

Interactive comment on “Coupling of the VAMPER permafrost model within the earth system model iLOVECLIM (version 1.0): description and validation” by D. Kitover et al.

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Response to Interactive Comment by C. Avis

The authors thank the referee for providing a thorough review of the manuscript. His ideas and corrections will surely improve the clarity and message of the work.

General Comments

1. One area that I found that the paper was a little lacking was that there was not much comparison or discussion of how the model stacks up against other coupled permafrost models in terms of the level of complexity of the processes represented and how well

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the model captures the observed distribution of permafrost. The validation comparisons for the coupled model are compatible with how other coupled climate/permafrost models have been evaluated in the literature and so it should be fairly straightforward to compare the results of this model's validation against the work done by other permafrost modeling groups and I think would strengthen the paper.

Response: We also agree that the paper would benefit from some comparison with similar studies. Although there are not many, we will discuss comparison with a recent larger project from Koven et al. 2012, which compared Earth System models ability to reconstruct permafrost.

Change: We propose to compare our results of modeling present permafrost distribution to the work of Koven et al. 2012. This is done at the end of section 3.2.1.

2. A second area that I found a little lacking was that there is not much description of the hydrology of the model (or lack thereof). From a read through of the paper, it seems that the model is solely tracking heat flow through the subsurface layers and no fluxes of water are explicitly described in the subsurface layers. The authors suggest that they account for some of the influence of water and ice in the ground (e.g. p. 7989, L19 “deep 1-D heat conduction model with phase change capability”,) but a little more detail as to how this is handled is appreciated. I.e. is it assumed that the ground layers are saturated at all times with moisture in order to perform these calculations? If hydrology is not explicitly handled in the model, the authors might want to comment about how this omission may impact their results.

Response: The authors agree that hydrology is an important factor in permafrost modeling and was largely skipped in terms of a discussion piece. The VAMPER model does not explicitly handle any hydrology other than assuming a saturated subsurface. On the other hand ECBilt integrates a simplified surface hydrology and a bucket model but as of now, these elements are not coupled to VAMPER. It is indeed likely that the results are impacted by a lack of coupled hydrology between VAMPER and ECBilt.

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Change: We propose to acknowledge the state of hydrological modeling when appropriate throughout the manuscript, including as the reviewer suggests 1) more detail on the hydrology in the VAMPER model description (section 2.1.1), its presence in ECBilt (section 2.2.2) and, impact of no coupled hydrology between VAMPER and ECBilt on the results (section 3.2.1, 4th paragraph).

3. Finally, I think there are a few other climate metrics that could be included in the paper. The authors' inclusion of a reasonably sophisticated snowpack scheme into the VAMPERS model produces fairly substantially different results compared against the model being run without a snow component. Yet, there is no discussion of how well the model represents the timing, extent and thickness of snow cover. The authors might consider comparing their model's snow cover output against observation or reanalysis based datasets and discussing how well snow cover is captured, especially if this is indeed a major determinant of permafrost characteristics.

Response: Modeling the snowpack against observations was previously validated while in development. However, these validations required specific site parameterizations when compared with observations. For a global earth system model of coarse resolution, this kind of observation validation is simply not possible. What would be parameterized for a very dense, wet snowpack near Anchorage Alaska would not apply for a dry thin snowpack on the windy plains of Siberia. This applies likewise for the characteristics of the snowpack along with the snowmelt model. Further, it should also be acknowledged that the role of VAMPERS in terms of adding a snowpack was to simply prescribe general snowpack characteristics so that we can simulate the effect of the snowpack offset between the air temperature and ground temperature, essentially adding some layers to the 1-d heat conduction model. The actual snowpack melting, timing, runoff, etc. is performed within ECBilt and description of this model is already published in Goosse et al. (2010) and Opsteegh et al. (1998).

Change: We propose to add as a supplement some figures showing the ability of the parameterized, site-specific version of VAMPERS to reproduce the snowpack as

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compared to observations. The provided graphs were made for two sites: Crescent Lake, Minnesota and Slate Creek, Alaska. These sites were selected because they had all the available data: daily air temperature, daily soil temperature, and daily snowfall in meters of snow water equivalent.

