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The Cryosphere Discussions

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 #1

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1 Main points:

The authors describe model developments made in the SiBCASA model (a land surface model). This includes an improvement to the dynamics of organic matter in the soil, restrictions of root growth, leaf growth and GPP based on soil freezing, and a new intialization procedure to include the observed permafrost carbon distribution in the model. They then discuss and show the improvements to the model performance and the differences compared with the old model version. Finally there is some discussion of which factors control the simulated permafrost distribution.

These model developments are important and this work makes a significant contribution to permafrost modelling. In general the paper is quite clearly written. I therefore recommend that this work is worthy of publication in The Cryosphere, but there are currently some weaknesses that need to be addressed.

The main weakness is that the analysis of the results is lacking. Firstly, the demonstration of changes to the simulation and how these result from the model developments requires more detail. Secondly the part where permafrost dynamics are attributed to meteorological drivers is not scientifically rigorous. The other sections would also benefit from some improvement and my suggestions are found in the comments below.

2 Detailed comments:

Abstract - This should summarise all the results, so add a sentence about the impact of the root developments as well as the dynamic SOL. Also it would be good to add that initialising with observed SOC gives a better SOC distribution in the simulation than the previous method, just to make this clear.

Introduction -
Page 3139 Line 5: Please define what you mean by active layer thickness.

Lines 7-8: In fact, Burke et al (2013) does not use a constant carbon density but uses the observations from NCSCD.

Page 3140, Line 5: "Here we describe a fully dynamic SOL to demonstrate the importance of coupling soil biogeochemistry and thermodynamics": In the analysis later you demonstrate the importance of having a soil organic layer of realistic thickness but you do not demonstrate the importance of making it dynamic. I would not suggest that you change this statement but rather that you should do some more analysis/discussion (see later on).

Methods -
Is the SiBCASA model used in any coupled GCM/earth system model? This would be good to mention if so.

Section 2.1 (Frozen carbon initialization)

It is not clear how you intialise the permafrost carbon - is it at the beginning of the 900 year equilibrium run? If so how do you determine the maximum active layer thickness? Or is it at the end of the 900 year equilibrium run, so your equilibrium run is performed with no permafrost carbon present? Or do you do two equilibrium runs: one without permafrost carbon and a second one when you have initialised it based on the active layer in the previous run? Please describe this procedure more clearly.

Page 3142, line 18: These equations appear unexpectedly with nothing leading up to them to say what they are. I suggest to add a sentence here along the lines of 'The carbon in each layer is divided into three pools as follows:' and then give a definition of the pools.

Section 2.2 (Dynamic SOL)

"SiBCASA already accounted for the effects of organic matter on soil properties like porosity, ...". Please give more details of this! It is important for understanding the work. For example, did the properties vary depending how 'compressed' the organic matter was? Do they vary with depth assuming the organic matter is more compressed at depth? What properties are used and how are they combined with mineral soil properties?

It is unclear how the carbon was dealt with in the previous version. The implication is that all carbon entered (and stayed in) the top soil layer and there was none in any deeper layers? Is that true? Please clarify in the text. How did that allow the model to include permafrost carbon (with constant density), as you have mentioned and compared with (Schaefer et al. 2011)?

In equation (4) you multiply C_{max} by f_c. This seems strange given that there was already a factor of f_c in the definition of C_{max} in equation (3). Is there a mistake here? When you say 'C_{max} is 140 kgm^{-3}', perhaps you mean 'ρ_{max}' here? Please check this.

What is the purpose of defining OLT$_{max}$? (Equation 5)

Section 2.3 (Root growth and soil thermal factor)

I am slightly confused as to how your root growth works. Is the root profile prescribed as exponential? If so, what difference does it make if the roots are only growing in the thawed layers? Does this affect the input of carbon to the different soil layers (as well as the autotropic respiration)? That part was not clear in the text.

Results -
Page 3147, line 13-14 "so the effect is not as pronounced" - what effect are you talking about here? Not clear.

Line 20-21: Slightly confusing to say "a strong peak" when refering to the first plot, since the peak is stronger in the second plot. Probably better to just say "a peak".

Line 26: This paragraph is not very clear. I'm not sure what you mean by 'coupling'. It would be better explained along the lines of... 'In the version without SOL the ALT was generally deep in forest biomes, but in the new version there is a thick SOL (due to high GPP), which leads to a much shallower ALT. This means there is now a significant amount of root growth in the permafrost itself, which puts carbon directly into the permafrost stores. This is unrealistic, and the frozen soil restrictions on GPP and root growth together eliminate this problem.' (maybe you could write it better but that is the general idea.)

