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Biogeosciences Discussions

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

Anonymous Referee #4

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General comment This paper addresses some aspects of modelling SOM decomposition. It is part of a series together with the papers by Reichstein et al. and Knorr et al. The paper addresses an important scientific issue, and the authors raise some valid concerns with regard to the approach used in an earlier publication by Knorr et al. Overall, the quality of the paper is ok. However, the paper would benefit from a wider literature coverage, and a stronger argument to underpin the importance of 'stereochemical reasons' for using a variable reference decay rate.

Specific comments As stressed in relation to the paper by Reichstein et al., the discussion of strengths and weaknesses of different approaches to estimate the temperature sensitivity of different SOM pools is both timely and important. The present article tackles the issue from a modelling perspective, and the authors question the validity of some assumptions made in the Knorr et al. (2005) paper. The main argument put

forward by Fang et al. is that using a fixed value for A in the Arrhenius model is inappropriate because of different chemical properties of SOM pools, and that by fitting the model with variable A and E does not show that the resistant pool is more sensitive to temperature, as suggested by Knorr et al. This is a valid point, and the fact that their alternative, i.e. to use variable A and E for different pools yields a similar fit without invoking differences in temperature sensitivity, is important. However, the discussion of possible cross over in decomposition rates of labile and resistant pools is highly academic, and for a general reader, it is difficult to follow the authors' argument shere. 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. The second point refers to the procedure of fitting the model to available data to find specific temperature sensitivities of C pools. This argument in part 1 of the article is largely the same as stressed in the accompanying paper by Reichstein et al. Clearly, using different data sets yields different results. The main problem remains, i.e. the lack of appropriate experimental data to address the question more directly. The statement that 'we feel that it is more approapriate....'(pg 729, line 10) is not very convincing. In the second part of their article, Fang et al. point to other evidence for differential temperature sensitivities of stable and labile C pools. However, their literature review does not include any recent studies addressing the issue by using a more direct way. The authors should attempt to cover the available literature more broadly. They should also point out that the real problem is the lack of suitable experimental data to directly evaluate the question of temperature sensitivity in relation to the quality of SOM pools. Overall, the message perceived from this article is that the conclusion drawn by Knorr et al. (2005) may be premature because of the simplified model used, and that there is some evidence from other studies that different pools have the same sensitivity to temperature. But the latter discussion seems biased and the authors fail to offer alternative ways forward to generate the necessary data.

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

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Interactive Comment