

Response to reviewers: A parameterization of respiration in frozen soils based on substrate availability

Below is our response to reviewer comments on our manuscript, ‘A parameterization of respiration in frozen soils based on substrate availability.’ We highlighted in yellow the specific changes to our resubmitted manuscript. We also made a number of corrections to the references as requested by the editor.

Reviewer 1

This manuscript presents an interesting application of a solute diffusion model within a soil carbon dynamics model to account for the effects of changing liquid water content as frozen soils thaw. I have two main concerns with this manuscript:

First, the same approach was recently published by Tucker (Soil Biology & Biochemistry 78 [2014] 90-96), but this work is not cited and Tucker is not given credit for having developed this idea. Indeed, Fig 2 of the present manuscript is nearly identical in form to Fig. 1 of the Tucker paper. Tucker used data from non-arctic areas, but the issue of freeze-thaw is still applicable. Tucker modified the Dual Arrhenius Michaelis-Menten (DAMM) model, which simulates diffusion of soluble C substrates in soil water films, and he showed how this diffusion is slowed drastically when the water is mostly as ice rather than in a liquid phase. He also included the effect of swelling ice occupying more pore space than liquid water, thus also limiting diffusion of O₂ into the soil. He demonstrated that the very large Q₁₀ values for soil respiration commonly observed across the small temperature increment between frozen and unfrozen soils is attributable to this diffusion effect rather than an actual high temperature dependence of the enzymatic activity. The present manuscript should cite the Tucker paper and the related DAMM papers as the source of this innovation.

Response: We incorporated references to *Tucker* [2014] (Line 94-6, 446, 453-5). We thank the reviewer for bringing this paper to our attention. We based our modeled liquid water fraction (ϕ_l) on the *Nicolson et al.* [2009] formulation while *Tucker* [2014] used the *Romanovsky and Osterkamp* [2000] formulation. Both are derived from the original power law formulation of *Lovell* [1957], so we would expect strong similarity between Figure 2 in our manuscript and Figure 1 in *Tucker* [2014]. We used a very different approach from *Tucker* [2014] and the DAMM model, since SiBCASA does not include Dissolved Organic carbon (DOC), solute diffusion, and oxygen diffusion (Line 115-7, 144). However, we give due credit to *Tucker* [2014] for linking liquid water fraction to simulated respiration (Line 94-6).

Second, I don't understand the discussion about the “original Q_{10f}” formulation. The authors don't make it clear what their original formulation was. Is it simply a constant Q₁₀ across all temperatures? If so, what value of Q₁₀ was used? Or was the original formulation one in which a very high Q₁₀ was applied across the freeze-thaw temperature increment and more normal Q₁₀s were applied above and below? I suspect that my lack of understanding of this might contribute to my sense that the authors' conclusion about long-term versus short-term effects is counter-intuitive. It would seem to me that it is the short-term respiration response that would not be adequately simulated by the conventional Q₁₀ model when soil temperature changes from -2C to +1C. For this short-term response across this small temperature range, the diffusional effect needs to be used to skillfully simulate the observed pulse in soil respiration. It also seems to me

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that the longer-term effect of a change of MAT from slightly below 0C to slightly above 0C could be simulated by the traditional Q10 approach. However, the authors have reached the opposite conclusion. I'm obviously missing something, but I believe that their explanation is inadequate.

Response: We clarified the text to state SiBCASA used a Q_{10} of 1.5 and a Q_{10f} of 200 (Line 129-32). In SiBCASA, Q_{10} formulation applies to all temperatures and the Q_{10f} formulation applied to $T < 0$ °C (Line 129-32). We clarified the text to state that 'long term' in this context is 500-10,000 years for temperatures below -1 °C, where the Q_{10f} formulation depletes the frozen carbon (Line 89-91, 476). We already state that the Q_{10f} formulation does a pretty good job for shorter time scales of 10 years or less (Line 472-3). We also clarified the text to state that there are two aspects of substrate availability: the amount of thawed organic matter and DOC diffusion in the thin water films (Line 54-5, 142-3). SiBCASA does not include a DOC pool and solute diffusion processes (Line 115-7, 144) so this parameterization focuses on the amount of thawed organic matter (Line 144-5, 156). We also clearly state that the new parameterization completely replaced the original Q_{10f} formulation (Line 147-8).

Although this work is not entirely novel, because Tucker already applied this approach, the work is still worthy of publication because it is being implemented in a larger model that has broad applications to the fate of carbon in areas of permafrost. As long as Tucker is acknowledged (BTW, this reviewer is not Tucker) and the explanation of short-term versus long-term effects is better explained, I believe that this work could be suitable for publication.

Response: We referenced Tucker [2014] (Line 94-6, 446, 453-5) and we better explained what we mean by 'long-term' and 'short term' (Line 89-91, 476)

Reviewer 2

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

Response to reviewers: A parameterization of respiration in frozen soils based on substrate availability

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.

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 ϕ_{crit} and b (Table 1)?

Response: We inserted text stating that we calculated ϕ_{crit} from the power law formulation and obtained the b_i 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).