



EGUsphere, referee comment RC2  
<https://doi.org/10.5194/egusphere-2022-1091-RC2>, 2023  
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## **Comment on egusphere-2022-1091**

Anonymous Referee #2

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Referee comment on "Differential temperature sensitivity of intracellular metabolic processes and extracellular soil enzyme activities" by Adetunji Alex Adekanmbi et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-1091-RC2>, 2023

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I have read the manuscript titled "Differential Temperature Sensitivity of Intracellular and Extracellular Soil Enzyme Activities" by Adekanmbi et al.

The study has two main objectives. The first objective is to evaluate the thermal sensitivity of the extra- and intracellular steps of soil organic matter decomposition. The second objective is to evaluate the potential of microbial communities to acclimatize/adapt to a temperature treatment over 60 days.

The study is well written and the introduction and discussion sections are well-supported with relevant hypotheses and current literature. I find that the topic and questions raised in this article are of great interest, as there is still a lack of understanding about the thermal sensitivity of soil microorganisms, their potential to adapt to climate change, and the implications on soil carbon decomposition. The study is well-written and has a clear introduction and discussion with well-stated hypotheses and up-to-date bibliography. However, the study has three main limitations that I highlight below.

1- I understand the idea of removing substrate limitation by feeding microbes with glucose, but using this as a proxy for intracellular enzyme activity is confusing. Other factors, such as diffusion, active transport, and carbon use efficiency of the microbes, among others, can also impact this step. Additionally, comparing the intracellular decomposition process (which involves multiple enzymes) to an extracellular specific enzyme reaction (such as beta glucosidase or chitinase) seems not appropriate. The authors should rephrase this in their manuscript and consider discussing non-limited respiration or maybe glucose-induced respiration.

2- I do not understand why the authors are calculating Q<sub>10</sub> at different temperatures. It is known that one of the main limitations of Q<sub>10</sub> is that it can change depending on the temperature range chosen for calculation. Why did the authors not use linear regression between the natural logarithm of enzyme activity (V<sub>max</sub>) and temperature, and convert to Q<sub>10</sub> values based on the relationship:  $Q_{10} = \exp(10 \times \text{slope})$  (as cited in Zuo et al, 2021, German et al, 2016 and many other articles)? Can you please provide a strong rationale for why this method was not used or present a single Q<sub>10</sub> value calculated in

this manner.

## Refs

-The effect of soil depth on temperature sensitivity of extracellular enzyme activity decreased with elevation: Evidence from mountain grassland belts. 2021. Yiping Zuo, Hongjin Zhang, Jianping Li, Xiaodong Yao, Xinyue Chen, Hui Zeng, Wei Wang,

-The Michaelis-Menten kinetics of soil extracellular enzymes in response to temperature: A cross-latitudinal study. 2016. German, D.P., Marcelo, K.R.B., Stone, M.M., Allison, S.D.

3- This study only uses one soil and three temperature treatments to explore the relative thermal sensitivity of extra- and intracellular steps of decomposition. I acknowledge that determining thermal sensitivity in the laboratory is a lot of work, but using only one soil and three treatments is still very limited compared to other published studies. The authors should clearly state this limitation in the abstract and main conclusion to avoid extrapolating or overstating the main findings (which are indeed interesting).

Line to line comments:

Line 100: "measurement of enzyme activity at different temperatures in the lab is not an experimental treatment in itself (compared to the 60 day of temperature treatment). This sentence is misleading. The experiment did not have "60 experimental units," but 12 (3 incubation temperatures x 4 replicates).

Line 97: A space is needed between the two commas.

Line 128: What does MUB stand for? Was the buffer pre-incubated at different temperatures?

Line 326/327: "Accumulation of monomers" needs to be reformulated.

Line 343: "It is tempting" is not appropriate scientific language. Please rephrase.

Line 346: Please remove the hyphen in "trimeric."

Line 349: Please remove the comma.

Line 361: Double space?

Line 384: Changes in the thermal sensitivity of enzymes could have indicated an adaptation of the enzymes produced by the microbial community.

Line 386: Please use "Vmax" or "apparent Vmax" instead of "concentration," as you did not measure it. Please make sure to use consistent terminology throughout the manuscript.

In the supplementary material: Could you please specify if the curves on the graph are the mean of all samples or just one sample for illustrating the reaction? (Figure S-3:  $\beta$ -glucosidase).

Line 391: You could calculate enzyme-specific activity (normalized by microbial biomass) to test if this statement is correct or not?

Figure 2 caption: Please specify that it is Vmax.