4. Similarly, the authors suggest that ECBilt does a good job of simulating surface air temperatures save for a few noted anomalous regions. But a plot using the same polar projection as the others in the paper showing how well annual average (and/or seasonal average) surface air temperatures from the model compare to observations would be quite useful to back up this claim.

Response: Thank you for pointing this out but a lot of research, in varying model configurations, has been produced to discuss the results of ECBilt. It would be outside the scope of this work to substantiate and re-analyze how well the air temperatures are produced. It is the goal of this work to highlight VAMPERS and how well as a semi-coupled version within iLOVECLIM, the present permafrost state can be reproduced.

Change: We propose to maintain the current discussion as it is presently written in the manuscript.

Specific Comments

1. p. 7991, L3: I was initially confused by what was meant by saying VAMPER(S) was “semi-coupled”, though the authors clarified this at the start of section 3.1. My suggestion would be to present this information when the term semi-coupled is first used much earlier in the article. Also, in section, 2.2.2, the authors describe a two-way coupling between VAMPER(S) and ECBilt via VAMPER(S) passing GT heat fluxes to ECBilt. I presume that it is meant that the coupled components are capable of this two-way interaction, but for the purposes of the validation experiments described in section 3.1, the model is run in a semi-coupled configuration?

Response: Thank you for highlighting an early point of confusion. We agree that bring-

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ing more clarity between semi-coupled and coupled would do better for the manuscript. Indeed, as the referee concluded, the model is run semi-coupled as intermediary step to fully coupling and for validation of the VAMPERS model to recreate the contemporary extent/thickness of permafrost as function of the iLOVECLIM climate. In 2.2.2, pursuant to comments from the other referee, the authors have already provided additional clarity that the coupled version is for future model experiments.

Change: We propose as the referee suggests to better describe the term “semi-coupled” in the Introduction: “We use the term semi-coupled since not all the model mechanisms are fed back to each other and in this case the effects of permafrost do not impact the climate”. We also propose to provide more clarity in section 2.2.2 that the coupled version will be done in future experiments and that the semi-coupled experiments are indeed for validation purposes.

2. p.7992, L12: The authors mention the inclusion of geothermal heat flux and lithology as new aspects of VAMPER in the coupled version of the model. How significant are the differences in the model results if these modifications are not included? I ask, because both of these are modifications that are not always included in coupled permafrost models and it might be an interesting sensitivity analysis to compare these different configurations. It is mentioned (p. 8000, L. 12) that a sensitivity analysis was conducted for the geothermal heat flux, but the authors do not comment on the results of that analysis.

Response: In the (semi) coupled version, we incorporated spatially varying maps which allowed the parameters of porosity and geothermal heat flux to vary in the VAMPERS model, depending on location. This is what we refer to as enhancements. Allowing heat flux and lithology to spatially vary, as opposed to a constant value throughout the globe, did not provide any notable change in the simulated permafrost distribution. This implies that the forcing of air temperature, regardless of the lithology or geothermal heat flux, dominantly determined whether permafrost was present or not. As a result, changing the lithology / geothermal heat flux will not be noticeably different in a map

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which only illustrates permafrost extent, i.e. whether it exists in the given gridcell or not. On the other hand, however, permafrost thickness is indeed sensitivity to subsurface lithology and geothermal heat flux. It is the sensitivity of permafrost thickness to porosity and the geothermal heat flux, which was demonstrated in the earlier sensitivity study published (Kitover et al., 2013). The authors acknowledge this differentiation between sensitivity to permafrost extent versus permafrost thickness was ambiguous.

Change: We propose to rewrite the sections on geothermal heat flux and lithology enhancements in order that there is clearer understanding between sensitivity of spatially varying the parameters and sensitivity of permafrost thickness to the parameters.

3. p. 7994, L16. The authors mention a constant heat flux and porosity setting used in the timestep comparison section. How were these values chosen? I presume they are simply values for a chosen gridcell in the model but is there a particular reason these values were chosen, or were they selected at random? I have the same comment regarding the values used for the parameters in the Stefan equation (Table 1) could the authors provide a source for these values or justification as to why they were selected?