It is misleading to suggest that 'coupling between GPP and ALT' does not reflect reality, when in fact there is a real coupling between these quantities. Here it is a negative feedback: Increased GPP → increased litter → increased SOL → reduced thawing → reduced GPP. There is also, however, a positive feedback on soil organic carbon as more soil carbon → increased SOL → reduced temperature → reduced respiration → more soil carbon. These feedbacks are potentially important and it is great that your model now includes them, but there is no analysis of these feedback processes, which is a shame. Please cite Koven et al (2009) (which appears in your reference list but not in the manuscript), and include some analysis to demonstrate the coupling in your model. This would significantly improve the worth of the research.

Page 3148, line 14 onwards. You are discussing the total amount of soil carbon in the simulations. Here it would be good to compare this with the observed total from NCSCD (as well as spatial distribution in Fig. 6).

Page 3149, line 1 (and end of prev. page): "overestimation of the SOC in Central Siberia occurs due to high SOC at the initial time step" - what do you mean by initial timestep? How could that happen? I think if you explained the spinup procedure more clearly in the methods this would be easier to understand.

Page 3149, line 3: How can the ALT be identical when the soil carbon amounts are different? I thought that the physical soil properties depended on the amount of carbon? Again perhaps if you had explained in more detail the changes to soil properties in section 2.2 it would be clearer how this could be the case. (See my comments for 'Methods' section, above.)

Discussion -
In the second paragraph of the discussion you are talking about Figure 7, which shows the spatial patterns of various physical forcings (air temperature, downward longwave radiation, snow depth, and soil wetness factor). Many claims are made about which factors are contributing to the permafrost distribution and in what way. This analysis is not rigorous and must be improved before the work can be published. For example, "maximum snow depth in South-East Canada is almost half that of West Siberia, which suggests that snow in SE Canada, most likely, is not a major contributor to warm ground temperatures" - well what about the fact that there is a 'critical' snow depth below which

the soil is much more sensitive to changes in snow depth (see eg. Ekici et al. 2014a): shallower snow does not necessarily imply that it has much less effect. An actual relationship between two variables can only be asserted based on analysis such as a regression or spatial correlations. If you would like to say which factors have the most influence, either comparing the spatial correlations of the forcing variables with ALT, or perhaps performing a multiple regression of all the variables against ALT, would give you much more definite claims. I suggest you replace this paragraph by a more rigorous analysis of the influencing factors.

Similarly, the final paragraph mentions that the CRUNCEP data may not have the zero degree isotherm in the right place. What makes you think that? Why might it not be a problem with the longwave radiation, for example? Could you perhaps find some in-situ air temperature measurements to support that claim?

Conclusions -
Again you claim that the dynamics of the SOL are crucial. In fact, as far as I can tell it is the presence of the (right kind of thickness) SOL that is important. For example, this could be achieved with a static method based on soil carbon observations (such as in Chadburn et al. (2015)). You should show in the paper why your method is better or at least discuss the findings of Koven et al. (2009), which showed the impact of the dynamic coupling between soil carbon and soil properties.

Lines 9-10. I'm not sure what this sentence means. Particularly, how did the change to plant root growth improve the ALT? And how did it improve the soil carbon? Was this because the carbon was no longer input to the permafrost? I think this needs to be clarified here or better explained in the analysis.

Line 14-15: "The initialised soil carbon respired during spinup due to abundance of permafrost within the top 3m." This does not make sense physically. Please check.

Figures -
Figure 7, please define 'water stress factor'. Also, do you really mean 'non-

dimensionless'? I guess it should just be 'dimensionless'?

A final suggestion I have which is currently missing from the paper: Some observed data to support the changes in GPP that have resulted from your changes to the model (eg. those shown in Figure 4). Does simulated GPP improve, and if not, why not - or how bad is it?

3 Technical:

Page 3138, line 17: "with impacts on ecosystems, infrastructure, and emissions to amplify climate warming" → "with impacts on ecosystems, infrastructure, and emissions which can amplify climate warming"

Page 3139 Line 19: Typo, 'Intercomparision' → 'intercomparison'.

Page 3140 Line 5-6: "... to demonstrate the importance of coupling bigeochemistry and thermodynamics to improve the simulated permafrost ..."→ "... to demonstrate the importance of coupling bigeochemistry and thermodynamics for improving the simulated permafrost ..."

Line 10: "...carbon dynamics in comparison."→ "...carbon dynamics in comparison to the previous model version."

Page 3149, Line 21: "for two months - September and October - over 10 years"

Page 3150 Line 5. 'suggest' → 'suggests'

Line 10: 'funding' → 'findings'

Figure 5: "(b) from the current run, correspondingly." would read better as "(b) in this study."

Figure 6: Again remove the word "correspondingly"

Figure 7: Caption says 'September and November'. I presume this should be 'September and October' as in the text.

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