**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 Anonymous Referee #2

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.

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General comments

- This paper is generally well written and the aims are clear and the equations mostly appear sound. However, there are several shortcomings of the paper. First of all, the methods discussed and compared in the paper are not new. Even their Conduction convection equation has been presented and compared before in other papers by the authors. It is good though to see all currently available equations together and compared.

The comparison of algorithms is new. The objective of this article is to compare six algorithms. Gao et al.(2008) only compared the conduction-convection equation to a thermal conduction equation for soil temperature prediction, but this article extends the comparison to six more algorithms.

- Secondly, the key approach used by the authors to assess the reliability and robustness of the methods used in their comparison is to compare measured and predicted soil temperatures (e.g. in Figs. 4-7). However, these methods are developed to derive thermal diffusivity, not temperature at depth z2 from temperature at depth z1. Whether this works well or not depends on the heterogeneity of soil with depth (the soil is assumed to be vertically homogeneous, but this is not necessarily the case, in fact it is very unlikely). So for me the only reliable proof of whether any method works better than another is to see thermal diffusivity plotted against soil moisture content (as a scatter graph, not a line graph as in Fig 3 !) for as wide a range of moisture contents as possible. The authors have 44 days, were only 7 of those suitable???

This work was limited to a soil surface layer (from 0.05 to 0.10 m depth) where the vertical heterogeneity is neglected. Because this 7-day period is representative of the clear days in the entire experimental period, we selected clear days from the period. Analytical methods have restrictions on the surface boundary conditions. Clear days best approximate sinusoidal variations.

-Only a comparison of the shape of the thermal diffusivity versus soil moisture content curves for all methods (where we are looking for thermal diffusivity to increase up to a certain moisture content and then go down again until saturation is reached) would lead to indisputable proof of which method is superior to the others (at least for this soil type and experimental set-up..). In my opinion the paper cannot be accepted until a graph like this has been presented.

We have reported earlier that the thermal diffusivity does not increase with increasing soil moisture when soil is wet (Figure 4a in Gao et al.(2008)). At this site located in an arid climate zone, the soil was never saturated during our experimental period, so we did not plot the thermal diffusivity versus soil moisture content.

Minor comments

- The units for heat capacity are not given on pages 2249/2251

We added them.

- The description of LOPEX at the end of the Introduction comes out of the blue. Also, Horton et al, Heusinkveld et al. and Verhoef et al could be mentioned here as studies were various methods were compared. The one including convection is relatively new, they need to say that more explicitly.

Yes. We improved this section by mentioning the work by Verhoef et al. The work by Horton et al. and Heusinkveld et al. were already mentioned. But, we want to maintain the LOPEX at the end of the Introduction because it tells readers what experimental data were used for testing.

- Line 10, p 2253: n=2 is not a boundary condition

We corrected it.

- You are using eta in Eq. 1 and theta in Eq. 21 for moisture content.

We corrected Eq.(1).

- Lines 2/3 page 2256 need to be grammatically improved.

We corrected it.

- The paper needs to make clearer that the harmonic equation will be referred to with HM and what the difference is between HM1 and HM2

We clarified section 2.1.5. The difference between HM1 and HM2 is that the thermal diffusivity $k$ was determined by the Least Squares Algorithm (Horton et al., 1983) in HM1 and by a different fitting method (Heusinkveld et al., 2004) in HM2.

- Is there something wrong with Eq. 21 or 22 (or the definition of W)? I can't see how they logically follow from each other

We changed Eq. (21).

- Page 2258, line 10: these differences don't necessarily mean anything due to differences in bulk density etc.

We deleted it.

- Why is T005 smoothed using a 2-hr algorithm? Is this necessary?

Because we set the boundary condition to be a sinusoidal function (Eq.(4)), we have to determine a sinusoidal curve which is closest to the curve of the measured soil temperature. In this way, we have to apply smoothing. We selected 2-hr to be the average period because the value of $A_1/A_2$ almost did not change after smoothing.

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- Fig. 3 should not have lines, this is a scatter relationship.

We deleted it.

- What is the Horton et al equation? Another Harmonic equation, I presume. This needs to be made clearer in the Methods section.

Horton et al. (1983) explained their Least Squares Algorithm in detail, but it is hard for us to express it in an equation. Same principle applies to the fitting method by Heusinkveld et al. (2004).

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Best wishes.

Sincerely yours

Ling Wang, Robert Horton, and Zhiqiu Gao