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

First, there is some confusion in describing the level off of decomposition rates at high temperatures. E.g. at P2 L6, enzyme denature should be described as irreversible enzyme denature, so one will not confuse it with reversible enzyme denature. As a mater of fact, the MMRT theory is largely based on reversible enzyme denature (though its authors did not say so), which was known as early as in the 1980s (Murphy et al., 1990: Common features of protein unfolding and dissolution of hydrophobic compounds, Sciences). The idea was then combined with the concept of a single rate-limiting "master

reaction" to model the respiration of bacteria by Ratkowsky et al. (2005: J of Theoretical Biology). A much earlier study by Sharpe and DeMichele (1977: J. Theoretical Biology) also derived a similar curve as MMRT, and was used in the model ECOSYS (Grant et al, 1993: Soil Biol. Biochem.) to simulate microbial decomposition. More recently, the same idea was applied in the model Tang and Riley (2015: Nature Climate Change). I think the authors of this study should report these developments so readers will have a more complete picture of this problem.

It is incorrect to say that the MMRT is based on reversible enzyme denaturation. The answer to this comment by Reviewer 1 clearly explains why this is not the case, and our explanation that MMRT describes the changes in activation energy with temperature is in fact correct. Furthermore, we believe that a discussion on the origins of one enzyme-level theory over another is well beyond the scope of this manuscript. Our measurements and level of abstraction are at the level of overall respiration fluxes and how are they influenced by temperature, moisture and oxygen. We only mention the MMRT and denaturation as a context for expected responses, but a detailed description of enzyme reaction theories would introduce a level of detail that would serve more as a distraction rather than a useful context for the present analysis.

Second, P2. L10-11, I think this criticism is not quite true. Authors who applied these concepts never said moisture should remain constant; rather they just focused on temperature, because temperature is considered as the most important factor. Moisture effect could be very well incorporated into those applications, which may be under way and ECOSYS has done this in the 1990s.

This is really not a criticism, but rather an important consideration when using these functions. Temperature effects on enzyme activity and decomposition rates operate under the assumption that all else remains equal except temperature. This is very important for developing and testing these functions, but in practical applications one must also consider that other environmental factors also change. This is the only point we wanted to make here.

Third, in describing the moisture effect, the authors missed the physiological effect that the moisture will impose on microbes as soil matric pressure becomes more negative. Such effect was shown to be important in Grant and Rochette (1994: Soil Sci. Soc. Am. J.), Manzoni et al. (2016: Soil Biology and Biochemistry) and Yan et al. (2016: Biogeochemistry).
We added a sentence addressing this physiological effect.

Fourth, in describing the incubation, the geometry of the incubated soil is not clear, e.g. what is the thickness of the cylindrical soil column? Such overall thickness will definitely affect the interpretation of the empirical data.
We included a description of the area, height, and volume of the soil columns as well as a calculation of the bulk density of the soils.

Finally, in describing the modeling approach, the authors did not lay out the hypotheses that lead to their model structure. For instance, under what conditions should this model structure be assumed applicable? Apparently, the model as proposed will only be useful for a soil column neither too shallow nor too deep. For a too shallow soil in natural environment, oxygenation will be very effective under the variable environment (through mechanisms such as wind pumping), so both the oxygen and moisture effect will be hard to discern from empirical data. For a too deep soil, difference in the vertical distribution of all decomposition variables will invalidate the homogenous assumption as built in the model. Also, the model assumes the microbial dynamics is totally slaved to the moisture and oxygen effects, so hysteretic behavior due to population dynamics as identified in Tang and Riley (2015) will be missing. The population dynamics may be very important in field conditions.
The scope of application of the parameterized model does not go beyond than that of the incubated soils. It is not our objective to propose a general model that can be applied to field conditions. We were only interested in testing a model that include the three main environmental factors (Temperature, Moisture, Oxygen) on a homogeneous organic soil consisting of two kinetic pools. We acknowledge that for predicting field

data a more complex model may be needed, which is expressed in the last paragraph of our discussion.

To make this point even more clear, we added a sentence in our model description section indicating that the objective of this model structure is only to explain our experimental data, but more complex models may be needed for other applications.

P3 L29-32: this could be summarized as parametric equifinality.
Yes, these are similar terms. We added the word 'equifinallity' in parenthesis so readers know that they are synonymous.

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