Geosci. Model Dev. Discuss., 7, C2690–C2694, 2014
www.geosci-model-dev-discuss.net/7/C2690/2014/

Geoscientific Model Development Discussions

Interactive comment on "Coupling of the VAMPER permafrost model within the earth system model *i*LOVECLIM (version 1.0): description and validation" *by* D. Kitover et al.

A. Atchley (Referee)

aatchley@lanl.gov

Received and published: 18 December 2014

The authors describe results from a coupling of a permafrost model (VAMPER(s)) to an Earth system model (iLOVECLIM), or more specifically an atmospheric component (ECBilt) within the larger Earth system model. Two changes to the permafrost model were implemented 1) subdaily time step and 2) the addition of a snowpack representation. Results and needed equations from these improvements are presented and discussed in detail. On this point, I find that the authors have structured a clear paper and have done a good job describing how these two changes work. The model performs over a large time and spatial scale and therefore has been designed to pro-

vide information of larger climate trends, which out of necessity tend to neglect smaller scale processes. I think the scale of this model may be somewhat inadequate to address the utility of simulating a snowpack, as results showed the affect of adding snow to not significantly reduce model error, but significantly change model outcome. Studies that refine model scale to match observational scale would help answer the utility of simulated snow at larger scales. Nevertheless, these improvements represent refined process information and are a needed development to large-scale Earth system models. Therefore, it is of specific interest to know how accounting for fine scale process information in large-scale models affects model performance.

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 compared to what is already available from other Earth system models? 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. 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 mater latent and sensible fluxes are (mentioned page 8000 paragraph 5) calculated. Here I think the relatively minor changes mentioned above will help the manuscript be a more impactful paper and provide readers with specific information regarding coupling schemes.

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.

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?

3) Page 7994, L25-28 & 7995, L1-5: The process behind the thermal offset is not well described here. I assume it is due 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.

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

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.

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.

7) Page 7999: Given that VBAMPER(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.

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.

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

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.

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?

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.

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.

Technical corrections:

1) Page 7996, L2: Omit 'below' in "Table 1 below gives. . ."

2) Page 7996, L13-14:"Goodrich (1982) is a well-know study which recognized the importance of including snow in numerical modeling of subsurface temperatures." Though obviously an important citation for this manuscript the sentence seems out of place here and not a concluding sentence to the paragraph.

3) Page 7996, L22: "As a result, there are an addition set" should be, "As a result, there is an addition set"

4) Page 7997, L9: The acronym 'EMIC' is not defined.

5) Page 7997, L13: A ripe snowpack has traditionally been associated with the temperature of a snowpack and the ready-ness to melt, not snowpack density, which I believe Dingman (2002) also uses ripe this way. However agree most snow deformation models limit snow density to below 500kg/m3.

6) Page 8002, L12: Omit 'to be'

7) Page 8007, L14-20: Are these two sentences supposed to make the last paragraph in this section? If so, they don't seem to go together, but it looks like important information.

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