

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 anonymous reviewer #1 for their insights and opinions on the manuscript: How well can we predict soil respiration from climate indicators, now and in the future?

It is unfortunate that our attempts to correct for soil carbon stock were again missed. We would like to point to P1985 Ln 9-17 and Supplement 3. Using the relative proportions of silt:sand:clay was unrevealing, but it now seems important to revise the manuscript and make this a central part of the main analysis, and possibly explore other proxies of soil organic matter for those sites where this is not reported alongside the respiration data (most of them).

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The reviewer was disappointed that the components of the flux were not separated between autotrophic and heterotrophic contributions; as were we. As discussed on P1987 Ln 19-26, this would be an ideal dataset. Unfortunately, this is still practically impossible in the field. One could assume a contribution to total soil carbon efflux from autotrophic inputs of, say, 40%, but the respiration dataset used in our analysis collates field observations over some 40 years, covering different soil strata, under different vegetation and with different climates. Separating the relative components after the event would necessitate a fairly arbitrary choice of relative contributions. Additionally, the relative contributions also change seasonally, and much of the annual data we have is inferred from discontinuous observations of total soil carbon efflux extrapolated to give an annual sum. Finally, field observations of total soil carbon efflux are used to parameterize the heterotrophic component of soil respiration (e.g. all the data used in Lloyd and Taylor, 1994), so it is actually comparable.

Regarding the similarity between this manuscript and the Bond-Lamberty and Thomson (2010) paper, which is considered 'nearly identical in premise', we openly admit that the raw data is similar. However, they present evidence for an increase in total global soil respiration over time due to increased temperature, whereas we use this data spatially (filtered to avoid pseudoreplication), in conjunction with two additional data sources, and as it relates to climate and the subsequent implications for large-scale modelling.

It is unfortunate that the reviewer believes we 'imply throughout the paper' that 'the theory of heterotrophic respiration depending on soil temperature is incorrect'. It is of course important to be explicit and clear with interpretations, not seek connotations or undertones. At no point do we say that heterotrophic decomposition is not sensitive to temperature (in fact, the contrary P1979, Ln 17-19). We accept that this is not clear enough in the introduction, and a revised version will elaborate on the form of the equation used and the pool structure of the major models. This will frame the analysis much better and lead to clearer conclusions. This manuscript considers the applicability of

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the coarse scale models used by the IPCC (Todd-Brown et al., 2012). Calculating the flux from decomposable pools in these models depends only on a temperature and moisture dependent rate constant. Since the temperature and soil moisture values used by that rate constant will in the future be driven by predicted climate variables (MAT and MAP), we look to see how well these currently accord with historic observations (quite well; generally within the 95% confidence interval). Additionally, it seems that what is quite defensible and readily apparent at the plot scale, is not as readily observed at the scale used by these models and this analysis (e.g. P1984 Ln 1-14). The manuscript goes on to examine the ways in which decomposition can be modified in the future (anthropogenic influences, feedbacks), leading to the conclusion that a temperature and moisture dependent rate constant seems unlikely to continue to predict respiration adequately, given the current and anticipated changes.

Reply to more detailed responses:

P1979 Ln 27-28: Thank you for pointing this out, we agree that this sentence can be phrased much better. The assertion is that soils already exist, and can therefore decompose. If aboveground productivity stopped, the soils wouldn't necessarily also stop decomposing. The subsequent point is confusing: it is not clear what the soil pools are being compared to in order to refer to them as being slow. Much of the annual flux of carbon from tropical sites is <2 years old, for example.

P1980 Ln 1: That they are dynamic is a mistake that has propagated through the literature for some time now then, e.g. Meersmans et al., (2012); Totsche et al., (2009).

P1985 Ln19: We don't understand your criticism: it is never purported that this manuscript parameterizes heterotrophic respiration. This line refers to the general parameterization of heterotrophic decomposition in models as elaborated in P1980, Ln7-21. An obvious improvement would be to include the general temperature and moisture dependent equations to which we often refer, and which are functionally identical in all the global climate models used in the latest IPCC report (Todd-Brown et al.,

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2012).

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