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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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General Comments

I would like to thank the authors of this paper for their succinct and generally well-written submission. The authors describe the modifications made to the VAMPER permafrost model for the purpose of the coupling it to the iLOVECLIM earth system model of intermediate complexity. While the VAMPER permafrost model that is described is relatively simple compared with state of the art permafrost models that are included in sophisticated GCM-based climate models, it is compatible with the level of complexity of the climate model to which it is being coupled. The modifications made to the VAMPER

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permafrost model are clearly described and tested. The authors indicate an interest in studying permafrost dynamics across geological timescales – an area in which there has been little work done from a modeling perspective as far as I know. So, this work should help towards filling this research gap and a computationally efficient EMIC with a straightforward permafrost model would be a good tool for the long-timescale integrations required for such studies. The coupled model would also presumably be a good platform for parameter-sensitivity studies to assess how the subsurface thermal regime depends on a number of factors that are, as yet, poorly constrained by data given the difficulty of making field measurements in permafrost regions.

There have already been a number of attempts made by other modelling groups towards incorporating representations of permafrost within EMICs and GCM-based climate models. 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 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.

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

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this omission might impact their results.

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. 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.

Other than these points and the specific comments I have below, I found this to be a solid paper and I look forward to hearing the results of future modelling studies using this coupled model.

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?

2. p.7992, L12: The authors mention the inclusion of geothermal heat flux and lithol-

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ogy 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.

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?

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.

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?

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

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

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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.

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

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.

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

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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?

Technical Comments

1. p. 7993, L21 “in lieu of”.. this doesn’t make sense. Do you mean “in light of” ?
2. p. 7994, L16 “Subject to differ”... this is a bit of awkward English usage. Suggest “allowed to differ”.
3. p. 8008, L25 “relatively course”. Should be “relatively coarse”.

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