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

*p. 3, line 5: The soil columns contained 450 g of homogenized soil. It would also be good to have an idea of the dimensions of the columns (diameter, height). This determines, for instance, the surface area that is subjected to drying and the distance that the oxygen flow travels through the sample. Further, an estimate of the bulk density of the soil or of the proportion of pore space in the samples would be helpful. It is especially important that the pore space was similar for all soil columns so that differences in diffusion potential of oxygen, water and temperature does not influence the results.*



For each cylinder, we had 450 g of soil in a volume of about 785 $cm^3$, which results in a bulk density of about 0.57 g $cm^{-3}$. This was similar for all samples and care was taken to have similar bulk density across all treatments.

It is important to keep in mind that soil water modifies the amount of filled pore space, and for this reason our moisture treatment is expressed in water-filled pore space. This obviously has consequences on the diffusion characteristics for each moisture treatment, which results in the observed differences in respiration rates.

*p. 3, lines 9-11: One of the reason to choose Arctic soils that is mentioned is the "low temperatures at which its microbial community is constantly exposed", which "facilitates the possibility of observing strong responses at the extreme of the temperature range". I agree that one would expect a strong response of the microbial activity after step-increasing the temperature by ∼20 to °40C, but the reaction might be more related to stress physiology than an actual temperature response, especially during such a short treatment period (35 days). Therefore, I would not stress this point too much and briefly touch the issue with stress responses after drastic step-change in environmental factors in the discussion section. Additionally, I suggest to add another advantage of using Arctic soils for this incubation study: The large amounts of C stored in the Arctic region in combination with the fast warming (compared to the global average). Also moisture (and oxygen) is an issue in that region because of the impenetrable permafrost layer that is present under a large part of the surface. Further, it is important to restrain your conclusions to Arctic soils, as their dynamics might differ from soils from more moderate or tropical climates. For instance, it has recently been shown that the C balance of soils from Arctic and subarctic regions are more sensitive to warming. It might be that the influence of moisture and or oxygen (and especially the interactions) differ between climates (and probably soil types, but it would dilute the story too much to dig deeper into this). It would be very interesting to perform a similar study with soils from different climate regions.*

These are all good points. The reviewer is correct in that the high temperature treat-

ments we applied may induce physiological stress in microbes. The effect may be expressed as a short-term physiological response or as a long-term change in microbial communities (Schimel et al., 2007). Our $CO_2$ respiration measurements however, cannot distinguish between these two type of responses. Although our incubations were short (35 days), this is still enough time for the microbial community to shift. We may not be able to say anything here about the mechanistic response at the microbial level, but we observed an aggregate response that may combine both microbial physiology and community composition. At the level of abstraction we are focusing in this manuscript, this overall response is important for representing climate change effects in soil models.

We also agree with the reviewer in that it would be very interesting to replicate this experiment for soils from different ecosystems. We may be able to observe very different responses for tropical or temperate systems.

We added some text to the methods and discussion section addressing these points.

*p. 4, line 4-6 and 24-25: The fractionation of slow and fast cycling C pools (with different decomposition rates) is not well introduced. Add a paragraph in the introduction as rationale why it is interesting to separate into slow and fast cycling pools when investigating temperature, moisture and oxygen effects on decomposition rates. Also, expand the discussion on this subject.*
This is an important point that we did not address properly in our previous version. It is not only the interaction among multiple environmental factors, but also how they affect different rates. Our modeling approach includes both a fast and a slow pool that are modified by these different environmental factors. We included some text in the introduction, the model description, and the discussion addressing this topic.

*p. 4, line 4-6: Define T, W and O. I would also change W (Water) into M (Moisture), which fits better with the title.*
Definitions were added, and $W$ was changed for $M$ as suggested.



*p. 4, line 21 and 22: 35, 90, 20 and 25, 15, 1: Add units to the numbers.*
Done

*p. 6, line 3-4: "Decomposition rates were highly sensitive at a narrow part of the oxygen range, while for moisture this range was wider (Figure 4)." The oxygen range in this study covered the full range of oxygen that can be expected, from 1% (anoxic) to 20% (the maximum that can be expected; atmospheric O2 con- centration). The range with the highest sensitivity to oxygen in Figure 4 runs from 0 to 2.5%, which is about 12.5% of the range. Also for moisture a broad range is covered (15 to 90%). The range with the highest sensitivity to oxygen in Figure 4 runs from 0 to 10%, which is about 11% of the range (if the maximum is set to 90%). As there is little difference between 12.5 and 11% of the range, I do not understand the statement that the sensitivity to moisture occurred at a broader range. Can you explain this in more detail?*
Our previous description of the intrinsic sensitivities was ambiguous as noted by the reviewer. We re-wrote completely this paragraph in light of other reviewer's comments and the modification of Figure 4. We do not refer here about these ranges anymore, but mostly to the overall shapes of the dependence and sensitivity curves. We also put more emphasis on the predictions for the specific treatment levels and not so much on specific predictions outside the values for which we have no data.

*p. 8, Figure 4: It is strange that the strongest response for all three parameters occurs outside the treatment range of this study. Is it possible to extrapolate your findings that far?*
Figure 4 was modified to avoid emphasis on interpretations outside the ranges where we do not have data. The new version of Fig 4 gives more emphasis on the specific treatment levels where we have measurements, and we only provide model predictions outside these values as a reference for the functional response of the model.

*p. 1 line 7; p. 3 Lines 9; p. 9 line 8 Change "arctic" into "Arctic"*
As Reviewer 3 pointed out, the site is really a boreal and not an Arctic ecosystem. We changed arctic to boreal throughout the manuscript.

## References

Schimel, J., Balser, T. C., and Wallenstein, M. (2007). Microbial stress-response physiology and its implications for ecosystem function. *Ecology*, 88(6):1386–1394.

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