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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.

Anonymous Referee #2

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In the paper a simplified version of Null-Space Monte Carlo method is applied to asses the prediction uncertainty for different metrics to characterise the development of permafrost in the next century. The authors show that the parameters obtained in a previous paper have a rather high uncertainty, which of course results in uncertain predictions.

General comments:
The paper is well written and well structure. As I am soil physicist and and not from the climate or permafrost modelling community I don't feel competent to judge if this is really new to this community. I think the paper might be valuable in demonstrating the high uncertainty resulting from uncertain knowledge of soil properties, even if (com-

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pared to the rest of the community) a lot of temperature data is used and thus can be published.

However, in my opinion below all the details given by the authors there are three major points which are not properly highlighted (though partially mentioned) by the authors:

- 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.

- 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 temperature 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

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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.

- 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.

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.

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