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

Anonymous Referee #4

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Sierra et al studied how the interactions among temperature, moisture and oxygen concentrations control the decomposition rates of soil organic matter using a combination of modeling and incubations. The study is of course important, however, the paper could be further improved if clarification is done for a few places that I will list point by point below.

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.

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.

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).

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.

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.

Other comments:

P3 L29-32: this could be summarized as parametric equifinality.

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