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

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We thank reviewer 3 for his/her comments on our manuscript. Here we quote comments in *italics* and provide our answers below each major comment.

P1 Line 7: The site is boreal, not arctic.
Yes, we changed to boreal.

P1 Line 9: The conclusion about temperature effect need to be tempered or qualified in the context of the limited range of temperatures evaluated.
We modified this sentence slightly, not mentioning that decomposition 'increases' with 'increases' in temperature, since we only have two temperatures; but mentioning that

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decomposition rates 'were high' at high temperatures provided oxygen and moisture were not limiting.

P1 Line 10: This is a significant conclusion, even though it seems relatively obvious- having a good experimental design to say this conclusively is useful.
Thanks.

P2 Line 4: How does this 45C threshold correspond to your high temperature? Is 45C broadly constant across ecosystems?
We mention this 45°C threshold only to introduce the MMRT, which is supposed to operate at lower temperatures than this threshold for enzyme denaturation.

P2 Line 24: True, and a major strength of this study.
Thanks.

P3 Line 8: Again, boreal, not arctic.
Changed.

P3 Line 9: This statement is not necessarily true depending on the content of labile, readily respired substrate. It should be explored a little further and contextualized with other studies that evaluate substrate limitation of soil respiration in organic soils, especially from boreal regions.
We believe that high organic soils minimize the potential of substrate limitation, but the reviewer is right in that this may not be the case always. For clarity, we added the word 'may' to this sentence.

P3 Line 18: It is unclear exactly how this measurement was used to evaluate the soil respiration or decomposition rate. Can you please clarify? Was it evaluated as change over a set time interval, or as increase over the known background from the input air? At what frequency was this measured?
For each cylinder, fluxes were measured every other day as the difference in concentration between the output and input air, multiplied by the air mass flow rate. We added a

more detailed description about the quantification of respiration rate to the new version of the manuscript.

Eqn 1: Please be clear about what exactly dC/dt represents. Is it the instantaneous or the cumulative dCO2, is it CO2 or CO2-C?
Here, dC/dt represents the instantaneous change in the carbon content for the incubated soil. The respired CO_2-C is obtained after solving the system of differential equations and calculating the output flux from the numerical output. We added more details on the model description to make this clear.

P4 Line 10: Does this mean that gamma also varies by each treatment level? And initial C1 and C2 also vary by treatment level? I would like to see some presentation of the actual C fluxes, and the change in C1 and C2 over time.
Yes, the values of γ change for each treatment level for the first optimization and this is presented in Figure 2. For the second optimization, we obtain a probability distribution for γ, which is presented in Figure 3.

P4 Line 12: Fitting the full model in eqn 2, is gamma now fixed? Also, are there limitations to fitting a q10 function with only two temperature points?
Again, for the second optimization, where equation 2 is set explicitly, we obtain a probability distribution for γ. It is not a value that changes from one treatment to another, but a range of values with some probability.

The main limitation of fitting a Q_{10} function with two temperature values is that the obtained uncertainty range is very high, which is evident in the probability distribution presented in Figure 3, and the predictions in Figure 4.

P4 Line 14: Thanks for presenting this supplement.
Thanks for the comment.

P 4 Line 27: I am more surprised at how similar the k1 and k2 values in Fig 2 are across such a broad range of O2: Can you explain this result more clearly.

This is well explained by the sensitivity functions in Figure 4. The intrinsic sensitivity of decomposition rates with respect to temperature is higher for temperature, intermediate for moisture, and lower for oxygen for the treatment levels we selected.

P5 Line 5: Can you please elaborate a little further on fig 3? We do see a few seemingly high correlations that might be worth describing in more detail. For instance, Ko and ks.
Here the concept of 'high' correlations is relevant. For exploring collinearity between parameter sets we are interested in finding correlations above 90-95%. This would be indicative that parameter values lie within a straight line. In analyses of ecological data, researchers often describe correlations above 0.3-0.4 as 'high' due to the inherent variability of ecological processes. However, the aim here with Fig 3 is to find near linear correlations as evidence of collinearity among parameter values. Therefore, we do not consider the obtained correlations as high for the purpose of our analysis.

P5 Line 6: Am I missing the posterior parameter estimates? It would be very useful to have a table of these parameter values and credible intervals.
We added a table with these parameter values and their uncertainty.

P5 Line 7: Why did you use this range rather than the 2.5-97.5? It looks like your estimates of the temperature function might be challenging in that case, which isn't that surprising with only two temps. I think it is worth revising these figures to have both the 25-75 and then standard 95% credible interval presented.
We included now the 5 and 95% interquartile ranges for the second optimization.

P7 Line 1: The discussion should give some analysis of the temperature response. In particular, how do the estimated q10 values compare to other q10 values using soils from similar boreal forest sites? Also, what is the temperature range at this site? You describe the 45C threshold in the introduction, but then use a much lower temperature as the high temp. Is this higher than temperatures the soil organisms at this site regularly experience? Is it higher than projected future temperatures for this site?

Our new version of the manuscript includes a discussion on the obtained dependence and sensitivity functions for temperature, moisture, and oxygen, but we do not include a discussion on comparing the obtained Q_{10} values with others found in the literature.

The objective of our modeling analysis was to obtain relevant parameters for the interpretation of the experimental results. We are not concerned here on describing or interpreting our parameter values as representative for modeling this type of soils under non-experimental conditions. Under field-conditions other physical and biological processes may have also a strong effect on decomposition and respiration rates not relevant under the experimental conditions of our experiment. For this reason, we are reluctant to compare the dependence function and the Q_{10} values against others found for other type of soils under completely different measurement, experimental, and modeling setups. Previously, I have strongly criticized the practice of comparing Q_{10} values from different studies using different functions (Sierra, 2012), and do not consider appropriate to do such a comparison here.

P8 Line 3: Can you please describe some of these interactions more specifically? It seems as though a lot of the work is presenting marginal responses. Is there some reduction of temperature sensitivity at high water content? or a reduction of oxygen sensitivity at low water content? Please clarify what interactions you mean.
To address these interactions more explicitly we introduce a new figure (Fig 5) calculating the value of ξ for the specific treatment combinations. This figure help us to discuss the interactions among the three variables in more detail.

P8 Line7: I am not sure I follow. Looking at figure 1, most CO2 was respired at high water content. Are you thus comparing the low to the high oxygen rates, and then inferring a response in the absence of continuous oxygen flow? Please be explicit about that.
This sentence refers to the study of Tucker and Reed (2016) and not to the results presented in our manuscript.

P9 Line 3: Perhaps discuss more thoroughly how the inclusion of dynamically changing air-filled pore space might relate to your results. This is a suggestion, not a necessary revision.
We extended this paragraph to give better details about how the model can be modified for representing more complex processes or for applications to the field level.

Technical corrections
*P1 Line 16: *significantly*
Done

References

Tucker, C. L. and Reed, S. C. (2016). Low soil moisture during hot periods drives apparent negative temperature sensitivity of soil respiration in a dryland ecosystem: a multi-model comparison. *Biogeochemistry*, 128(1):155–169.
Sierra, C. (2012). Temperature sensitivity of organic matter decomposition in the arrhenius equation: some theoretical considerations. *Biogeochemistry*, 108(1):1–15.

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