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The Cryosphere Discussions

Interactive comment on "Process-level model evaluation: A Snow and Heat Transfer Metric" *by* Andrew G. Slater et al.

Anonymous Referee #2

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General comments: In this paper the authors develop a relatively straightforward diagnostic metric (SHTM - Snow Heat Transfer Metric) for establishing whether the heat transfer through the soil-snow layer is realistically simulated by a climate model. The diagnostic is based on the amplitude equation for the conductive heat flow which is integrated over the period when air temperature are below freezing to obtain the difference in the seasonal temperature amplitudes at some depth in the soil, and the effective snow depth which describes the insulating effect the snow layer over the accumulation season. The authors use observed air temperature, snow depth and soil temperatures at 20 cm from climate stations in Russia, Canada and the USA to obtain an estimate of the curve relating the effective snow depth to the normalized difference in temperature amplitude (Figure 3). There is considerable scatter around this curve which the authors describe as "noise" or "error". However, I suspect that the results shown in Figure 3

represent a number of different curves that reflect different snow-climate regions (e.g. Sturm et al. 1995) and soil properties (e.g. organic soils). The authors then compare the ability of 13 CMIP5 climate models to replicate the observed relationship derived from the surface observations using all land grid points north of 55 deg N (Figure 4). The results show major differences between models but one question that crops up at this point is whether the somewhat limited spatial sample of observations (Fig. 2) influences this comparison. Repeating the analysis for grid points nearest the observations would answer this question.

The large difference in SHTM between models is worrisome but we don't get any sense from the paper of the climatic consequences of a poor fit to the observed heat transfer relationship and how much of the poor fit is coming from representation of snowpack versus the specification of soil thermal properties. Presumably one would not use a model with a poor SHTM metric for studies of the soil thermal regime or permafrost, but apart from that I'm not quite sure what the metric tell us. The metric would certainly have value in evaluating the performance of different versions of climate models and land surface schemes. One aspect of the paper that could be expanded on (topic for follow-on paper?) is the spatial variability in SHTM in observations and models.

In conclusion this paper is a useful addition to the literature and a testament to Drew's ability to derive practical applications from complex processes.

Detailed comments:

- Page 1, line 30: Mudryk et al (2016) would be a useful reference to cite in this context as it specifically addresses the uncertainty issue in observational SWE datasets

Mudryk, L.R., C. Derksen, P.J. Kushner and R. Brown, 2015. Characterization of Northern Hemisphere snow water equivalent datasets, 1981–2010. Journal of Climate, 28:8037-8051.

- Page 5: Observed data. The authors have a rather limited sample for characterizing the NH land area average amplitude used in eqn. (7). It would be instructive to provide the readers with some idea of the variability in Fig. 3 for a sample of the major snow-climate (e.g. Sturm et al. 1995) and ecoclimatic regions.

- Figure 4: I suggest you use “scatter” rather than the statistical term “error”

- Figure 5: the derivation of Figure 5 is not provided in the paper and there is no discussion of this Figure. This shows the CMIP5 ensemble close to the observations but this is a potentially misleading message.

- Figure 6: What about spatial variability in SHTM? Is this important? How does this vary between models? To what extent do the different geophysical fields used in models contribute to this variability i.e. how much of a model’s behaviour in SHTM is related to representation of the snowpack versus specification of soil thermal properties?

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