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Hydrology and Earth System Sciences Discussions

Interactive comment on "Characterization of spatial heterogeneity of groundwater-stream water interactions using multiple depth streambed temperature measurements at the reach scale" *by* C. Schmidt et al.

Anonymous Referee #1

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General Comments

This is a nicely presented, well written case study using methods that are well established in the literature. The basic idea was presented by Lapham (1989), drawing on the work of Stallman (1965) and subsequently refined and used in many field studies by Jim Constantz and his group at the USGS (e.g., see the collection of case studies edited by Stonestrom and Constantz 2003), and others. Specifically, this paper is an extension of the kind of work presented by Conant (2004). (Reference citations to the works cited above are given in the present paper.) The authors need to clarify the

nature of their contribution and make clear that they are presenting an application that involves the use of well established methods.

Specific comments

1. The introduction is misleading as the authors imply they are introducing a new method for studying groundwater-stream interaction when in fact this method is well established in the literature. (See my general comments above.) Later, in Section 2, the authors refer to these earlier studies and acknowledge that temperature profiles have been used before in stream studies but they don't make clear that these studies use essentially the same methodology as in the present paper. They suggest that their work is an extension of the work of Conant (2004) and say that unlike Conant, they measured temperature at several depths. This is essentially a correct statement but a little misleading. Conant also measured temperature at depth (see his Figure 6) in order to select the most representative depth (0.2 m) to take the rest of his measurements. The authors also do not make clear that because they have measured temperature profiles, they can calculate velocity from the temperature measurements. Conant was not able to do this since he generally measured temperature at only one depth. So, this aspect of the present work is indeed a very nice extension of Conant's research.

2. Thermal conductivity of the streambed sediments was taken from general literature values. It is possible to measure thermal K in situ using a thermal conductivity probe (Sophocleus 1979, Soil Sci. Soc. of Amer. Jrnl.).

3. In calculating velocity, the authors used an average temperature for the stream in the Bredehoeft and Papadopulos (1965) solution. The Stallman (1965) solution allows the use of temporal sinusoidal temperature fluctuations at the upper boundary. Silliman et al. (1995), for example, used a modified version of the Stallman solution to calculate stream fluxes based on temperature measurements. Moreover, the observed temperature fluctuation in the stream could have been incorporated if a numerical solution had

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been used, following the work of Lapham (1989) and Stonestrom and Constantz (2003) and others. Although the authors state in section 6.7 that their focus is on spatial and not temporal variability, it would be interesting to know if there are diurnal differences in fluxes driven by stream temperature as Constantz et al. (WRR, V. 30, 1994) found.

4. In section 6.7, what, specifically, is the basis for the conclusion that there must be a 5-10 degree C temperature difference between stream water and groundwater?

5. In Section 6.7, the velocity constraints are addressed qualitatively. Others have addressed the velocity constraints quantitatively and the authors should cite their findings (e.g. Lapham 1989).

6. The authors conclude that although the channel they studied has relatively homogeneous sediments, there is considerable heterogeneity in fluxes. Earlier in the paper, the authors list several possible explanations for their finding of upward flow in the streambed where piezometers indicate downward flow. They conclude this is due to "streambed roughness induced by single gravel grains". But this presumably does not explain the heterogeneity in discharge fluxes. I was surprised the authors conclude that there is a lot of heterogeneity because earlier (top of p. 1430) they make the point that the spatial variation in fluxes observed at this site is less than in natural river channels such as the one studied by Conant (2004).

Technical corrections

Beginning of Section 2. "fluid phase of the sediments" should be changed. This is not what you mean to say.

Bottom of p. 1422. I'd reference Stallman (1965) instead of, or in addition to, Stallman (1963). Also, same placeĚ Papadopulos is misspelled.

"river Elbe" and similar constructions for other rivers probably should be "Elbe River".

Instead of "artificial river" and "artificial channel", I'd say "man-made channel".

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The processes in the heat transport equation are conduction and advection. So, I would not refer to this equation as the “heat advection-diffusion equation” as is done throughout the paper.

Top of p. 1425. Need to refer to a figure when citing piezometer numbers.

First sentence Section 6.5. Temperature is given in degrees Kelvin. Use centigrade as elsewhere in the paper.

First sentence in section 6.7. “were” should be “where”

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 1419, 2006.