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Interactive Comment

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 A. Atchley 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. "While the authors provide a good background of VAMPER and iLOVECLIM development, specifics of how these improvements compare to the state of other large-scale Earth system models is missing (at least in the introduction), and therefore the contribution of this new capability is somewhat lost to the reader. Specifically how this model is well suited to capture the transient nature of permafrost

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Interactive Discussion
Discussion Paper

compared to what is already available from other Earth system models?"

Response: We agree that a review of the current state of Earth system models with respect to permafrost modeling would bring additional relevance to the work.

Change: We propose to add a section in the Introduction reviewing other model capabilities in capturing permafrost.

2. "The manuscript would also benefit with a more detail description in section 2.2.2 of how VAMPER(s) is coupled to ECBilt, that moves beyond figure 3 to provide specific equations and mechanics of the coupling process. Is this an implicit or explicit coupling scheme? Specifically, equations showing how air surface temperature is incorporated in VAMPER(s) and how the ground heat flux is used in ECBilt would be beneficial to readers interested in coupling processes across the land atmosphere boundary."

Response: This work is focused on the VAMPER side (i.e. semi-coupling) of the coupling where the air temperature from ECBilt is taken by VAMPER. This is fairly simple and the authors believe is described sufficiently. The associated equations and how this is solved within VAMPER is described in Kitover et al., 2013. The ground heat flux on the ECBilt side will be described in the future when the full coupling experiments are published. It is not relevant for this work beyond the conceptual idea, as illustrated in Figure 3.

Change: We propose to give a reference to the reader in section 2.2.2, which allows them to look at all the equations and how they are solved within the VAMPER model.

3. By carefully reading the paper it is apparent that the VAMPER(s) surface temperature is simply the air temperature, and while that may be adequate for the scale of the model, it is then not clear how heat flux or for that matter latent and sensible fluxes are (mentioned page 8000 paragraph 5) calculated.

Response: We agree with the referee that this is not a clear description of how the full coupling works, particularly with no equations. At this time, we choose to give

GMDD

7, C3381–C3389, 2015

a conceptual description of the full coupling without the equations since they are not actually applied in this work.

Change: We propose to additionally mention that that this description (page 8000, lines 5 – 9) is for a full coupling, which is to be described in detail with associated equations in the future work of a fully coupled ECBilt-VAMPER iLOVECLIM experiments. We have also provided more clarity on the heat fluxes at the ground surface: "The air surface temperature is calculated within ECBilt as a function of the heat balance equation where the major heat fluxes across the air/surface interface are incorporated: sensible heat flux, latent heat flux, shortwave radiation, and longwave radiation."

Specific Comments

1. Page 7993, L15-20: It is not clear that the subdaily time step is forced by diurnal air temperature because it is later stated (Page 7994 L9-13) that the temperature forcing is a sine function for the annual temperature with no subdaily (night versus day) signal.

Response: Authors agree that these are conflicting statements. The larger timesteps of previous research of course neglects diurnal effects. But it is also the case that our model, although using a 4-hr timestep, cannot capture these either.

Change: We propose to remove "in turn ignoring diurnal air temperature behavior".

2. Page 7994, L9-13: Why not use a daily timestep instead of a subdaily timestep of 4hrs? How is the sine forcing function able to capture diurnal effects? Is the 4-hour timestep only due to model convergence issues?

Response: As assumed by the referee, a subdaily timestep is used for convergence issues within VAMPER. The approximation error is reduced by using a smaller timestep. Since the suggestion of diurnal effects was removed (see specific comment 1 above), we do not claim to capture diurnal effects.

Change: We propose no change here

GMDD

7, C3381–C3389, 2015

Interactive Comment

Full Screen / Esc

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Interactive Discussion

Discussion Paper

3. Page 7994, L25-28 & 7995, L1-5: The process behind the thermal offset is not well described here. I assume it is due to differences in ice, water, and air thermal conductivities and that during the summer when positive thermal propagation is occurring the active layer is more insulative thus reducing permafrost warming. Conversely during the winter when the active layer is frozen, it is more thermally conductive and permafrost is cooled. This processes is not well described here and therefore the results by themselves seem counter intuitive.

