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Interactive comment on "Is resistant soil organic matter more sensitive to temperature than the labile organic matter?" *by* C. Fang et al.

C. Fang et al.

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We are grateful to the two referees for their comments which have helped us to improve the clarity of our paper.

Referee #3 questioned the use of a fixed "A" in modelling. Based on our understanding of SOM decomposition, the contribution of different pools should include temperature-related and temperature-independent contributions. In an Arrenius type of model (as in Knorr et al.), all temperature-related contributions of a pool are included in a parameter referred to as activation energy, E, and all temperature-independent contributions are reflected by the reference decay rate, A. Each parameter may vary, or may not vary, with SOM pools. Combination of A and E will simulate different behaviour of SOM pools in different environmental conditions. Similar parameters are used in other type of models. At present, most model simulations suggest that the temperature-independent contribution is commonly greater than that of temperature-related one within a normal

range of temperature variation. If a fixed “A” is assumed for all pools, all contributions of a SOM pool can only be temperature-related, thereby fixing the conclusion of Knorr et al. that more stable SOM is more sensitive to temperature before the modelling even begins. It is not therefore an outcome of the study by Knorr et al., but an assumption inherent within it.

An analysis on the uncertainties of modelling in different ways is certainly helpful. We agree that model fitting does not confer certainty that the model is correct (indeed all implementations of the models have R2 greater than 0.9). We intend to show only that a model with variable A is able to fit the data equally well compared to a model with fixed A. Slightly different uncertainties would add little information in this context due to the nature of the fitting exercise. It is not our intention to suggest which model is correct or better than others, only that the models can produce equivalent fits despite different assumptions.

Our writing style was thought by referee #3 to be confusing or to lead a reader to conclude that we feel that labile and resistant pools behave the same way. At present, the assumption that the labile and resistant pools behave in a similar way simulates well most measured data from short term field observations and laboratory incubation experiments. However, if taking into account the complexity of SOM decomposition and difficulties and uncertainties in dividing SOM pools and lack of adequate experimental data, we cannot exclude the possibility that the resistant pool may be more temperature sensitive than the labile pool. We have reworded to make this clearer.

Referee #3 suggested that the continuous curve for the modelled value in Figure 1a is possibly misleading because it gives the impression of a time series. Currently, some studies incubate soil samples separately under different constant temperatures (discrete data) to estimate the response of a soil sample to changing temperature (time series). Modelled lines in Figure 1a are supposed to represent the time series of SOC under changing temperature. The drawback of this method was discussed in Fang et al. (2005).

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Referee #3 suggested that the citation of literature should be more informative. The article has been revised accordingly. Both referees pointed out that a broader literature coverage and discussion will be helpful. We agree with this. However, this short discussion is due to a concern of the implications of Knorr et al.'s conclusion. As we pointed out in our paper, we feel that the method used by Knorr et al. to derive their conclusion is unsafe, and other explanations are possible. It is not our intention to discuss whether the resistant pool is more or less temperature sensitive than the labile pool (the experimental data is lacking). A broad review and discussion on the relationship between SOM decomposition and temperature in relation to SOM quality / pools is of interest, but is not the focus of this paper.

Referee #4 suggested that our discussion of the possible cross over in decomposition rates of labile and resistant pools is highly academic, and that it is difficult for a general reader to follow the authors' argument. This is not the main point of our paper, but in this series of discussions, Knorr et al. suggest that by allowing A to vary, the decomposition rates of resistant and labile pools may cross over. Our short discussion attempts to point out that such a crossover is unlikely to happen, and could have a biological basis if it did occur. We agree that this is a fairly specialised point, but it is necessary to address the point raised by Knorr et al. in their related paper in this series. We have tried to explain it more clearly in the revised version.

Referee #4 stated that "What is puzzling is the fact that the authors' fit their model to their own data from an incubation experiment to underpin their argument, whilst questioning - a few lines later- the value of such an approach." We fit Knorr et al.'s model to both the data used by Knorr et al. and our own incubation data. What we then state is that model fitting cannot be used as the sole evidence to conclude whether resistant pool is more sensitive than the labile pool, if the fitting is not significantly better than the fit obtained with other forms of model (e.g. with variable A).

For modelling purposes, model parameters should be appropriately set up based on: a) a general knowledge about the process to be simulated; b) independent evidence

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(experimental data and maybe others); c) data fitting or other analysis indicating that such a choice of parameters is significantly better than others. We fit Knorr et al.'s model to data without any presumption about parameters A and E (or any assumption of A and E). Based on our understanding of the terms of "resistant" and "labile" pools and the relationship of these pools with measured decomposition rate, we argue that allowing A to vary is more appropriate than to pre-fix A for all pools. We do not imply that our fitting is correct or better than that of Knorr et al., only that a model with variable A is able to fit the data equally well compared to a model with fixed A. We have revised the article, according to referee's comments, to make this clear.

We entirely agree with referee #4's comments that most pressing problem in settling this debate is the lack of experimental data which can be used directly (or even indirectly) to evaluate the question of temperature sensitivity in relation to the quality of SOM pools. Knorr et al.'s conclusion is premature, not because of the simple model they used, but because of the inappropriate assumptions (a fixed value for A) when the model is set up. We have revised the paper accordingly to make this clear. To answer the question whether the resistant pool is more sensitive than the labile pool, further experimental studies are required. Discussion on "alternative ways forward to generate the necessary data" is beyond the scope of this short article.

Interactive comment on Biogeosciences Discussions, 2, 725, 2005.