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Biogeosciences Discussions

Interactive comment on "Temperature sensitivity of soil respiration is dependent on readily decomposable C substrate concentration" *by* A. A. Larionova et al.

Anonymous Referee #4

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I have read this manuscript with great interest, but it was extremely difficult to follow, mainly because of the very poor language. If the authors would be invited to submit a revised version, it is absolutely crucial that the manuscript is corrected by a native English speaker, because this level is not acceptable.

In the study, Larionova have performed some very nice experiments which provide insight in the response of soil respiration to the addition of labile substrate. These experiments surely deserve to be published. Unfortunately, this study was not well-designed to study temperature responses. What the authors should have done is expose the samples to short-term changes in temperature. In this case, the immediate

physiological respiratory response could have been detected. The comparison of the two temperature regimes is interesting, and perhaps more relevant than the short-term temperature response in itself, but it cannot be used as an estimate of the Q10. By incubating soil samples at two different temperatures for two months, one cannot distinguish the temperature sensitivity of soil repiration from other responses. Microbes could have adapted their physiology (acclimation), microbial community shifts might have occurred, Sn could have differed, and so on.

Also acclimation cannot be proven by the mere fact that a certain parameter was less than twice as high in the 12 degree treatment than in the 22 degree treatment.

To me, the current analysis is not acceptable for publication, but the data could surely be analyzed from another point of view.

What also intrigued me is the statement of the authors that at substrate concentrations lower than Km, km becomes insignificant again and thus the temperature sensitivity increases. This is mathematically incorrect, and further trivial, because as the substrate concentration becomes that small, the reaction rate also becomes so small that the temperature sensitivity hardly plays a role.

I was also unsure about the sample size. In the text it is stated that there were 3-4 replicates, but in the graphs n=5?

The authors compare a forest soil with an arable soil, and attribute the observed differences to the difference in the amount of C (depletion of SOM, p. 2014). These soils differ in many more aspects than just the organic matter content, so I would not assume that differences in their responses to the addition are related to the different C content.

In conclusion, I believe the authors should complete revise their manuscript, not focus on the temperature sensitivity, or acclimation, but on the responses to the glucose additions, and this under two temperature regimes, which then reflect an integration of all

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4, S792–S794, 2007

aspects of temperature reponses: the physiological temperature sensitivity, acclimation, adaptation, etc.

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