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

Anonymous Referee #1

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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 to 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.

Second, I don't understand the discussion about the "original Q10f" 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 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.

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.

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