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

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The three discussion papers in this issue (Knorr et al., Reichstein et al., Fang et al.) provide an exciting insight into the current discussion on the temperature dependence of SOM decomposition. Obviously, they are written largely from the viewpoint of applied modelling. Here, some remarks are added regarding two facets of CO2 efflux measurements from soils by incubation studies from the viewpoint of an experimental scientist. These facets are i) the quality of measurement, and ii) how appropriate are measurements of CO2 efflux from soil samples to understand the underlying processes of temperature dependency?

i) What do we measure? Most lab incubation studies try to separate temperature effects on different SOM "pools" by using incubation of various duration at different temperatures. Artefacts induced by either instantaneous measurement (probably no time

for micro organisms to adapt to temperature) or by long-term parallel incubations (pool sizes change over time at different rates depending on temperature, leading to misinterpretation of temperature effects) have been discussed already in the literature and will not be treated here. The focus of the first part of this comment is on more fundamental factors, namely ia) the effect of CO2 dissolution and dissociation in the soil solution, and ib) temperature optima.

ia) Amongst others, dissolution depends on temperature, and a higher proportion of CO2 goes into the soil solution (and is thus not being measured when considering only headspace CO2) at lower temperatures. Dissociation depends on both, temperature and pH, with higher pH values leading to a higher proportion of CO2 dissociated (and thus not being measured as CO2 in the headspace). Together, ignoring these processes may cause misinterpretation of the CO2 produced. Though in many cases the major part of CO2 will accumulate in the headspace, subtle differences due to CO2 in soil solution will systematically change the calculated temperature effect.

ib) With respect to temperature optima, it is likely that micro organisms adapt to the ambient temperature range of their habitat. Incubations should ideally not exceed the measured or estimated temperature range under field conditions, otherwise a possible failure of organisms to function at very high or low temperatures may cause misinterpretation of temperature responses if substrate quality is considered as the only variable. A potential indicator for a mismatch between the organism's adaptive capacity and applied temperature is an observed deviation of the fitted Arrhenius function to the measured values (i.e., the increase is not exponential). Measured values that deviate significantly from such a fit should not be considered for the interpretation of dependencies of temperature response from substrate quality. For example, in the study by Holland et al. (2000) that provides the experimental basis for the model described in Knorr et al. (2005) exponential functions are fitted to measured data. Even for their first step of incubation, where artefacts due to different substrate availability in parallel incubations should play no role (see also discussion in Holland et al. 1995), some

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of the sites clearly did not obey Arrhenius kinetics (Figs. 2, 3 in Holland et al. 2000). Temperature dependencies generalised by means of such fits will confound effects due to substrate quality and effects due to microbial temperature optima.

ii) How appropriate are measurements of CO2 efflux from soil and how to design experiments that specifically address temperature sensitivities of different “pools” of organic matter?

One known problem during the incubation of soil samples (and likewise for flux measurements in the field) is that mainly the labile OM is measured, and this effect is enforced for sieved samples because of aggregate disruption and subsequent release of CO2 from formerly protected OM. In studies addressing potential differences in temperature response for different types of OM, one attempt to circumvent the dominance of active OM is to incubate for longer time periods and thus to exhaust the labile fraction. However, in our own study (Leifeld and Fuhrer 2005) this approach did not reveal any trend in Q10 values either because there is no trend or because such an approach is not reliable. To obtain information on stabilised fractions, re-considering the mechanisms of OM stabilisation in soil is valuable. Recalling the comprehensive conceptual paper of Sollins et al. (1996), three classes of mechanisms can be distinguished: recalcitrance (i.e., substrate quality), interactions (i.e., physico-chemical controls via sorption, for example), and substrate accessibility (e.g., accessibility modified by aggregation). In a lab incubation of bulk soils, observed differences in temperature dependency of CO2 efflux cannot clearly be attributed to one of these stabilising factors. If we aim to separate these effects (I think we should because otherwise we oscillate in a model world with many unknowns), we need to investigate more strictly causal relationships. Regarding recalcitrance, such experiments could, for example, include measurement of temperature optima of well defined enzyme-substrate reactions (e.g., Wirth and Wolf, 1992) or degradation of model compounds (see e.g. Fierer et al. 2005, Fig. 2). Regarding abiotic controls, they could include comparisons of sterilised and biologically active samples (see e.g. Marschner and Bredow, 2002) or

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sorption-desorption experiments at different temperatures. Such experiments (there are not many of them yet) will, by broadening our mechanistic understanding, also help to improve modelling approaches.

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