

Interactive comment on “Does the temperature sensitivity of decomposition vary with soil organic matter quality?” by M. Reichstein et al.

Anonymous Referee #3

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General comment This paper contributes to the discussion of the temperature sensitivity of SOM decomposition. It is part of a series together with the paper by Fang et al. and the reply by Knorr et al. The authors raise concerns about the analysis of data in an earlier publication by Knorr et al. and question the validity of their conclusions. The comments are useful and valid, and the overall quality is good. However, it offers little in terms of a strategy to move forward.

Specific comments The sensitivity of SOM to temperature, and in particular the sensitivity of different SOM pools, is a crucial issue in the ongoing debate of possible effects of climatic change on terrestrial ecosystems. Most current knowledge is drawn from results of soil incubation studies and respiration measurements, which can provide useful but only indirect information. Thus, an open discussion of the strengths and limitations of these approaches seems appropriate. The present paper raises con-

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cerns about the recent analysis of soil incubation data, and the conclusions drawn by Knorr et al. (2005) with respect to turnover times of different SOM pools. The authors stress the problem of data selection for the analysis, and the use of regression analysis to extract functional relationships. They propose alternative ways of data analysis yielding different results from those obtained by Knorr et al. These concerns are all legitimate, and the warning that firm conclusions drawn by Knorr et al. may be premature is valid; however, the paper is not fully convincing in providing a sound alternative to the analysis by Knorr et al. When the authors refer to 'a more appropriate way' of analysis (see Abstract), they refer to an alternative criterion for the selection of data for the statistical analysis. They claim that the selection should be based on the relationship between incubation time in the experiment and the turnover time of the material in question. Although theoretically this criterion seems to make sense, any experimental evidence to underpin the importance of this criterion is lacking. Obviously, by using different criteria for selecting data for the regression analysis will yield different relationships. The second point refers to the validity of statistical regressions in general. The authors are right in stressing that functional relationships cannot be extracted from statistical relationships, and they demonstrate rather nicely the problem related to parameter estimations from experimental data. Although the standard deviations used in their examples seem high, they have a valid point, but this is not really new information. The real problem is perhaps not the statistical analysis itself, but rather the availability of suitable data for this analysis, and the interpretation of the results. What would be needed is an approach that yields direct and more specific evidence of the temperature sensitivity of different SOM pools. The authors address the need for more appropriate studies, but again, I miss concrete ideas for how these studies should be designed. They point to problems arising from using either short or long incubation times, but what would be preferable, or what would be the alternative? Perhaps the authors could make some more specific propositions, instead of just raising questions. Overall, the present contribution is important in pointing out some limitations of the statistical analysis of experimental data to estimate the temperature sensitivity of different SOM pools,

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but the paper would gain from a more precise description of the data, which would be needed to overcome the present limitations, and their proper analysis.

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