

Interactive comment on “On the available evidence for the temperature dependence of soil organic carbon” by W. Knorr et al.

Anonymous Referee #3

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This paper is a defense of an original Knorr et al paper in Nature, against two new studies (proposed for this journal) by Fang and Reichstein. My immediate reaction was to question whether it is justified to write such a paper, when the best solution would surely be for this paper not to be published, and the work by Fang and by Reichstein to be "communications arising" in Nature, following the original Knorr work. However, since this is not the route taken, then the issues raised here are important. Understanding the climate sensitivity of soil respiration is of paramount importance in a warming world, and especially the issue here as to whether the non-labile pools might also feedback on the carbon cycle due to climate-carbon cycle feedbacks. This possibility is obviously of concern.

The abstract is long and difficult to read, and it is not entirely clear whether the authors agree with Fang or not. Obviously they disagree with Reichstein et al because of their

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data selection methods. This is a shame, because it masks three major issues of a) what data to use, b) what models to use and c) do the models actually disagree. That said, I found the end of the paper much clearer and very helpful. The very last sentence finally explains the abstract use of the word "possibility" (the authors might wish to make the abstract more direct like the conclusions).

The central scientific argument as to the correct model to use is set out around Equation (1). A frustration of these "linked" papers is that parameters used in one paper are defined in another, so they do not become "standalone" entities e.g. definition of variable "A". A technical concern is the "A more principled reason not to consider varying both A and E, however as it is possible for the different $k(T)$ curves to cross each other.....turning an initially more labile into a more stable pool at some cross-over temperature". This worries me, because it could actually be possible for an initial "more stable at low temperatures" soil pool at particular locations to become much less stable at "high temperatures". Indeed it is this exact eventuality that worries global climate modellers - there may be carbon pools in Northern Latitudes that are stable at present, but this could change "overtaking" rates at more temperature parts with different [A,E] values. Unless I've missed something, I do not see that the "curve-crossing" between different locations means a physical law has been broken? The authors should show this in a plot/schematic as it is so central to the argument against the Fang approach.

The authors might not wish to do this, but my recommendation to Biogeosciences is to accept this paper, the Fang paper and the Reichstein paper, but only as a single entity describing the unknowns. Would the authors be willing to pool their ideas, and where alternative models still explain the data, be honest - state this - and express it as a remaining uncertainty? The current three papers do not do justice to what is an extremely important and worrying possibility - that soil respiration acts as a positive feedback on human fossil-fuel burning.

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