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Interactive comment on "Effect of soil property uncertainties on permafrost thaw projections: a calibration-constrained analysis" *by* D. R. Harp et al.

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We thank the reviewer for a careful review of our manuscript and the insights she/he has provided. In the responses below, we describe how we utilized the three major points and one minor point provided by the reviewer to clarify and improve our manuscript. A PDF of the manuscript with marked-up revisions is also provided and referred to below with line numbers.

Major Points:

1. It is obvious from the high parameter uncertainty (and not surprising for a soil physicist), that temperature data alone is not sufficient to get a well confined parameter set.

As freezing and thawing of porous media is a tightly coupled process where heat and water transport interact, there is obviously information missing about the total water content of the material. Additionally, the information content in the calibration data is quite low as can be seen in figure A-1 to A-3. The temperature is constant for long periods of time as a consequence of the zero-curtain effect or isolation by snow. I am pretty sure that an in-depth survey (e.g. with virtual data) would show that temperature measurements at fewer locations combined with measurements of water and ice content would give a parameter set with much less uncertainty. Thus the availability of only temperature data should be mentioned as one of the main reasons for the uncertain predictions.

We thank the reviewer for indicating that the limitations of using temperature data alone should be more clearly stated in the manuscript. The reviewer brings attention to a relevant point that incorporating different types of data can constrain parameter uncertainties. The point is particularly relevant with respect to augmenting temperature measurements with water and ice content measurements to help constrain soil property uncertainty in permafrost regions. The manuscript quantifies the uncertainty in the case where only temperature measurements are available, a common scenario given the relative ease with which temperature measurements can be obtained compared to many other types of data. The soil property uncertainty would be expected to decrease if other types of data were incorporated, such as ice and water content. To ensure that this point is clear to the reader, a paragraph has been added to the introduction (lines 91-96) and the existing discussion has been augmented in the discussion and conclusions section (line 552).

2. Even with a total of 16 calibrated parameters the model is obviously not at all capable of describing the data. The authors refer to the fraction of temperature measurements which are in the 95 percent confidence band. However, given the fact that the temperature does not vary much most of the year, this is of minor importance. At the times when the temperature changes most (during freezing and thawing) the tempera-

ture measurements are nearly always well outside the 95 percent confidence band. A model, which can not reproduce the data will most certainly result in a ill-conditioned parameter estimation problem. I would expect that a thorough analysis of the response surface of the objective function should show a number of local minima. However, due to the high computational effort, the authors concentrated in this paper on investigation of the uncertainty around a single calibration point, which might result in an underestimation of the uncertainty.

We thank the reviewer for indicating that our decision to apply NSMC to a single calibration point as opposed to multiple calibration points was not fully described. The reviewer brings up a good point with respect to NSMC; a limitation of NSMC is its linear approximation of the correlation matrix. However, in our inspection of the uncertainty produced by NSMC around the single calibration point, we discovered that parameter combinations spanned the majority of the parameter space (refer to Figure 2). Investigation of demarcation between null space and calibration space described on lines 291-299 indicated that the inclusion of parameter combinations outside the selected null space resulted in larger simulated temperature ranges than warranted. We therefore concluded that applying NSMC to a single calibration point does not underestimate the soil property uncertainty in our case, even though this will not necessarily be true in other cases. We have added a paragraph on lines 246-250 to clarify this to the reader.

3. The authors did not mention how they set the initial condition of the porous medium in the calibration process (neither in this paper nor in the cited paper of Atchley et al. (2015). Especially the amount of water initially in the profile is a crucial point, which might result in bad calibrations if not properly set.

We thank the reviewer for identifying this omission, and agree that a description of the initial conditions are important. A sentence has been added to the Methodology section on lines 120-123 describing the spin-up process used to generate the initial conditions for the calibration model.

Minor Point:

1. A minor point is that the authors only use a simplified version of the Null-Space Monte Carlo method (which already is a simplified scheme itself). In the ideal case a globally convergent inversion scheme would be used to obtain pareto-optimal parameter sets. In the Null-Space Monte Carlo method parameter sets with a similar agreement with the data are obtained by analysing the (linearised) correlation matrix at the terminal point of a gradient-based inversion scheme. The (quasi) null space of the correlation matrix of the linearised problem is used to obtain initial guesses for such parameter set. In the original Null-Space Monte Carlo method these are then improved by again applying the gradient-based calibration. This is not done by the authors, which might lead to an overestimation of the parameter uncertainty. I can not really follow the argument why the authors do not deem this necessary. On the other hand Tokin and Doherty (2009) hinted at the necessity to use a Multiple Null-Space Monte Carlo method, with more than one starting point if several local optima might be present. This is not done here, which might lead to an underestimation of the parameter uncertainty.

The reviewer brings up a valid point with respect to recalibration of the initial null space samples. Our decision to skip the recalibration step is unconventional as far as NSMC is concerned to date. However, this decision was based on careful analysis of the non-recalibrated null space samples. Recalibration was not used in the NSMC process here to ensure that simulated temperatures were not constrained beyond plausible ranges of uncertainty given measurement and model structure uncertainties. Based on the RMSE of the measured vs simulated temperatures, we concluded that the parameter uncertainty is not overestimated and that recalibration would lead to underestimation of the uncertainty and bias in the ensemble. We used this information to decide that recalibration would not be a prudent step in the present case. A sentence has been added (lines 302-304) to further explain the rationale for not recalibrating the ensemble. The paragraph that contains this sentence has been modified and moved within the manuscript to help clarify this discussion. The rationale for not using Multiple NSMC is addressed in Major Point #2 above.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/9/C1661/2015/tcd-9-C1661-2015-supplement.pdf

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Interactive comment on The Cryosphere Discuss., 9, 3351, 2015.