

Interactive comment on “Comparison of six algorithms to determine the soil thermal diffusivity at a site in the Loess Plateau of China” by Z. Gao et al.

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Review of ‘Comparison of six algorithms to determine the soil thermal diffusivity at a site in the Loess Plateau of China’ by Z. Gao et al.

N.B. This material is also given as a pdf file in which the equations that dropped out here are correctly reproduced.

Major comments

The paper is effective in pointing out the deficiencies of the various heat flow models used to estimate the soil thermal diffusivity, but although I am not familiar with that lit-

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erature, I strongly suspect most of these were already known. Despite the obvious limitations, the various algorithms were only tested against each other, and independent measurements of the relevant soil properties were not made. Why was this independent check not performed?

I find the experimental part of the paper inadequate. Although by and large the methodology is suitable for the task at hand, I think a proper evaluation of the various techniques require a more elaborate data set. You simply cannot draw strong conclusions based on measurements over only seven days on a single bare soil. To be convincing, one needs different soils, different seasons, various weather conditions, and different vegetation covers. And the omission of independently measured values severely limits the value of the data set. Furthermore, measurements at only two very shallow depths limits the scope of the study to soil surface processes, although heat flow in soils affects crop development through the heat regime in the entire root zone.

In all, the paper has the feel of reporting unfinished work; I expected either a more thorough assessment of the various algorithms through more comprehensive field work as detailed above, or a push towards an improved algorithm to which the authors refer in the conclusion. In the final two of the minor comments I discuss what can be inferred on $\lambda(\eta)$ from the material presented here; this left me wondering why the emphasis is so strongly in k instead of λ . The paper offers no rationale for this, although I am willing to accept there are valid reasons of which I am not aware. With the broad readership of HESS expanding the Introduction to provide this rationale if it exists may be worth considering. As can be seen in the final minor comment, I am particularly worried about the casual way in which the dependence of the thermal diffusivity upon the water content is brushed away by simple averaging (p. 2260, l. 8-11). This deprives any future theoretical work from including significant and relevant physics regarding the interplay between water content and diffusive heat flow.

Given the properties of water, significant heat transport takes place through flow of liquid water and water vapor flow, and heat conversion can be quite significant at evap-

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oration fronts in drying soils in arid climates. The body of literature devoted to these processes is poorly represented in the algorithms presented here; only one of them includes conductive heat flow, and does so in a highly simplified fashion. The authors recognize this but do not follow through.

In its current state, the paper explains the limitations and inadequacies of the various algorithms by demonstrating them on a limited data set, and stops there. I would like to see more substance, and a more complete development to contributing new knowledge.

Minor comments

p. 2249, l. 4: What is true for the soil surface?

p.2251, l. 4: I do not understand the rationale for restricting the observations to the top 10 cm of the soil. I can imagine the temperature profile in the root zone to be important for the vegetation development. Observations at larger depths would be valuable for applications outside the realm of climate studies that you mention in the Introduction.

p. 2251, eq. 3: I think C sub g does not belong there.

p. 2252, l. 13: the average temperatures are true averages that can be estimated by the daily minimum and maximum; only if the sinusoidal approximation is perfect would the estimate be exact.

p. 2258, l. 5-7: Is it not obvious that there is an upward flux of soil water during evaporation?

p. 2258, l. 12-13: Please explain why the smoothing does not reduce the estimates of the temperature amplitudes, or why reduced amplitudes are not a problem.

p. 2259, l. 14-17: Please include references to the 'earlier researchers' (or rather: research). Since air is a good heat insulator, it stands to reason that λ monotonically increases with the soil water content. Also, you presented a linear relationship between

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the soil heat capacity and the volumetric water content with a positive slope. Thus we have:

With $\lambda(\eta)$ an unknown, monotonically increasing function, and a equal to $(1-\eta_s)Cs$.

If k peaks at some value of η (denoted η_{peak}), then its derivative must be zero there. This implies:

Rearranging, separation of variables, and integration gives:

Note that the integration constant c must be positive if λ is to increase with η . The equation shows that λ must (at least locally around the peak) depend linearly on η . Did you find anything in your data or in the literature to corroborate this?

p. 2260, l. 8-11. According to the first equation I give above (that I derived from the material in the paper), k depends on η in a complicated way. Clearly, the sensitivity of k to the timing of the measurements of temperature pairs that you allude to here, must be related to changes in η during the day. Therefore, k is not purely a soil property and I fail to see why you need to average it, thus losing the very real and highly relevance time dependence of this variable. Using an averaged value in models is likely to give poor results, particularly with the various non-linearities in the relevant relationships. This dependence of k on η even points to a possible alternative that is not at all considered in the paper: in analogue to the electrical conductivity, expressions may be developed (or perhaps already are available) for the heat conductivity as a function of moisture content: $\lambda(\eta)$. Together with eq. (1) for the heat capacity this would give sufficient information to describe temperature-gradient drive heat flow in soils, making the more elusive heat diffusivity superfluous. This approach could well be better suited to derive practically applicable modeling strategies for heat flow in soils with variable water contents.

Comments regarding the presentation

Please give dimensions when you explain variables on first occurrence.

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Explain the relation between thermal diffusivity and conductivity when you present them on p. 2249, l. 6. The current presentation suggests three parameters in the heat equation where there are two.

p. 2256, l. 2-3: Sentence does not run.

Please also note the [Supplement](#) to this comment.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 2247, 2009.

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