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Interactive comment on "A parameterization of respiration in frozen soils based on substrate availability" *by* K. Schaefer and E. Jafarov

K. Schaefer and E. Jafarov

kevin.schaefer@nsidc.org

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This paper uses numerical modeling to estimate the relationship between the size of the liquid water fraction and soil temperature, and uses this relationship to predict the substrate diffusion limitation on heterotrophic respiration at freezing temperatures. The modeling provides a clear hypothesis for why apparent Q10 is high at sub-zero temperatures.

Though this topic has been a point of discussion for some time (Burt & Williams, 1976, Eberling et al. 2003, Monson et al. 2006), this paper applies recent data and an interesting application by connecting the frozen bgc model to SiBCASA and is therefore appropriate for publication in Biogeosciences.

My main concern with the paper is that it is not clear how the VWC data are used to model heterotrophic respiration. Respiration is calculated from a linear relationship with VWC (12042, line 4-5), but the agreement between observed and modeled respiration in Figure 6 suggests that respiration is more responsive to metabolic temperature effects than substrate limitation. Perhaps using an Arrhenius fit to this data that is moderated by VWC would provide a better fit to the data presented in Figures 5 and 6.

Response: We agree that accounting for the diffusion of Dissolved Organic Carbon (DOC) in the thin water films would provide such a VWC modulation and improve the match with observations between -5 and 0 °C. We clarified the text to state that there are two aspects to substrate availability: the amount of thawed organic matter and DOC diffusion (Line 54-5, 142-3). We now clearly state that the parameterization focuses on the amount of thawed organic matter (Line 144-5, 156). We expanded our discussion about why respiration responds in a non-linear way to VWC (Line 143-7). SiBCASA does not include a DOC pool or solute diffusion (Line 115-7, 144) and incorporating such processes into the model is beyond the scope of this paper (Line 458-9).

At any rate, I think it would be helpful to include a short discussion of how respiration is calculated by SiBCASA in the Methods section.

Response: We inserted a short description of the basic carbon pool prognostic equations in the methods section (Line 118-132). We refer readers to Schaefer et al. [2008] for a full description (Line 129).

More minor suggestions and questions:

In Mikan et al. 2002, Figures 1&2 show CO2 efflux vs. Temperature data that looks similar to the data presented in this paper (Figure 5), but when plotted as ln(CO2 efflux) vs. Temperature, you can see a clear change in the slope of the line below 0 degC. Perhaps a figure such as this would help to convince the reader that there is in fact a change in slope near 0deg C.

BGD

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Response: Converting to log scale does show a difference in slope, but the data and the model output is noisy and the change in slopes is not as clear as we had hoped. Consequently, we decided not to convert natural log axes in figures 5 and 6. We did change the wording from 'a much sharper decline' to 'a faster rate of decline below freezing' (Line 376).

How did you determine the values for phicrit and b (Table 1)?

Response: We inserted text stating that we calculated ïĄęcrit from the power law formulation and obtained the bi values from the literature (Line 183-5).

p. 12034, line 22 ": : :ratio of organic matter density to the density of pure organic matter" I know what you mean, but a little confusing.

Response: We rewrote the statement, which, upon reflection, was indeed unclear (Line 225-6).

Interactive comment on Biogeosciences Discuss., 12, 12027, 2015.