Response: In the timestep comparison section, the heat flux (60 mW/m²) and porosity (0.3) were chosen because 1) they are very commonly occurring values in the subsurface since 60 mW/m² is around the global average (Davies, 2013) and 0.3 is near the porosity for a sandy mineral material (Magara, 1980) and 2) the values are the same used in the prior sensitivity study from Kitover et al. (2013) and therefore keep in continuity with previous studies regarding VAMPER. For the Stefan equation, the parameter values such as dry density and thermal conductivity are the same values used in a stand-alone version of the VAMPER model, which in turn calculates based on methods such as the geometric mean of the composite of subsurface components (water, ice, and dry soil). The methods behind these calculations can be found in Kitover et al., (2013). The forcing is a reasonable range of cold region temperatures (for example, -6 C is about the average annual ground surface temperature in Barrow, Alaska) given

within a normal amplitude.

Change: The authors feel that the parameter used in both the timestep analysis and Stefan analysis are standard commonly occurring values and as such, are self-explanatory. We propose no change is necessary here.

4. p. 7995, L4. The authors mention that the thermal offset is often expressed in a ratio format, but then don't make use of this information anywhere else in the article. I suggest cutting this sentence as it's redundant.

Response: The authors agree that this is an unnecessary statement.

Change: We propose to remove this sentence as suggested.

5. p. 7995, L20, L25. I assume that some of these variables (e.g. thermal conductivity of unfrozen soil, dry density of the soil) are identical or closely related to variables used in VAMPER code itself. Is this the case?

Response: Thank you for checking on this since making a fair comparison between the Stefan equation results and the VAMPER model results rely on using the same parameter values. Indeed, the values used in the Stefan equation were specifically chosen (for instance soil thermal conductivity) because they are the same ones calculated by the VAMPER model. These calculations are outlined in the Methods section from Kitover et al., 2013.

Change: We propose no change is necessary here.

6. p. 8001, L2. Porosity is not synonymous with soil water content unless the soil is at saturation. I think that's the case in the authors' model, but it should be spelled out.

Response: The authors agree that it was presumptuous to conclude that in any case soil water content is equal to the porosity. As the referee pointed out, we indeed assume the subsurface is saturated. Also note that in the Methods section, which we make reference to of Kitover et al. (2013), it is stated that the subsurface is assumed

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to be saturated.

Change: We propose to explicitly state in this section that we assume a saturated subsurface in the VAMPERS model..

7. p.8002, L4 onwards. The authors state on p.8001 that model experiments are run-semi coupled so that the climate is unaffected by changes in permafrost. Then on p.8002, they describe an asynchronous coupling methodology that is run until “approximate equilibrium between ECBilt temperatures and the VAMPER(S) model is reached.” I can see this coupling methodology as being necessary in the full coupled model, but is it needed in the semi-coupled configuration? If the climate does not respond to permafrost, why not just couple the VAMPER(S) model to the climate once the climate component is already in equilibrium and then simply run the VAMPER(S) model using the ECBilt air temperature forcing until it’s in equilibrium? This discussion adds to the confusion of how the “semi-coupling methodology” is handled in the paper as mentioned earlier.

Response: When the semi-coupled experiments begin, the iLOVECLIM model is already at equilibrium. The asynchronous approach is used because it allows a repetitive forcing (100-yr average temperature values) to the VAMPER model since it needs to spin-up or “catch-up” to the already-equilibrated climate. The repetitive forcing is done “off-line” so that iLOVECLIM does not have to run in synch with VAMPER, in turn making the experiment faster. If the asynchronous method is not used, the VAMPER model would take a lot longer to reflect the iLOVECLIM climate, whether or not it responds to the permafrost. The asynchronous approach has nothing to do with the type of coupling. The same asynchronous approach would be used in a fully coupled scheme as well. This transparency, applicable either coupled or semi-coupled, is why we disagree that it adds confusion. In addition, the semi-coupling is now explained twice (due to specific comment no. 1) for clarity, which includes a figure (3).

Change: We propose no change here.

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8. p. 8003, L5 Re: Figure 7. I would have found the comparison between the Circumpolar PF map and the model data to be a little clearer had the PF map been plotted as a third panel, rather than beneath the permafrost thickness map. Also, it would be good to compare the total areal extent (i.e. total area in square km) of PF vs. the estimates from the Circumpolar map.

