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The Cryosphere Discussions

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

A. Hasler (Referee)

andreas.hasler@geo.uzh.ch

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General comments

In this paper a new algorithm of the ground heat conduction is developed and applied to estimate the thermal properties of saturated soil. Thereto a minimization of the deviation between forward modeling and field temperature measurements is performed in two subsequent procedures: For an initial approximation of physical soil parameters and model coefficients, particular measurement intervals are considered stepwise. The resulting values permit a global minimization with respect of all parameters over the whole calibration period. The application of the evaluated parameters over three years

of data shows a good accordance between modeled and measured temperatures.

The presented work is relevant for temperature modeling of wet periglacial soils but also for the analysis of existing field temperature datasets. Especially the concept of parameter estimation using selected data subsets is a useful and novel technique applicable to other situations.

Despite the interesting content of the paper, the reader is not sufficiently supported in understanding and distinguishing different model approaches and parameter estimations. Minor revisions in description and structure are needed to make the presented work accessible for a larger community.

Compared to title and abstract, the development of the new algorithm takes a bigger weight in the main part and the conclusions. If this algorithm is not yet presented in other publications, its visibility here should increase.

To consolidate the results of the final model evaluation, the described difference between measurement and simulation could be compared with the accuracy of preexisting models for the same situation.

Specific comments

Title: In consideration of the comment above, I suggest to integrate the aspect of model development also to the title (e.g. estimation of thermal properties of saturated soils minimizing differences between a new heat conduction model and in-situ temperature measurements).

Section 2:

The definitions of some parameters repeat and are sometimes inaccurate (see technical corrections). Formula (3) could be supported with a graph (or refer to figure 2 with an additional b value).

Section 3:

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Subtitles would help for internal structure of the paragraph as the change from review to description of approach is not very sharp: e.g. 3.1 geophysical techniques; 3.2 inverse modeling techniques; 3.3 inversion by cost function minimization; 3.4 example

Section 4:

From p. 226, l. 11 the description of the developed algorithm starts but it is still under the subtitle: 4.1 A review of numerical methods. A new subtitle (4.2) would emphasize this change.

p. 226, l. 14:The term between formula (13) and (14) would better be expressed as formula with the mentioned left hand side.

In the finite element formulation the latent heat term reappears (16) and does not follow the earlier introduced C_{app} expression.

Section 5:

I don't understand the system used for the indices $^{(i)}$ and $^{(j)}$ in this paragraph (as they both refer to soil horizons and not to the matrix dimensions?).

How exactly the porosity η is estimated (p. 234, l. 3) without influencing the later approximation of thermal conductivity?

On p. 235, l. 6 it is mentioned that C_t was approximated before but it is nowhere mentioned. Refer to formula (6) and the earlier approximation of C_f and η.

Section 7:

How do model errors influence the results of this approximation? Are they negligible?

The difference between measurement levels and layers of the estimation should be stated clearer to avoid confusion (p. 236, l. 17ff).

Section 8:

p. 244, l. 11ff: But this depends on parameter b which is estimated in this model!?

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Section 9:

Better do not stat the algorithm development on the final position of the conclusions (p. 245, l. 5ff) as long as it is not one of the major topics of this paper (see also comment above).

Table 3: use the term *initial approximation* in the table description

Table 4: text not finished

Figure 3: mark area of right graph within left graph

Figure 4: legend and description are repeating

Figure 8 to 11: Are all this figures necessary to follow the procedure of the initial approximation?

Technical corrections

p. 217, l. 4: θ_l is volumetric *liquid* water content

For formula (3) one should emphasize that T is in °C

p. 218, l. 1: Φ is mentioned as soil saturation. But ice should not be included in Φ. Better: liquid pore water fraction

p. 221, formula (9): m is not introduced, shouldn't it be n?

p. 222, formula (10): same as (9), also in text

p. 228, l. 5: reference for Picard iteration

p. 234, l. 1 (after (26)): η is *porosity*, not *water content*

p. 242, l. 17: *global minimization* instead of *initial approximation*

p. 243, l. 3: *parameter estimation* instead of *initial approximation*

p. 244, l. 13: clarify figure 2, not formula (2).

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Interactive comment on The Cryosphere Discuss., 1, 213, 2007.