Response: Since we have a simple model (absent of vegetation, organics, an unsaturated subsurface, or variable water content) we can easily attribute the thermal offset to seasonal differences in thermal conductivity, whereas the thermal conductivity of ice is four times that of unfrozen water and therefore the freezing front is propagated more effectively than the warming front. This difference causes a shift in the average depth-temperature profile (Fig. 1a)

Change: Additional clarity is provided in this section with the above explanation.

4.Page 7996, L25: equation 3: Is there a reference for this equation?

Response: reference available

Change: reference of Lynch-Stieglitz (1994) provided

5. Page 7998, L10-15: Here, the snowpack is discretized into three layers, but it is not clear has to how each layer evolves due to snow age and snow deformation. Why not just a one layer snow model? Perhaps it would be beneficial to describe the differences of each layers deformation process.

Response: VAMPER is a finite difference model and integrates the snowpack using three overlying snow nodes (layers). As with standard finite difference models, it typically results in a better (reduced error) approximation when multiple nodes are used. There is no unique deformation process to each layer. It simply depends on the timing and degree of the freezing/warming into the snow layers. They undergo the same deformation/melting/freezing rules. The model redistributes the snow layer thicknesses and associated density with each time step.

Change: We propose to add a simple explanation stating that there is no difference in deformation between the layers: “All three snow layer are subject to the same processes and simply depend on temperature, time, and thickness for their respective deformation and/or melting.”

6. Page 7999, L22-23: It is not clear what is meant by, “In this case, the air surface temperature from ECBilt is assumed to be above the snow.” Does this mean that the snow surface temperature is the air temperature? If so, that should probably be explicitly stated as there are other ways to assign snow surface temperature.

Response: We agree that this is a bit ambiguous. As the referee assumes, this indeed means the snow surface temperature is the air temperature.

Change: We propose to rewrite as suggested: “In this case, the snow surface temperature is taken to be the air surface temperature.”

7. Page 7999: Given that VAMPER(s) is a 1-D model, there is no lateral heat conduction or water flow, and while this is not uncommon at this scale, it is worth mentioning, so that the reader is aware of this simplification.

Response: We agree that it would provide added awareness of the VAMPER simplification and limitations.

Change: We propose to add sentence: “As VAMPER is a 1-D model, there is no lateral energy (heat/water) transfer between adjacent grid cells in the subsurface.”

8. Page 8000, L7-8: Here a heat balance equation is mentioned for use in VAMPER(s), but this equation is not presented in this manuscript. In order for the reader to understand exactly how VAMPER(s) is coupled to ECBilt it is necessary to present this equation in order to show which terms are provided from and to ECBilt. This will also help, the reader understand how exactly sensible and latent heat fluxes are calculated, which

is an important bit of information. On that note, it is worth presenting any equations on the ECBilt side to show how the coupling of subsurface and atmospheric models function.

Response: As responded to in the General Comments 2 and 3, the semi-coupling does not use any equations but rather just a passing of the air temperature variable from ECBilt to VAMPER. To find the set of equations used by VAMPER, the reader is referred to an earlier paper (Kitover et al., 2013) which presents the equations, including the standard heat diffusivity equation.

Change: As also explained in the General Comments 2 and 3, we propose that some additional clarification is provided in this section. The equation which describes the full coupling is not yet necessary for a semi-coupled model. The equations for the individual models, VAMPER and ECBilt, are available in Kitover et al., 2013 and Opsteegh et al., 1998, respectively.

9. Page 8001, L19: Was the whole model run for just the northern latitudes or whole globe? Please clarify for the reader.

Response: Model is run for whole globe.

Change: We propose as suggested to clarify that this was run for the whole globe

10. Page 8002, L5-14: While this is somewhat discussed later in the paper, it is also important here to acknowledge that while assuming the permafrost is at equilibrium with the atmosphere is perhaps an acceptable approach to this difficult problem, it is known that permafrost is not currently at equilibrium.

Response: We agree that we should make this disclaimer.

Change: We propose to add in the sentence: "Although the model approaches a steady state through the subsurface depth, we acknowledge that in reality, some of the permafrost regions are not at equilibrium since they are responding to recent warming."

11. Page 8003, L8-10: "This swing of inaccuracy is the result of attempting to match results for a low resolution grid to spatial overage of much higher resolution." This is somewhat of a simple answer to a much more complicated problem, which really highlights the need for to reconcile observational scales and modeling results. However, without specifically testing a model with spatial resolution matching the observations, it is not appropriate to state the miss match is uniquely due to scale issues, though probably part of the problem. Instead it may be more appropriate to ask if this low resolution grid is a valid approach to investigate the utility of simulating a snowpack? Is the snowpack really a model enhancement?