Response: The authors disagree that the PF map should have been a separate panel. In this case, it would have been more difficult to see the overlap where the agreement and disagreement is. However, it is a good suggestion to compare total areal extent calculations and would better quantify the comparison rather than just make it visual.

Change: We propose to include a calculation of total simulated surface area of permafrost, as compared to the list from Koven et al., (2012).

9. p. 8003, L8“. This swing of inaccuracy is the result of simply attempting to match results from a low resolution grid to much higher resolution.” I agree that this is certainly part of the reason for the mismatch, but there are other factors that are known to strongly influence the ground thermal regime in permafrost regions which, I believe, are lacking in the model. For example, as far as I can tell, the authors don't account for snow-vegetation-permafrost interactions, nor the presence of organic components of soil whose thermal and hydrological parameters can be quite different from mineral soils. Also, if there's a problem with the snow scheme in the model (difficult to assess without a snow cover validation), this would presumably have a major impact on the PF distribution

Response: We agree with the reviewer that this result is only partially due to grid resolution.

Change: We propose to reword some of the discussion regarding “the swing of inaccuracy”, in particular acknowledging that the resolution is only partially the cause. There is added subsequent discussion of some of the missing air-ground coupling components following in this section 3.2.2.

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10. p. 8007, L2. The authors show that the simulated MAGT are generally slightly lower than observations, indicating either a cold bias in the climate model or some issue in the ground-air coupling. In either case, one would conclude from this observation that the model is typically simulating ground temperatures that are a bit too cold. Can the authors reconcile this observation with the earlier statement (p.8003,L8) that ECBilt-VAMPERS underestimates the permafrost extent ? These two observations seem contradictory shouldn't a model that generally underestimates ground temperatures produce a greater distribution of permafrost? Or is the cold bias specifically something that affects higher latitude points and not points along the southern boundary of the discontinuous permafrost zone?

Response: It is understandable how this contradiction could be interpreted the way the reviewer describes. However, there are two separate conclusions here. The first is the extent which is underestimated with the snow component included. The second conclusion is that the subsurface temperatures are bit too cold which implies that the areas which do have permafrost are perhaps overestimating the depth.

Change: We propose no change.

Technical Corrections

All technical corrections have been accepted and used to edit the manuscript accordingly.

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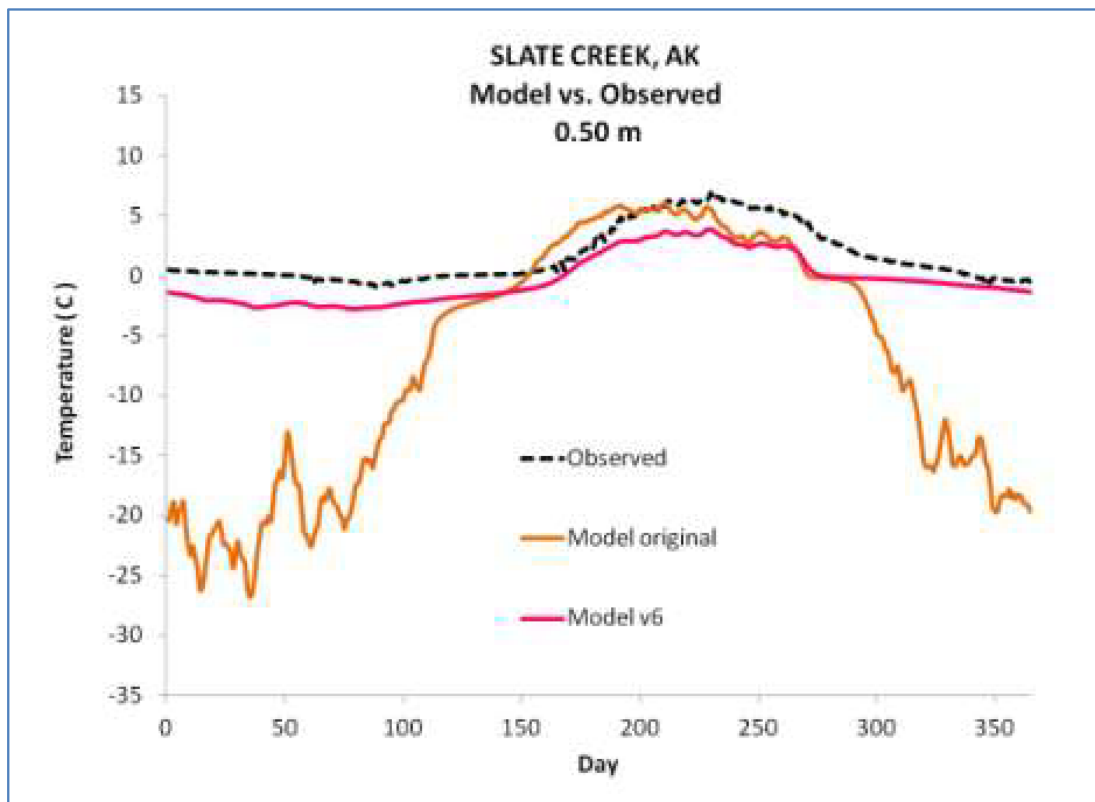
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Fig. 1. Comparison of modeled average subsurface temperatures (0.5 m deep) with and without employing snowpack in VAMPER

