and therefore respiration rates, is not limited by the levels of available resources. More importantly, the slope of the near-linear increases in these treatments is highly dependent on the factor levels, a strong indication that rates depend on the three main factors imposed, something that is later backed up by our modeling analysis.

3. What was the temperature range in the field that the soils are exposed to? This soil was collected from the active layer at a Caribou/Poker Creek watershed in central Alaska. The active surface layer is exposed to large changes in temperature during the year, from a minimum temperature close to -19°C to a maximum of 18°C, so the annual temperature range is close to 40°C. This information was obtained from the Bonanza Creek LTER site, data summaries for the CPCRW station (http://bnznet.iab.uaf.edu/vdv/vdv_historical.php). We included this information in the site description section of our manuscript.

4. What bulk density was the soil packed to in the cores? Do these represent what

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might be observed in the field?

Each cylinder had a volume of 1570 cm^3 (10 cm diameter, 20 cm height), and contained 450 g of soil. About half of the cylinder was filled with soil, so the approximate bulk density was 0.57 g cm^{-3}. Typical values for bulk density in organic horizons and peats are between 0.1 and 0.5 g cm^{-3} (Hossain et al., 2015). We believe the bulk density within our cylinders was realistic and corresponds to typical values for these type of soils.

5. Pg 3 8 is 'fallowed' meant to be 'followed'?

Changed.

6. Pg 4 ln 5-10 Include abbreviations O, Ko, W in text

Done.

7. Pg 4 ln 23 There were only two temperatures used so that statement respiration did not decrease at higher temperatures should strictly be singular "at the higher temperature".

Changed as suggested.

8. What are the error bars on fig 2? Fig2 I also printed this out in black and white and it was very difficult to see what line was what, symbol could be changed and a dashed line used.

Arrows represent 25 and 75% quantiles of the distribution of the parameters obtained through Bayesian optimization. This information was added to the figure caption. Symbols in figure were changed to improve readability in white-and-black print outs.

9. Figure 2 and 3 this not really my area and I think a little more description of what these graphs mean would be useful.

We added more description on the main text and on the figure captions.

10. Figure 4 is it reasonable to make prediction of a full curve of temperature response based two temperatures? And furthermore make prediction above and below the tem-

perature measured? Similarly a very steep curve is predicted for the oxygen content response between two end points.

Good point. Here we only wanted to show the predictions from a model that fitted well the data at the specific points within possible ranges for the controlled variables. We believe it is also interesting to know what the model predicts within and outside the range of possible values. However, one must be very careful with the interpretation of these results since, as the reviewer points out, we do not have data outside the specific points where we imposed our treatments.

To address this issue we modified Figure 4, showing explicitly predictions for the specific treatment combinations where we have data, and only plotting the predicted curves as dashed lines for reference.

11. Pg 7 ln 5. I disagree with the statement of increases in temperature being almost always associated with decreases in soil moisture is really a matter of temporal scale of interest. For example between seasons this is certainly possible wet and cold vs hot and dry and this would allow microbes time to adapt. But increases in temperature diurnally can also occur. It would be not unusual for a soil to cycle by 5 to 10 C during a 24 hour period where moisture content would be steady and there is less time for adaptation.

Yes, this is a matter of scale. At some time scales, increases in temperature are accompanied with increases in moisture (e.g. as soil unfreezes), it may dry at other scales (e.g. in the spring season in mediterranean ecosystems), or it may remain constant during a 24 hour cycle. We explored these different dynamics in a previous manuscript (Sierra et al., 2015, Fig 1), and concluded that in a large number of relevant cases, soil temperature and soil moisture change simultaneously.

