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Discussions

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

Anonymous Referee #1

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Review of BG-2009-181: Temperature response functions introduce high uncertainty in modelled carbon stocks in cold temperature regimes (H. Portner, H. Bugmann, and A. Wolf)

The manuscript by Portner et al. can be divided into two sections. In the first section, soil respiration data (previously assembled in a database by Hibbard et al.) from a range of temperate ecosystems are used to evaluate five response functions (including Q10, Lloyd and Taylor, Arrhenius. . .) for characterizing temperature-respiration relationships. In the second section, the different response functions are incorporated into the LPJ-GUESS model, and 1200-y simulations are carried out for an elevational

transect in the Swiss Alps, to investigate uncertainty in modeled soil C stocks under future climate.

The first section is quite similar in approach and objectives to a handful of papers that have been published over the last 5 or 6 years, including work by Janssens, Del Grosso, and Richardson; results of the present study should, I think, be put in the context of this earlier work.

Janssens, I.A. et al. 2003. Climatic influences on seasonal and spatial differences in soil CO2 efflux. In: Valentini, R. (Ed.), Fluxes of Carbon, Water and Energy of European Forests. Springer, Berlin, pp. 233–253.

Del Grosso, S.J. et al. 2005. Modeling soil CO2 emissions from ecosystems. Biogeochemistry 73: 71–91.

Richardson, A.D. et al. 2006. Comparing simple respiration models for eddy flux and dynamic chamber data. Ag & Forest Met 141: 219-234.

The second section does not seem to follow logically from the first, especially since there is no overlap between the sites with measurements and the site that is modeled.

The main conclusion (P8149 L7) seems to be that the use of the L&T temp response function in LPJ-GUESS cannot be improved upon.

Other comments.

1. The introduction highlights some of the challenges of modeling soil respiratory processes. On P8131 L15, it is stated that "a consensus has not yet emerged on the climate sensitivity of soil carbon decomposition", then on L23+, "decomposition of SOM is highly complex, as it is driven by a combination of factors". However, the authors then resort to evaluating simple, well-known models that effectively contain no pools, do not incorporate moisture (or other environmental driver) effects, lump together autotrophic and heterotrophic R, and are driven by a single soil temperature (of questionable representativeness). So the approach taken seems at odds with the motivation for the

study.

2. Re: P8132 L11. Since the soil R data are a combination of autotrophic and heterotrophic processes, it is not clear to me how the analysis performed really provides insight into heterotrophic respiration specifically; related to this, I find it very strange that later (P8135 L15+), that in the LPJ analysis, the L&T function is used for autotrophic respiration, but the five different candidate models are used for heterotrophic respiration.

3. The motivation for focusing on the Ticino catchment in the southern Alps is not clear, especially given that none of respiration datasets are from this region. Why not conduct the LPJ modeling for the eight sites used in the model selection part of the manuscript?

4. Sec. 2.1.2. It is not at all clear to me how the response functions were parameterized when included in the LPJ model. The section in the manuscript that appears to describe this (P8136 L22+) is quite cryptic and this needs to be improved. How were confidence intervals of turnover times estimated? I cannot find this in the manuscript.

5. P8136 L4+, L18. The way in which the parameter uncertainties were estimated needs to be documented (Monte Carlo methods or otherwise?). Furthermore, on L13 of this page it is reported that nonlinear OLS was used to fit model parameters, however, subsequently (P8145 L15) the increased scatter of model residuals at higher temperatures is mentioned; this indicates heteroscedasticity (non-constant error variance), which means that OLS assumptions are violated, and a weighted least squares approach should be used instead. Whether or not the error distribution is normal is not even discussed. Finally, on P8143, L18, there are comments about the need to consider parameter uncertainties (rather than an individual value), but it seems as if the authors treat the parameters as independent of one another (although on P8136 L19 the correlation matrix is mentioned).

6. The Gaussian and van't Hoff functions reach maxima before declining. This fine, but as in no instance are there data to constrain the declining portion of the curve (as

acknowledged on P8145 L10+), and so I find the decision to show the decline (i.e. Fig 1), or to draw any inferences from this (e.g. P8144 L25), surprising. Also, in light of this, I would be very hesitant about using these functions under climate change scenarios where the model is being used to make predictions well outside the domain used for parameterization. (Related to this: for at least one of the sites in Fig 1, it would be nice to see the confidence intervals on model predictions shown graphically).

7. Overall I find the discussion (which is repetitive and wandering) to be in need of reorganization and better editing.

8. The modeling is conducted over a narrow elevational range but then conclusions are drawn about warm vs. cold climates, high vs. low latitudes, etc (sec. 4.4). While I understand the need to present the results in a way that emphasizes their broad importance, I think this is a stretch, as there are many ways in which boreal/subarctic ecosystems are dissimilar from subalpine ecosystems.

9. It would have been nice to see the providers of the data (to the Hibbard database) acknowledged for their efforts.

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