

## ***Interactive comment on “Estimation of thermal properties of saturated soils using in-situ temperature measurements” by D. J. Nicolsky et al.***

### **Anonymous Referee #2**

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### **General comments**

This paper describes a method to determine ground thermal diffusivity using observed temperature time series at specified depths. To achieve this, a novel finite element method is described, which is able to deal with the development of permafrost by an enthalpy method. It can be used for estimation of diffusivity and other physical parameters, solving an inverse problem. Special emphasis is put on the necessity of good initial values, and a method is given based on prior solution of simpler subproblems. This approach is tested on field data. The paper is potentially of great interest to workers in the field, but also to other scientists dealing with permafrost and its effects.

Unfortunately the paper is not well organized, which makes it difficult for the reader to follow its logic. Thus the paper should only be published with major revision.

The paper should be reorganized in several places to ease following the logic. Section 3 ("Review of existing methods ...") contains parts which (1) typically belong into the introduction, reviewing deficiencies of older methods and motivation for new ones; (2) state the inverse problem (could be merged with Section 6); (3) give an example of the methodology, which could be shortened considerably or omitted.

Section 4 ("Solution of the heat equation... ") should directly follow the physical setup (Section 2, "Modeling of soil freezing and thawing").

Section 5 ("Selection of an initial approximation") should follow the description of the optimization process (Section 6), because the importance of the initial values depends on the algorithm, as in the case a gradient-type method. It could also be merged with the site-specific Subsection 7.2. Section 5 and Subsection 7.2 are particularly difficult to understand, and should be revised accordingly. Many of the details are probably not important to the reader, while the general idea of constructing physically reasonable subproblems is. The first two paragraphs of Subsection 7.3 ("Global minimization...") would fit perfectly into Section 6.

### Specific comments

The description of the physical and numerical model lack some crucial information.

Section 2: What fluid and ice properties are used?

Section 2: Unfrozen water content: As there are many possibilities for the choice of  $\Theta_l$  (see e. g., Lunardini, 1987 or Galushkin, 1997). Why use exactly this function? Is there observational support for this? Is the freezing curve really as 'discontinuous' near 0 °C?

Section 2: Boundary conditions: On p. 217, l. 10ff, it is proposed that of Dirichlet

boundary conditions (BC) are used at the ground surface and at a depth  $l$ . Why choose this kind of BC? In geothermal studies usually a Neumann BC is assumed at the bottom. At which depth  $l$  is the BC imposed? On p. 237 an  $l$  of 1.06 m is given. Taking the assumed length of time series ( $t = 120$  days  $\approx 10^7$  s) the thermal depth constant  $\sqrt{4\kappa t}$  is a few meters. If this is right, there probably will be numerical problems independent of the type of BC chosen (see, e. g., Stevens et al., 2007). The choice in Fig. 3 is more reasonable.

Section 3: The last paragraph is misleading. Some of the results apply only to gradient-type methods (which are not used in this article), which are usually meant by "iterative methods". There are many other methods (often of stochastic character), which will not get trapped in the nearest local minimum. Further, non-existence, non-uniqueness, and instability are common with most inverse problems, in particular data and model are uncertain. Therefore the discussion of uniqueness here is superfluous, as in inverse problems we usually are interested in characterizing a set of possible parameters. Additionally it is not at all clear, how the forcing function (surface temperature) should directly influence the uniqueness condition, if not by complicating the necessary basic physics.

Discussion: Though the idea of solving physics-based subproblems is interesting, the question of its applicability for general situations is open. It would probably not be efficient (or even not work) if the distribution of misfit measures in parameter space do not show elongated or banana-like structures, but bubbly features. The inclusion of more complicated, time-varying physics (e. g., unsaturated soils, fluid flow, salinity) may influence the situation. How would more sophisticated methods like Markov Chain Monte Carlo or Genetic algorithms on the problem (without the subproblem step)?

Conclusions: What is "commonly exploiting data assimilations"? Which data assimilation methods will be used with the estimates obtained with the methods described in this article? It would be interesting which further applications for the method will be possible.

## Technical corrections

P. 215 and References: Moelders, not Molders

P. 231, Eqn. (25):  $\tilde{m}_{ij}$  should be  $\tilde{m}_{ii}$ ? Also, having the formulae constraining time steps and cell size would ease the understanding.

P. 230, l. 10ff and Fig. 3: What is TA exactly?. The behavior of the "lumped TA" curve is strange: Is there any explanation why lumping leads to isotherms jumping from node to node? Which  $\Delta t$ , which parameter  $b$  were used?

P. 247: *Computers & Geosciences*, not *Computational Geosciences*

P. 249: No capitals in reference Stafford.

## References

Galushkin, Y. (1997), Numerical simulation of permafrost evolution as a part of sedimentary basin modeling: permafrost in the Pliocene-Holocene climate history of the Urengoy field in the West Siberian basin, *Canadian Journal of Earth Sciences*, 34, 9358211;948.

Lunardini, V.J. (1987), Freezing of soil with an unfrozen water content and variable thermal properties, *CRREL Report 88-2*, *Nat. Tech. Info. Serv.*, 88-2, 23.

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