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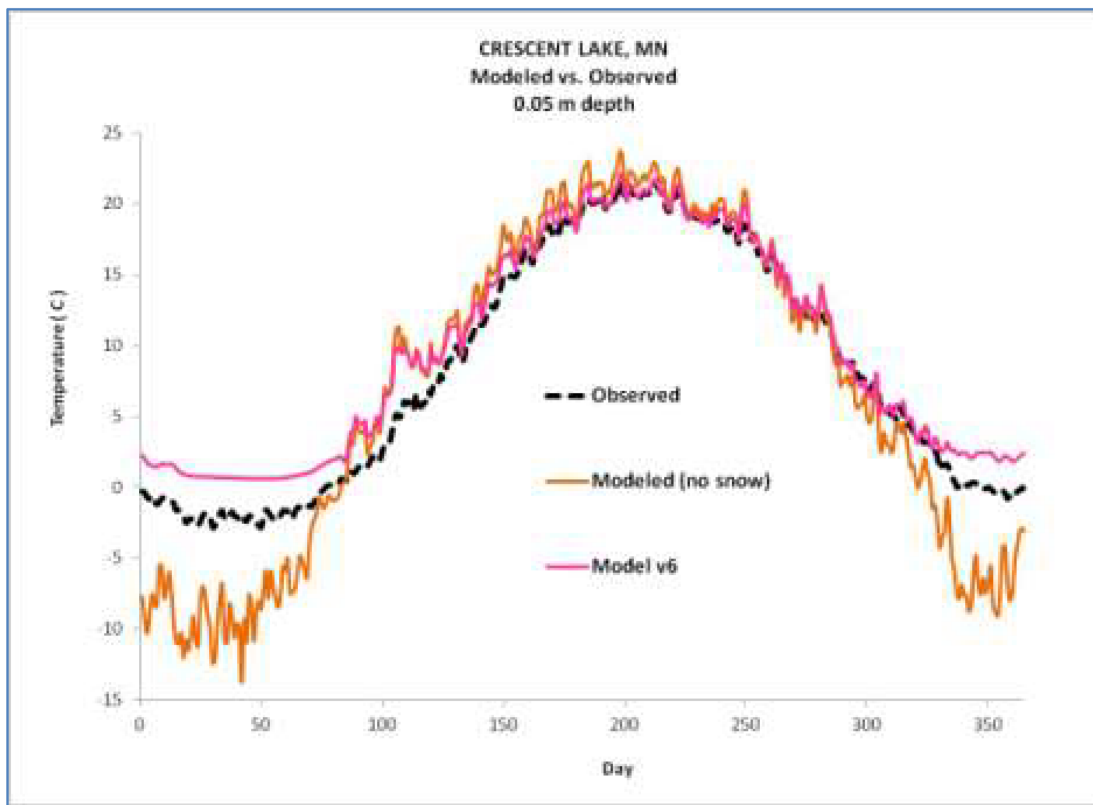


Fig. 2. Comparison of modeled average subsurface temperatures (0.05 m deep) with and without employing snowpack in VAMPER

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