Response: We agree that there may be other factors which contribute to the inaccuracy or mismatch that occurs whether the snow model is used or not. However, we still contend that it is better to model the surface offset induced by the snowpack, which is one of the most dominant factors in air-ground coupling (Smith and Riseborough, 2002) rather than ignoring it. The offset map (Figure 8) and comparison to observations (mentioned in the discussion in section 3.2.1) support the VAMPERS results. In addition, because the model works at a coarse spatial scale, we cannot paramterize it to specific observation sites. During the model development, as a single site permafrost model, we were able to match observation values. These figures, one for Alaska and one for Minnesota, are provided as supplementary figures.

Change: We propose to add in some discussion at this point regarding, as the reviewer suggests, problems with modeling snow at this resolution. In addition we will mention other factors which cause mismatch in the model results, e.g. air-ground coupling.

12. Page 8003, L15: I am not convinced that at this resolution, the snowpack model is an 'enhancement'. It is however an alternative model formulation that could be used to test some idea's, though I would argue that a more spatially resolved model would be more helpful in this case.

Response: We understand the reviewer's point that the snow model does not enhance (as to improve) the results. However, improving the number of options in the VAMPER model so that there is a more realistic representation does itself "enhance" or improve the model. Because of this distinction, we have rephrased, when appropriate, description of the snow component as an additional option rather then it as enhancing the results.

Change: We propose to describe, throughout the paper when appropriate, the snow component as an option rather than as an enhancement.

13. Page 8007, L4-6: Could the fact that the simulated colder subsurface temperature is due to the lack of calculating a surface energy balance to assign a surface temperature? Doing so would account for incoming radiation fluxes, which can warm the surface relative to the air temperature.

Response: The ECBilt land surface temperature which forces the VAMPER model is already a function of a prior computed surface energy balance. The interactions at the surface include standard energy fluxes: longwave and shortwave radiation, and latent and sensible heat fluxes. This is described in Goosse et al., 2010. Therefore, it is more likely that the colder than expected subsurface temperatures are a function of either the air-ground coupling which may overlook effects from vegetation and organic layers or the porosity (water content) parameter. This were already mentioned and discussed as possibilities (3.2.1, fourth paragraph, 3.2.2 last paragraph, 3.2.3 last paragraph)

Change: We propose no change here although due to some other comments the discussion points for this topic have been extended.

Technical Corrections

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

References

Goosse, H., Brovkin, V., Fichefet, T., Haarsma, R., Huybrechts, P., Jongma, J.,

GMDD

7, C3381–C3389, 2015

Interactive Comment

Mouchet, A., Selten, F., Barriat, P.Y., Campin, J., Deleersnijder, E., Driesschaert, E., Goelzer, H., Janssens, I., Loutre, M., Morales Maqueda, M.A., Opsteegh, T., Mathieu, P., Munhoven, G., Pettersson, E.J., Renssen, H., Roche, D.M., Schaeffer, M., Tartinville, B., Timmermann, A., and Weber, S.L: Description of the earth system model of intermediate complexity LOVECLIM version 1.2, Geosci. Model Dev., 3, 603-633, DOI: 10.5194/gmd-3-603-2010, 2010.

Kitover, D.C., van Balen, R.T., Roche, D.M., Vandenberghe, J., and Renssen, H.: New estimates of permafrost evolution during the last 21 k years in Eurasia using numerical modelling, Permafrost Periglac., 24, 286-303, 2013.

Opsteegh, J., Haarsma, R., Selten, F., and Kattenberg, A.: ECBILT: A dynamic alternative to mixed boundary conditions in ocean models, Tellus, 50, 348–367, 1998. Smith, M.W. and Riseborough, D.W.: Climate and the limits of permafrost: A zonal analysis, Permafrost Periglac., 13, 1-15, 2002.

Smith, M.W. and Riseborough, D.W.: Climate and the limits of permafrost: A zonal analysis, Permafrost Periglac., 13, 1-15, 2002.

Interactive comment on Geosci. Model Dev. Discuss., 7, 7989, 2014.