Biogeosciences Discuss., 11, C32–C34, 2014
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Biogeosciences Discussions

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

Anonymous Referee #1

Received and published: 5 February 2014

The authors did an admirable job compiling a large meta-data set based on the Bond-Lamberty literature survey. They attempted to correlate site level soil respiration with temperature and precipitation data to motivate revisions of existing decomposition model structure. However I find their analysis unconvincing and, in many ways, a repeat of what has been previously published in the literature. While I am a big personal fan of meta-analysis and applaud the effort that went into curating this data set, I don't believe that the authors were able to meet the expectations set in their abstract.

This paper is nearly identical in premise to the original Bond-Lamberty and Thomson (2010) Nature paper. While the authors arrive at opposite conclusions they do not acknowledge this previous work nor postulate why their analysis is different from the original paper (though they do cite this manuscript as a source for their metadata).

I find their methods section insufficient. While I applaud the detailed collection and filtering of the meta-data I would have liked to also see a review of their statistical analysis prior to their results section. I remain unconvinced that a straight correlation between soil respiration, un-separated between heterotrophic and autotrophic and not corrected for soil carbon stock, is actually informative to global soil decomposition models. Furthermore I'm disappointed to see that they did not, in fact, analyze the soil decomposition rates or turnover times, as was implied by their introduction discussion of Earth system models.

The first several paragraphs of the results section belong in the methods and introduction. Anytime you start siting other works in your results it probably belongs elsewhere. Collapsing the results and discussion section in this case makes it difficult to read. Separating it would better illustrate what the authors found in their analysis.

I don't believe that statistically significant differences in MAT would necessarily result in significant differences in soil respiration but that does not mean that the (very well tested) theory of heterotrophic respiration depending on soil temperature is incorrect as the authors continue to imply throughout the paper.

More detailed responses:

P1979 ln 27-28: This is a meaningless sentence. Yes heterotrophic respiration will continue (unless we hit a Snowball Earth situation). What you mean to imply I think is that there will be a net flux of carbon out of the soil because the soil carbon pool is large. This is not necessarily the case however because increases in heterotrophic respiration could be balanced by increases in NPP. Since the turnover time of the soil pools is relatively slow it's not clear how soil pools will respond to changes in NPP.

P1980 ln 1: Furthermore while the annual flux of heterotrophic respiration is much larger then anthropogenic emissions it is not 'highly dynamic' compared to the size of the soil pool itself.

P1980 ln 15-20: While I personally agree with your assertion that the scaling of site specific models to the global scale is not well handled currently. I don't think this is an obvious conclusion that you can hand-wave here. A careful analysis of site derived variables aggregated over realistic spatial and temporal heterogeneity is needed to back up this claim.

P1985 ln19: This study does not parameterize heterotrophic respiration at all models! If you want to say something about the parameterization of decomposition models I believe you'd at least need to test a one pool model with some kind of established moisture dependency and temperature Q10 function. I would expect that you would find that temperature is, in fact, a strong driving variable in soil heterotrophic respiration.

The Litton etal 2010 had very similar soil carbon stocks at their study site which allowed for Rs$\sim$k. This is NOT the case globally so I would not expect to find a strong correlation between Rs and MAT.

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