



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

Anonymous Referee #3

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The authors of this paper are very well recognised in their field as providing high quality work on soil dynamics, and so I was disappointed with this study which seemed solely to discredit the work of Knorr. Whilst a debate in the literature like this can be helpful, the actual criticism itself needs to be well written and relatively easy to follow, including being understandable to those who are not soil physicists. For instance, it needs to be accessible by those who are developing Earth system models. So, overall, whilst the paper has an important message and should ultimately be published, it needs improved clarity.

The writing style which does not lead directly to the issues. In Section 1, there is a line "Fang et al concluded that the decomposition of resistant SOM pool is not less sensitive to temperature than the labile pool." But the abstract states "...does not unequivocally support Knorr's hypothesis of increased temperature sensitivity.." (with the implication

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that these authors are not entirely confident of the Knorr paper - ie the hypothesis of this paper). But when these statements are put together, they either contradict or a reader might conclude that these authors feel that basically the labile and resistant pools behave the same way.

The literature could be used in a more informative way. For instance, writing "relationship between the temperature sensitivity of decomposition and SOM pools is of paramount interest" (and then citing 7 papers) is not useful because the reader does not know whether this is interesting because of a) possibly implications for the general Earth System, or b) they all have different depictions of temperature response. Yet, at the same time, key literature is missing. For instance after the first sentence of Section 1 "feedbacks between SOC and climate change": there are no references (how about Cox et al 2000 or Jones et al 2004 - the latter explicitly considering different soil respirations models in the global context).

There is a technical issue that maybe I have misunderstood, but I would like the authors to consider. They argue that using a fixed "A" by Knorr is central to differences in arguments regarding sensitivity to temperature. My first interpretation of this is that the authors believe "A" should be a function of temperature. Surely even if there were different "A" values for the various pools, the actual sensitivity to temperature would be the same? (eg a temperature raise of 2C would give the same percentage increase whatever the constant values).

Much emphasis in the paper is placed on tuning parameters to make various conclusions. Sometimes with parameter-rich models and relatively low number of data-points it is easy to get very high R2 values. However, this does not always mean the parameters are well defined, and that they have little uncertainty. Statistical packages are now available that provide such uncertainty bounds. It would be useful to see such uncertainty bounds for the revisited Knorr analysis, and re-analysis here. The measure to which either a) uncertainty bounds on parameters or b) model emulations overlap provides a formal way of making statements as to which is the correct model (to a given

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level of uncertainty).

Is "resistant" the usual term (as a counterpart to "labile") - some use "stable" or "recalcitrant"?

In Figure 1a, the continuous curve is possibly misleading because it gives the impression of a timeseries (which it is not - the different circles relate to independent measurements for environments with various constant temperatures).

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

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2, S425–S427, 2005

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