Toward improved parameterization of a macro-scale hydrologic model in a discontinuous permafrost boreal forest ecosystem
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Summary

This manuscript develops and tests a method for improved representation of permafrost-related spatial variability of soil and vegetation characteristics in hydrological models. Building on the empirical association between topographic aspect and localised presence/absence of permafrost (and consequential impacts on soils and vegetation), the authors develop two simple, high-resolution predictors of vegetation type and soil properties, resulting in new maps of parameter values for a land surface model. One of the two predictors is a local, site-specific map defining permafrost occurrence, and the other is aspect, the product of topographic analysis using widely available data. They compare their new parameter maps to a more generalised source of this information, using each as input to the VIC model. The authors find that the widely topography-derived information provides a useful improvement over the generalised soil and vegetation data. The result is potentially relevant to any region underlain by discontinuous permafrost whose occurrence is controlled mainly by aspect-driven differences in solar radiation, though the testing is still at a preliminary stage. Although the authors used VIC as their model, it seems that the approach is independent of the model, so may be of quite general interest.

Overall, I found the science question was clearly identified, and the proposed approach sound. To me the results of the testing (better performance measures using the new approach) were encouraging, as opposed to compelling. It's not clear whether the improved representation of soils makes a big improvement to the flow simulations, but it's a step in the right direction. In principle, given the field-based knowledge of permafrost impacts on hydrology, it seems entirely rational to include this kind of permafrost information if it is available.

Main Points

1. Near the end of Section 1, I think there needs to be a short section on strategies for representing subgrid variability in large-scale models. The general problem is well-known and there are several possible solutions, e.g. mosaic of tiles within a cell, statistical parameterisation, flux-matching parameterisation (MPR method of Samaneigo) ... How does your approach fit into the range of options? Why did you choose your approach?
2. Similarly to the last point, why did you choose VIC? Presumably your approach is quite general, and still applies if the model is not ViC?
3. P5 "by implementing a small-scale parameterization scheme" I found this terminology confusing. The phrase "parameterization scheme" is conventionally used (e.g. in numerical weather models) to describe how the grid-scale flux relates to grid-scale properties, when taking account (implicitly) of sub-grid variability. But in this paper, you are using that term to mean a method of creating grid-scale maps of soil and vegetation parameters. I think the use of the word "scheme" may be misleading to some readers.
4. P17 "we classified a grid cell as permafrost containing when the fraction of permafrost in the grid cell is greater than 0.5" (and also a similar point from P11) What are the implications of doing it this way at the 1 sq km scale? You need to discuss the length scale of the "true" permafrost field vs the model grid scale.
5. Table 3 The HighP catchment shows 42.3% permafrost when derived from aspect, but 83% permafrost when derived from permafrost map (based on a soils map of Rieger 1972). This deserves some comment about how aspect is calculated and up-scaled from 30m to 1km

(Fig 3a). How much of the 1972 permafrost has thawed since? Also, some comment is needed about the authors' decision to rely on the soil map of Rieger to define permafrost, rather than using for HighP the 55% of Table 1, or the 53.2% (and associated map) given by Haugen et al (1982, their Table 1, Figure 3). 83% is very different from 53%! Presumably this decision could have a significant impact if the aspect-based approach was to be scaled up to more of the Alaskan interior?

6. Figs 7-11: I did not find the details of simulated ET and soil moisture very helpful. They revealed what was generally expected from prior knowledge of LowP vs HighP, and no field observations were presented to verify the model outputs (if relevant field observations are available, they would greatly strengthen this part of the paper). The differences between parameterisations were clear once the runoff was shown, provided one knows that long-term storage changes are negligible. Perhaps the figures would be more relevant as supplementary material, whose purpose is to show that the model simulations of ET and soil moisture in these catchments are not inconsistent with process knowledge? With maybe only one key figure retained in the body of the paper?
7. P40 Paragraph starting "Several studies have documented that VIC soil moisture simulation is strongly sensitive to ..." This research question of the sensitivity to soil/veg parameterization of model-simulated ET and soil moisture is difficult to usefully address without observations of those processes. Now that we see VIC is sensitive to the difference in parameter fields, how do we know which parameter field leads to more reliable ET & soil moisture?
8. Usually, models such as VIC are used to assess hydrologic response at scales much larger than a few sq km. Having shown that VIC simulates the difference between LowP and HighP more accurately with the new parameterisation, it would have been interesting to see what difference this makes at larger scales, such as at the outlet of CPCRW. This would also tie in well with your stated aspiration for application on larger domains. Can you include such assessments?

Minor Points

9. P2 "Simulated hydrographs based on the small-scale parameterization capture most of the peak and low flows with similar accuracy in both sub-basins compared to the parameterization based on coarse resolution dataset." Not clear by what is meant with this last comparison
10. P5 "due to the large differences in the rates of and controls on ET" Do the differences in plant canopy also cause differences in snow interception?
11. P5 "Hydrological modelling using these coarse resolution datasets cannot produce accurate estimates of the spatially variable and basin-integrated watershed responses." You need to say why this cannot be done. I presume the main reason is that the coarse datasets contain spatially-smoothed characteristics, but hydrologic response is not a linear function of the true heterogeneous characteristics?
12. Fig 2: It would be good to mention briefly in the text what is the spatial density of the source data which underlies the gridded climate maps.
13. P10 "In addition, the relationship between vegetation, permafrost, slope and aspect [Hinzman et al., 2006; Morrissey and Strong, 1986; Viereck et al., 1983] is also introduced during the modeling activity." It is not clear what you mean by modelling activity - can you be more specific?
14. P13 During the description of VIC data sources (esp vegetation), it would be useful to remind the reader that these are coarse-scale inputs.

15. P17 When you introduce Fig 4a, I think it is useful to point out to the reader that the FAO soils map does not include any of the permafrost soils which are known to be present in CPCRW.
16. P21 "Values between 1.0 and 0.0 are widely considered to be acceptable levels of model performance [Krause et al., 2005]." I think that higher values of NSE than 0.0 are generally required to reach the "acceptable" standard. Please look a little more widely at the literature to establish a benchmark. In strongly seasonal cold climates, one can achieve values much larger than zero with models which are very poor indeed (See Bettina Schaefli's paper on "Do Nash values have value?").
17. Fig 5 (and later figures): More helpful to refer to LowP and HighP rather than C2 and C3 basins
18. Fig 6: For C-2 the observed streamflow is almost constant for extended periods, and drops suddenly in all 3 years. Is this the way the watershed functions or is it a measurement artifact?
19. Fig 12 - need to explicitly state that this water balance is based only on observations? (is that correct?)
20. P41 "The mean annual change in storage in the LowP sub-basin is about 35% higher than the HighP sub-basin." No evidence was shown to support this point. In Fig 12 it was stated that storage changes are negligible.