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Discussions

Interactive comment on "Effects of variability of meteorological measures on soil temperature in permafrost regions" *by* Christian Beer et al.

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This paper presents an exploration of a fairly theoretical question: what role does high frequency variability in meteorology have on the low-frequency dynamics of soil temperature in arctic regions. The authors show that, because snow tends to melt during brief warm period in the fall and winter, when temperature variability is reduced, less snow melts, which leads to more snow accumulation and warmer soils. This is not immediately obvious given the expectation that soil acts as a strong low band pass filter on temperature variability, but once explained, it makes perfect sense. Conversely, there is also a mechanism associated with bryophytes, which is less well explained in the text, but which leads to somewhat the opposite response.

I have a couple issues with the paper, but I first want to say that I disagree with the first

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Discussion paper

reviewer's comment that this is more appropriate for GMD. This is clearly a model application rather than a model development paper, so I think this journal is an appropriate one for the work.

One issue I have is in the explanation for the bryophyte mechanism. I get the argument about reduced productivity leading to reduced bryophyte cover, but this appears minor in the actual permafrost region itself. So I don't understand why the effects on soil physical properties ought to be large. Is this just an outcome of the soils being warmer and therefore thawed longer in the spring and fall or deeper in the summer under reduced variability, which then leads to different time-averaged soil physical properties since the thawed and frozen states are so different for organic soils? If this is the mechanism, then it is really specific to the bryophyte representation per se, or ought any realistic organic soil parameterization give qualitatively the same result? In any case, the fact that soil temperatures in the annual mean appear to be almost entirely warmer in the reduced variability case argues that the snow mechanism is the dominant one, so you probably ought to state as much in the paper.

A second issue I have is in what these idealized results actually mean in terms of more policy-relevant questions such as climate projections. Under warming, snow cover is expected to be reduced over most of the warmer part of the permafrost region. So that ought to attenuate the importance of the snow mechanism here. Furthermore, models which are driven directly by GCM future scenarios ought to explicitly capture this effect, so in principle this may already be built into those permafrost projections. But perhaps an issue is that for some studies, e.g. those which are used in the permafrost carbon network future scenario MIP, and many papers that are using that output, (and which I've been involved in, hence my waiving of anonymity in this review), the protocol forcing data for the future period is a hybrid that uses high frequency data that comes from reanalysis data, but with climatological anomalies relative to the present from a GCM imposed onto the reanalysis output for future. This design was chosen to avoid the mean-state biases present in almost all GCMs, but perhaps it leads to new problems that are exposed here if the variance changes. I.e., such an experimental design implicitly assumes that only the long-term means are changing and not the high frequency variability about the means, which is still coming from the reanalysis data. So, how big an effect is there from ignoring the projected changes in the variability? I suggest this paper would be most useful to the community if it actually tried to answer this question. Doing so properly would require a few more runs: i.e. something along the lines of taking a GCM scenario meteorology output, and then either include changes to the variance over time or not, while in both cases leaving the changes in the mean state due to the scenario, and then use those to force JSBACH offline to assess how large the bias in projected soil temperature, permafrost extent, etc, changes are in the current PCN-MIP way things are done. I recognize this would require substantially more work on the authors' part, but I'd suggest the authors consider doing such an experiment if they really feel like this is an important result that the community needs to take seriously.

A last minor issue, on page 9, line 268. This statement needs a lot more support if it is to remain. Several of the models in that study have some version of the effects described here. While every model that does will include effects like organic soil and snow physics differently, it is not at all clear that most of the models in that MIP will not include the essence of these effects. I don't think it is the authors' intention to assert that no other models have snow which melts during brief warm spells, or that the models that include organic soils (but not bryophytes) necessarily miss out on all of those effects described here, so I'd suggest not making the statement.

Minor point: The symbol (-) as a unit in several figures is unclear. I see that you are using it to mean relative fractional differences, but please state that somewhere in each figure.

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