Biogeosciences Discuss., 11, C913–C915, 2014
www.biogeosciences-discuss.net/11/C913/2014/

Biogeosciences
Discussions
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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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Received and published: 9 April 2014

We would like to thank anonymous reviewer #2 for their general comments on: How well can we predict soil respiration from climate indicators, now and in the future?

Akin to our response to a previous commenter, we question the reliance on primary production modelling to properly account for the dynamics of the soil system. A significant portion of the observed increase in soil respiration likely results from changes in substrate availability (leaf litter) due to changes in plant productivity, as you suggest. But to assume this adequately accounts for the total change ignores potentially important feedbacks: litter input could stimulate microbial activity to decompose existing/old 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 decomposition is observed (Weber et al., 2013). All these examples will change the turnover time (and therefore the rate constant, as turnover is 1/k). This is not currently accounted for in models. Whilst these effects may be trivial in size on the plot scale (a FACE study, for example), CO2 is expected to increases globally (many FACE sites, for example), thus making the changes non-trivial.

Again akin to a previous commenter, it seems Supplementary figure 3 was overlooked: it may be fitting to make this a central part of our analysis. It is unclear what the reviewer meant by:

'. . .the dependence of the respiration flux on the availability of substrate has to be accounted for when the climatic controls are investigated. The authors do not account for this important factor in their analysis.'

The meaning of 'substrate' is ambiguous. If substrate is the contribution of recent aboveground inputs (leaf litter, root exudates), then climate very much accounts for this factor in total soil respiration (e.g. by separating the observations by biome and ecosystem). If substrate refers to the existing soil carbon pools, then please see Supplementary figure 3. Another simplifying step was taken in Figure 3 (main text) by only using mid-latitude sites, which excludes extremes in soil carbon pools found in high-latitudes and the tropics.

A minor clarification: We do actually state (P1985 ln 9) that the flux is calculated by pool size as acted upon by a rate constant that depends on temperature (and moisture). As this is how the flux is calculated, then flux is mathematically dependent on the temperature, ergo the flux is temperature dependent, which would be especially evident if pool size is the same.

We believe that this particular analysis drives home the complexity of the soil system as missed by climate models currently, and the problems this will engender in the future (Figure 4). There is obvious resistance to changing the representation of heterotrophic

soil carbon decomposition in global climate models (see earlier comment), despite the obvious and improvable failings. It is this particular speciousness we hope to mitigate with this paper.

References

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Interactive comment on Biogeosciences Discuss., 11, 1977, 2014.