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Hydrology and Earth System Sciences Discussions

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.

Anonymous Referee #2

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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.

- 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???

-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.

Minor comments

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

- 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

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

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

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

- 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

- 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

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

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

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

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

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