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Interactive comment on "Temperature response functions introduce high uncertainty in modelled carbon stocks in cold temperature regimes" *by* H. Portner et al.

Anonymous Referee #2

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This manuscript describes an analysis of several models used to describe the temperature sensitivity of soil respiration, and it then links the uncertainty in those model fits to long-term storage of carbon in soils. The comparison among models for short-term CO2 flux rates is interesting, but I fund the link to long-term soil C stocks unsupportable.

First, there are some minor points of confusion regarding naming of soil C pools. In section 2.1.1, the authors refer to "2.85y, 33y, and 1000y, respectively", but it is not clear which pools are associated with these respective turnover times. The only list that I could find was at the beginning of that section, where the 3 pools are listed in the following order: "litter, slow SOM and fast SOM." However, that order doesn't make sense. I presume that the correct order is litter, fast SOM, and slow SOM, but that is also confusing, because the fast SOM should have turnover times of years, not decades.

At the end of section 2.2, the Century model pools are described as having turnover times "of 1-5y, 20-24y, and 200-1500y for litter, fast and slow SOM decomposition." This is incorrect for the Century model. Both litter layer pools and the fast pool in the mineral soil have turnover times on the order of years in the Century model, not decades. The slow pool has turnover times on the order of decades, not centuries or millennia. Only the passive pool has turnover times as long as 200-1500y.

This confusion about the Century model leads me to wonder if there is similar confusion about identifying pools in the LPJ-GUESS model in section 2.1.1.

More important than this confusion about pool names and their respective turnover times, is the more basic question of whether a model fit to short-term CO2 efflux rates, such as the fitting done in this manuscript with the database compiled by Hibbard et al. (2006), to address questions of long term soil C storage over decades and centuries. There are at least two problems with this approach. First, short-term CO2 efflux rates include the temperature sensitivity of root growth and root respiration over seasons as well as decomposition of SOM. The apparent temperature sensitivity across seasons may be inflated due to plant phenology compared to the actual temperature sensitivity of decomposition processes. Second, the long-term soil C storage is affected by stabilization processes, such as formation and destruction of soil aggregates and sorption and desorption of C substrates on mineral surfaces, but these are not captured in simple models of temperature sensitivity of respiration. The concept of a slow pool with a turnover time of several decades is useful, but the reaction constant (k) is not really the reaction rate of a single process, but rather an indicator of the net effect of several stabilization and destabilization processes and decomposition processes. The temperature sensitivities of these various processes probably cannot be inferred from fitting a model to contemporary measurements of soil CO2 efflux. On what basis do

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the authors think that this could be legitimate?

Assuming that the decomposition of the slow pool should respond to temperature in the same manner as contemporary CO2 efflux measurements suggests that the turnover times are simply functions of temperature in the same way that root respiration and decomposition of the fast pool are sensitive to temperature. However, the factors that stabilize soil C into the slow pool could be more or less sensitive to temperature. Moreover, the turnover times of these pools could vary with climate for a number of reasons, including direct responses of decomposition of SOM within aggregates to temperature, but also including formation and breakdown of aggregates and sorption and desorption processes. There is no discussion in this manuscript of the various processes that might affect turnover times in soils, and how those processes are affected by temperature.

The model assumes that 0.45% of litter inputs is transformed to the slow pool and 29.55% to the fast pool, apparently under all climate scenarios. Why would one assume that these transfer functions are constant with climate? The relative fractions that are decomposed within the litter layer and that are stabilized in mineral soils may also be temperature dependent, but this possibility does not seem to be considered.

In summary, I have no qualms with comparing the efficacy of various temperature models to see how well they simulate measured fluxes, but the conceptual link to simulating long-term soil C storage is not sufficiently well developed for the results to be useful.

Interactive comment on Biogeosciences Discuss., 6, 8129, 2009.