

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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We would like to thank the commenter for his perspective on the manuscript: How well can we predict soil respiration from climate indicators, now and in the future?

An important clarification: the data presented in our paper pertains to total soil respiration, for all sites, not heterotrophic respiration, as was unfortunately garnered. This is noted on P1981 In 9: “. . . the average total soil respiration is used”. We will endeavor to clarify this more thoroughly throughout. Heterotrophic respiration data on this scale is desirable, but accurate separation and recording of heterotrophic and autotrophic components of total soil respiration in the field remains impractical without significant disturbance. We believe this revises the first criticism.

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It is explicitly acknowledged (P1985 In 9) that model calculation of heterotrophic respiration at any given site depends not only on the rate constant, but also the size of the respiring pool of organic matter. Indeed, we pursued this avenue (Supplement, Figure 3). Unfortunately, the soil respiration data used in the analysis are rarely accompanied by organic matter measurements. Since soil texture is related to porosity, soil water-holding capacity, gas diffusion, mineralogy and therefore long-term fertility, total carbon pools correlate with clay content (Kaye et al., 2002), where clays retain more SOM with slower turnover times (if all other factors are equal), and negatively with soil sand (Hook and Burke, 2000). We hoped thus to find the ratio of clay:silt:sand revealing. That this was not the case may be due to the resolution of the soil texture data used, or the time-span of field observations.

We believe the use of turnover time of carbon pools raises an open question. Generally speaking, pool turnover is defined as: $\text{inputs} - k \cdot \text{SOM}$, where k is the decay rate, assuming constant proportional mass loss over time. This gives a mean residence time of $1/k$ (turnover of an amount equivalent in size to the starting amount for first-order) assuming equilibrium (inputs = outputs). However, this is often used in models to predict the response to disequilibrium. If the mass loss is at any point is disproportional to inputs, this model is invalidated; a positive feedback between soil temperature and decomposition is a disproportional mass loss. The decay rate is of course the summation of myriad environmental factors assumed to be in equilibrium. When one of these changes (e.g. atmospheric CO₂), how does this affect the steady-state assumption of turnover time (also, see Figure 4)? Would the turnover time then have to change? If the decay rate is constant, and turnover time is related to decay rate as above, then of course it does not change in current model structure (e.g. RothC).

Turnover time can of course be calculated by the decay rate. The decay rate of soil, k , can be calculated from observations of carbon efflux and known mass of SOM (chamber measurements in the field, lab incubations). This is then related to the ambient temperature during decomposition (with considerable uncertainty: Subke and Bahn,

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2009), to define the temperature-dependent rate constant, which is then acted upon the carbon pool. The rate of change in respiration efflux with temperature is the slope of the fitted regression between soil temperature and soil carbon efflux (a convolution of multiple processes); an interpolation of observations defined by the boundary conditions of the experiment. Such a statistical model contains no mechanistic understanding of the processes involved (Smith & Smith, 2009), so there is no scientific ground to assume it will be accurate outside the conditions under which it was derived; which is exactly the purpose of predictive climate modelling (Collins et al., 2012). A quantitative analysis of the model fit to data can only confirm how well the model simulates the data to which it is compared; we cannot confirm that a model will work well in other situations, but only confirm that a model does not work within the limits of the data. If we were to compare model output to our data as suggested, it would of course be possible to reach a statistical agreement (by tuning the decay rate, turnover time or SOM pool), but this does not give us necessarily any reason to assume the same model will be able to predict the response of the soil system to disturbances. A process-based model that predicts current observations can be extrapolated to the future, given the limitations of the process understanding. This has led to the important observation of Sitch et al. (2008) that dynamic global vegetation models perform well under current climate conditions, but when forced with future scenarios start to diverge widely.

Another way of looking at this is to understand that model behaviour is controlled by internal parameters (e.g. k , Q_{10}) and external changes to the boundary conditions (e.g. CO_2 concentration, temperature). The model is then used to simulate the future by stipulating new boundary conditions, with the internal behaviour still controlled by parameters derived under the original boundary conditions. However, there is a growing body of field data observing changes in parameter behaviour when one or more boundary condition is altered (Jackson et al., 2009; Selsted et al., 2012; Smith et al., 2013); changes which are seldom additive, but more commonly antagonistic (Dieleman et al., 2012). This equates to a variable turnover time or rate constant. In practice then, a rate constant fitted to data on an annual timescale restrains century-long turnover of pools,

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to model daily efflux in model output under external conditions it cannot be expected to work in, or a turnover time is used to restrain the decay rate under non-steady state conditions.

We find it remarkable the commenter knows offhand that the (averaged) 25% increase in total soil respiration is due entirely to increased NPP, without deconvolving the response. While this statement may be valid for a specific model, in general there is as yet no empirical evidence to sustain this claim. If the increase is indeed due to increased root exudation, root respiration and root turnover, then the convolved response shows a large increase immediately after CO_2 enrichment; however, if the carbon is incorporated into slower pathways, the convoluted response is not immediately significant, but is instead apparent a few years after CO_2 enrichment. Without knowing the age of the respired carbon, it is not possible to know how much is the direct product of increased NPP, and how much is due to an increased stimulation of "old" soil carbon. Whilst the former will be balanced by increased NPP, the latter is a potential feedback that cannot be modelled using a decay rate and turnover time set under "pre-disturbance" conditions, or with the modelled changes in NPP. Whilst it is undoubtedly true that CO_2 fertilization can stimulate aboveground productivity, this can only explain the increase in total soil respiration through increased root respiration, not account for the magnitude to which increased litter input could stimulate microbial activity to decompose existing soil organic matter (Hoosbeek et al., 2004) or stabilize it through physical protection (Hoosbeek & Scarascia-Mugnozza, 2009) or alter the fungal community composition in a depth-dependent manner, such that a horizon-specific (not pool) change in the decay parameter would be needed (Weber et al., 2013). Resolving these relative contributions would be a significant improvement to the increase reported in this paper, but requires a significant effort in data collection.

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