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Interactive comment on "How well can we predict soil respiration with climate indicators, now and in the future?" *by* C. T. Berridge et al.

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The point is taken that the data analysed included an autotrophic component. However, the authors' strictures concerning modelling methodologies focus on the heterotrophic component (Rh).

The paper does mention that Rh is the product of a decay rate and a pool size, but unfortunately the implications of this fact were not fully taken on board. The key implication, which seems to have been missed, is that the primary control of Rh is NPP. This is because the pool size can (and therefore does) build up to the point where NPP and Rh are of similar magnitude. For this reason, the temperature dependence of Rh across different sites should reflect the temperature dependence of NPP, and not that of the decay rate.

In their reply, the authors focus on what happens when there is an imbalance between NPP and Rh (i.e. the system is away from equilibrium). They state that the standard model is "invalidated" in that case. But the standard model does not assume equilibrium. It simply allows a part of NPP to be transferred to SOM each year, and a fraction of SOM (the decay constant) to be removed. This set-up naturally results in a tendency towards equilibrium, but the model does not assume equilibrium and is certainly not invalidated by dynamic variations in the decay constant. This section also states: "The decay rate is of course the summation of myriad environmental factors assumed to be in equilibrium." I am not sure what this means. One specific model (RothC) is mentioned, but RothC does not work in the way the authors seem to think.

Much of the rest of the reply is irrelevant to my argument. The injunction against extrapolating statistical relationships is disingenuous, given that the paper expects readers to accept a statistical lack-of-relationship as argument against a modelling approach that is supported by a large body of experimental evidence.

Finally: I did not claim to "know offhand" that the observed 25% increase in soil respiration in experiments with enhanced CO2 is due to the increase in NPP. I do suggest, however, that this is a simple and plausible potential explanation that is worth considering, rather than rejecting in favour of one that is both unquantified, and vastly more complex.

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