Response to Interactive comments on "Comparison of six algorithms to determine the soil thermal diffusivity at a site in the Loess Plateau of China"

Dear Dr. G. H. de Rooij (Referee)

We greatly appreciate your effort in reviewing our article. We have considered all of your comments in this revised manuscript. We greatly appreciate your helpful comments.

We respond to your comments item-by-item here. Our replies are in blue.

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 literature, 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?

Our comparison of algorithms was based upon measured soil temperature. Soil heat transfer is complex, involving conduction, sensible convection, and latent convection heat transfer processes. Each mechanism includes unknown, non-constant heat (and mass) transfer properties for a soil. Therefore, it is not possible to have an 'independent' or 'controlled' check, because for a given soil the properties are not known. The models that we evaluated use a simplification of the heat transfer processes by lumping conduction, sensible convection and latent convection into a single property – apparent thermal diffusivity. The purpose of apparent thermal diffusivity is to estimate temperature change with space and time. Therefore, the proper experiment to evaluate the apparent thermal diffusivity estimates is to determine how well estimated apparent thermal diffusivity can be used to estimate temperature with depth and time. This is what we did in the paper.

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 requires 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 limit 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.

We used representative field measurements to evaluate the algorithms. We think that the data set used is useful in evaluating the effectiveness of the algorithms to estimate apparent thermal diffusivity and calculate soil temperature changes. We focused on shallow soil, because heat transfer processes are more complex in shallow soil than in deeper soil. It is more difficult to describe shallow soil temperature than it is to describe deeper soil temperature. Algorithms that succeed in shallow soil will succeed in deeper soil.

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.

The paper has a focus on estimating apparent thermal diffusivity and soil temperature. A full evaluation of soil thermal properties is outside the scope of this paper.

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 evaporation 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.

All of the algorithms include conduction explicitly while only one algorithm explicitly includes sensible convection. However, all of the algorithms implicitly include convection. It is included in the apparent thermal diffusivity lumped parameter. It is included because conduction and convection both impact soil temperature. The apparent thermal diffusivity attempts to account for conduction and convection.

We appreciate your criticism and comments which are very helpful for our future work.

Minor comments

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

In this context, the soil surface stands for the skin of soil.

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.

We agree that the temperature profile in the root zone to be important for vegetation development, and observations at larger depths would be valuable for applications outside the realm of climate studies that we mention in the Introduction, but the purpose of this article is to improve the accurate knowledge of the soil temperature in the surface layer where weather is most affected.

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

We deleted it.

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.

Yes, but for clear day conditions that are nearly sinusoidal, the average temperature can be approximated from the maximum and minimum temperatures.

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

Yes, but it is important to elaborate this concept to make the paper more complete.

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.

Smoothing reduces amplitudes, but we are concerned about A_1/A_2. The reduction in both A_1 and A_2 does not influence A_1/A_2 much.

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

$$k = \frac{\lambda}{C_g} = \frac{\lambda(\eta)}{a + C_w \eta}$$

With $\lambda(\eta)$ an unknown, monotonically increasing function, and a equal to $(1-\eta_g)C_g$. If k peaks at some value of η (denoted η_{peak} peak), then its derivative must be zero there. This implies:

$$\frac{d\lambda}{d\eta}|_{\eta_{peak}}(a + C_w \eta) - C_w \lambda(\eta_{peak}) = 0$$

Rearranging, separation of variables, and integration gives:

$$\ln(c\lambda) = \ln\left(\frac{a}{C_w} + \eta\right)$$

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?

To fully analyze the equations, a careful laboratory study of thermal properties of several soils must be performed. These equations are really interesting and important for soil physics research, but the purpose of current work is to investigate apparent soil thermal diffusivity in order to estimate soil temperature changes. Our study is not designed to evaluate your equations.

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.

Soil heat transfer is very complex and involves several simultaneous mechanisms. For a complete review see Nassar, I. N., and R. Horton. 1997. Heat, water, and solute transfer in unsaturated porous media: Theory development and transport coefficient evaluation. Transp. Porous Media 27:-17-38. This article focuses on the simple apparent thermal diffusivity models for estimating soil temperature. The beauty of the simple models is in their utility.

Comments regarding the presentation

Please give dimensions when you explain variables on first occurrence. 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.

We revised it.

p. 2256, l. 2-3: Sentence does not run. Please also note the Supplement to this comment.

We revised it.

Best wishes.

Sincerely yours
Ling Wang, Robert Horton, and Zhiqiu Gao