The Cryosphere Discuss., 9, C1211–C1214, 2015
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The Cryosphere Discussions
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Interactive comment on "The importance of a surface organic layer in simulating permafrost thermal and carbon dynamics" *by* E. Jafarov and K. Schaefer

Anonymous Referee #2

Received and published: 14 July 2015

Overall, this is an interesting paper that describes an important process development in a model that has been used to examine permafrost C feedbacks. I would expect that a revised version will be suitable for publication in The Cryosphere. My overall comments are that certain process descriptions could be clearer, and the authors should try harder to compare more aspects of the model to observations. For example, since this paper is describing a way of distributing C vertically through the soil column, there should be some analysis of the model predictions of these vertical processes and comparisons against observations. I also think greater efforts need to be made of avoiding circularity in figure 6.

Specific coments:

p. 3138, l. 13: Better than what? Need to be more precise here about what you are treating as your reference case.

p. 3138, l. 19: Hugelius et al., 2014 state the range of C is 1100-1500 Pg, with a best estimate of 1300Pg. That paper is an update of the Tarnocai database, so the newer estimate should be used.

p. 3139, l. 7-12. Not sure the phrae "typically assumed a spatially uniform permafrost carbon density" is an accurate characterization of all of those papers. And also not sure if it si necessary for your argument; the point is that frozen volume matters, irrespective of what the soil carbon content is or how it is calculated.

p. 3139, l. 18. More accurate to say "the same thermal biases that lead to deeper modeled active layers also lead to warmer soil temperatures."

p. 3141, l. 11. This isn't the correct reference for the CRUNCEP dataset. Better to put a link to the Viovy website as is typically done.

p. 3141, l. 20. Is this relevant? Can't the model read inputs from restart files anyway?

p. 3141, l. 22. The citation should be moved to the end of the sentence and the name of the RCN is either the Permafrost Carbon Research Coordination Network, or the Permafrost Carbon Network.

p. 3142, l. 19. I think you are missing a sentence here to transition from how you set the total C stocks to how you set the partitioning among pools. Also what are nominal turnover times of these pools at some reference temperature?

p. 3143, l. 10. I think the right reference for this is Koven et al., (2009); also note that that model does not include sedimentation processes.

p. 3143, l. 21-23. How do these assumed C densitiies compare with observations, such as the vertical profiles shown in Harden et al., 2012? It would seem well, given that obserevd C densities top out at about 60-80 Kg C / m3 for all three permafrost soil

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types, so maybe useful to mention that as a check on the parameters used here.

p. 3144, l. 5. I'm not sure I understand the purpose of OLTmax. In the peatland case, OLT can be several meters.

p. 3144, l. 10-19. Useful to discuss that functional roles the prognostic roots play in the model behavior. Do they control productivity directly, or are they jsut used to track C stocks into the ground? Also, what is the basis for linking leaf growth to root growth, as in line 22?

p. 3145, l. 3. Need to explain how Fice works. Is this just a step function of one below freezing and zero above, or is it more complex?

p. 3145, l. 6-10. This looks self-contradictory. Either you limit roots to unfrozen layers, or you use an exponential profile, which necessarily continues to have roots (perhaps small, but nonzero) in the frozen layers. Need to explain this better.

p. 3145, l. 10. If Fice is not a step function, then there will still be some root growth in partially-frozen soils?

p. 3146, l. 18. Some discussion of how thermal dynamics were calculated in the old version is needed, since that is being used as a reference case. Was soil organic matter included in the thermal calcualtions at all, or if so, how did it differ from the current version? More generally, is the comparison against the old model the right comparison? Maybe it would be more informative to use just the new version, but turn off various processes to understand their relevance?

I don't see any information about vertical C profiles in the results, which would seem like a crucial analysis to assess the approach presented here. What does a typical profile look like? How do overall C profiles compare against datasets such as Harden et al., 2012?

FIgure 4: This should compare predicted GPP to a reference dataset such as that of Beer et al. to assess whether the GPP changes imporve the model relative to

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observations?

Figure 5: Also show the final version, as in fig. 6 here, to note the effect of using variable C density.

Figure 6, and also discussion points in the text about comparison of low C bias relative to observations in SE Canada and SW Siberia (p. 3148, l. 28, p. 3149, l. 12-27). The reason why these soils have such high C is that they are vast peatland complexes. I think you should clarify the ways in which the model does and does not include peat-like behavior. I.e. the accumulation of organic rich surface layers does seem like peat-like behavior. But only if there are feedbacks between soil saturation, C accumulation, and soil physical properties, as in peats. So the question to pose is: should the model capture the vast peatland complexes in SW Siberia and SE Canada, or not? Getting this right would require both having the right processes in the model, as well as having the right distribuition of saturated soils.

Figure 6. Given that you start with the NCSCD data in the permafrost layers, this isn't strictly a valid comparison, as there is a clear circularity in comparing input data against reference data. So it would be more appropriate to restrict the comparison to only the active layer C stocks to avoid this. You could sample the NCSCD only to the thaw depth predicted by SiBCASA, and then compare to only the active layer C stocks in SibCASA, to make such a comparison.

p. 3150, l. 29 - p. 3151, l. 6. This is speculation. There are many reasons why a model may overestimate or underestimate permafrost area, e.g soil processes, snow processes, albedo, etc. So it is not correct to infer that actual permafrost area is being lost just because a model does not simulate permafrost in a given area.

Interactive comment on The Cryosphere Discuss., 9, 3137, 2015.