12. Pg 9 Conclusions and discussion. That the authors did not find a decline in respiration at a single higher temperature (35C) but this does not mean that MMRT or similar functions are not important in moderating microbial responses in soil. The authors only had two temperatures 25 and 35 C. For the respiration rate to be lower *at 35C than 25 C would require the temperature optimum (temperature at which the respiration rate is maximal) to be closer to 25C than 35C. If a temperature optimum for soil respiration was near or greater than 35 C there would be no observed decline in respiration in comparison to the rate at 25C. If I have my logic correct then there is no support for the argument in the conclusions that scale in this case matter with respect to extrapolating MMRT from enzymes to soil systems.*

The reviewer is right here, and we acknowledge that our logic was flawed. It is correct that our two temperature treatments may not be enough to observe any potential decline in respiration rates as predicted by the MMRT, and this has little to do with scales. However, we still believe that there's an important mismatch between the scale at which the MMRT operates and the scale for observing soil respiration in soil cores and soil pits. The MMRT predicts a decline in respiration at high temperature provided all other environmental factors remain constant. But in most cases in soils, both temperature, moisture and oxygen change simultaneously. Our point with the manuscript is to bring to the attention that these interactions are very important for predictions at the soil core level, even though there may be mismatches with the predictions at the enzyme level.

To address this comment we re-wrote the conclusion section and are now more precise about the limitations of our experiment and the mismatch among scales.

References

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Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-474, 2016.

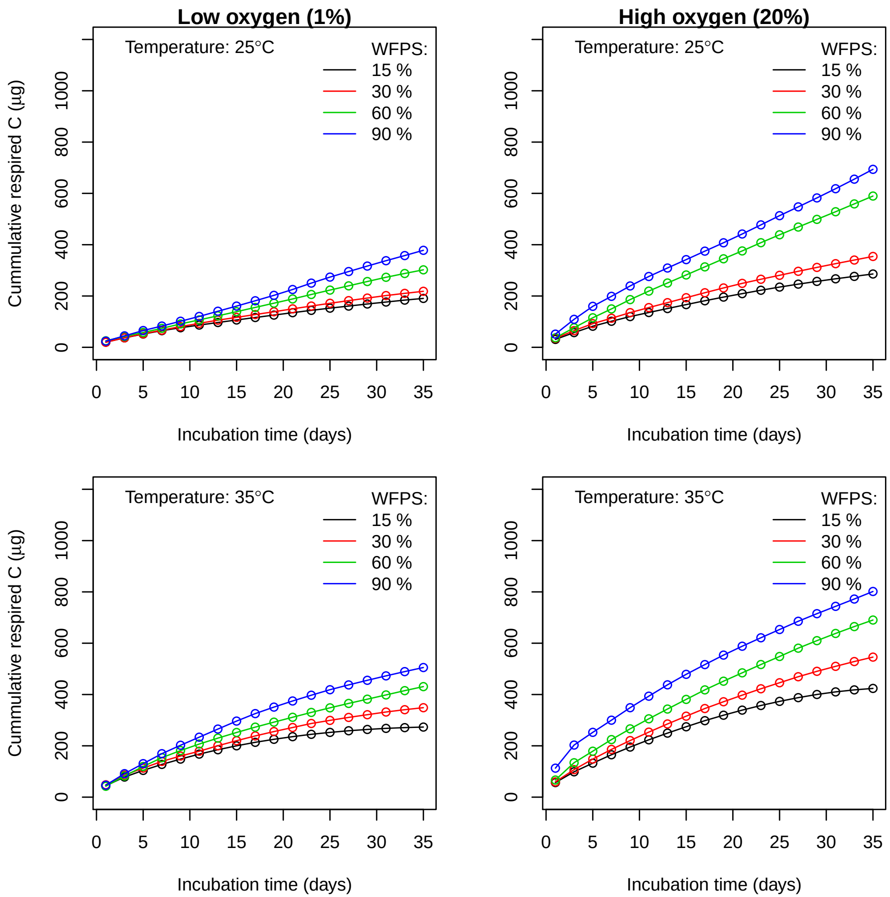

Fig. 1. Cumulative respiration for all treatments in the incubation experiment.

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