Hydrol. Earth Syst. Sci. Discuss., 8, C2867–C2872, 2011
www.hydrol-earth-syst-sci-discuss.net/8/C2867/2011/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Heat transport of diurnal temperature oscillations upon river-water infiltration investigated by fiber-optic high-resolution temperature profiling" *by* T. Vogt et al.

Anonymous Referee #2

Received and published: 19 July 2011

General Comments

I would like to preface this overarching comment with the statement that I believe the high-resolution temperature data set showing the "S-shaped" patterns of heat along vertical transects is unprecedented, and could likely only have been collected using DTS in this kind of innovative custom setup. These patterns may potentially be attributed to differential rates of horizontal seepage from the river through the shallow aquifer, which would be interesting to describe with vertical temperature profiles. That

being said, the paper does not seem to have a clear goal or direction from the outset. What initially seems to start as a quantitative investigation of seepage rates seems to change to a qualitative analysis of these patterns combined with some very simplified 2-D heat transport modeling. In the introduction it is stated that "Because high-frequency temperature fluctuations are lost due to strong dampening within a few meters of travel distance, the travel time from a losing river to a near-by pumping well may be inferred from the seasonal rather than diurnal temper25 ature signal (Vogt et al., 2009)." for which the first author is cited. Further, the Molina-Giraldo et al. (2011) paper is also referenced when discussing the transport of seasonal signals laterally from the river. It is unclear why the jump is made from seasonal data to using the diurnal signal at high spatial resolution in the vertical as an appropriate data set to study a system where water is expected to be moving laterally from the river through the riparian zone and shallow aquifer. How is this an improvement over using the seasonal signal for which the high spatial and temporal DTS resolution may not be necessary? More specifically why would the authors use a setup geared for studying vertical exchange between the river and hyporheic zone in the lateral shallow aquifer where seepage is expected to be generally horizontal, not vertical, as explicitly shown in the Figure 2 cartoon? The title of the paper seems to give the impression that the goal of this study was to quantitatively describe the river water infiltration, yet near the end of the introduction it is stated: "Our main hypothesis is that thermal exchange with the unsaturated zone effects the travel-time distribution of the diurnal temperature signal in shallow riparian groundwater. To test the hypothesis, we use time series of three high-resolution fiberoptic temperature profiles (vertical resolution=5 mm) to identify spatial patterns of heat transport in the river bed and the riparian zone upon river-water infiltration." If the main goal is to investigate heat exchange between shallow GW and the unsaturated zone this should be more explicitly discussed in the introduction with a more thorough review of previous work, and probably included in the title of the paper. This discrepancy is further shown by the 2-D spectral finite element model used by the authors to reproduce the general patterns seen in the vertical profiles, specifically

the shift of the diurnal signal in the time domain. This model is setup specifically with left to right horizontal flow not vertical seepage. The goals of the study seem best described in the first paragraph of the discussion, but call into question why the section "2.2 Analytical solution of one-dimensional heat transport equation" is included in the paper, if not applicable to lateral transport and not a part of the expressed study goals. The paper finally feels like it begins to find its way during the first section of the discussion. The description of the interference of the two signals, one carried from the river via advection and one propagated by conduction through the unsaturated zone is very interesting and novel. But of course even though these signals both originated at the land/river surface from diurnal heating they may be days out of phase from one another, or whatever the residence time from the streambed to the bank vertical DTS profile along the lateral flowpath(s) is. Therefore I believe presenting 1-D time-shift and amplitude plots (e.g. figures 5 C,D) of two signals along the same line is inappropriate. Further discussion of heat transfer from the GW signal into the unsaturated zone is interesting, but seems primarily based on the numerical model not the field data. In summary I believe this paper has some very interesting and well worked sections, but suffers from a bit of an identity crisis. If the goals of the study, evaluating lateral transport from the river through the shallow aquifer and exchange of heat with the unsaturated zone, are more methodically presented from the beginning, the paper would be much easier to follow. I anticipate this would take more explanation of why the high-resolution temperature profiles are appropriate for this study, and a reduction of the vertical 1-D flux presentation. Additionally I believe a "tighter" discussion of the S-shaped temp profiles is needed, and may take further numerical modeling based more strongly on the unique temperature records to explain. This manuscript could be an important addition to the field with some further work and clarification.

Specific Comments

1. At line 20 page 4 it is unclear why the line "An additional lag time should be accounted for sensors not placed in a screened interval due to thermal skin effects (Car-

denas, 2010)." is included, these seems better suited for the discussion section or not at all. In the same section line 25: "Laser pulses are injected into and backscattered along the fiber." is unclear and should be reworked.

2. Line 6 page 5: "Su′arez et al. (2011) found that DTS systems connected to wrapped optical fibers resolve temperatures with very small variability compared to traditional temperature sensors that may have less noise, but may not reach the high spatial resolution." it is unclear what "very small variability" refers to. I assume the authors mean variability between individual sensors that is due to differential drift/calibration, not noise? As stated DTS systems can have much higher noise, and therefore "variability" of the temperature signal.

3. Line 15, page 5: "The objective of this study is to investigate local heat transport upon river-water infiltration in the streambed and the riparian zone of the losing River Thur in northeast Switzerland." does not make sense to me. Do you mean use local heat transport to study water infiltration?

4. For section 2.1 the transition into the mathematical theory is quite abrupt, a sentence or two explaining why these specific heat transport equations are being presented is necessary- e.g. I assume these solutions are more applicable to anisotropic porous media, and therefore the field site in question, then the more commonly used 1-D models but this is not explained.

5. After reading the paper through it is actually unclear why this section is even necessary. The 1-D fluxes are not calculated from this dataset for any DTS profile except the one in the streambed, and this information, although assuring that it corroborates the 2010 investigation, seems tangential to this paper at best. The only reference to 1-D vertical flux calculations in the results section is: "The calculated seepage rates range between 0.8–3.0$\times$10$-$5 ms$-$1 and agree well with the results of Vogt et al. (2010b). In the following, we focus on the shoreline and the bank where an unsaturated zone exists and horizontal flow is dominant." which specifically states the 1-D seepage calculations

are not the focus of this work. This is later stated in the paper: “When the top sediments vary between flooded and dry, like at the shoreline, the flow direction is changing from vertical to horizontal. Hence, 1-D analytical solutions cannot be applied.”

6. Similar to my comment #5, Being quite familiar with the 2010 Vogt paper I understand why you are going through this development and why it is important to determining flux from noisy field records and at high temporal resolution (eg not just the daily max and min), but more text is needed to convey this to the reader.

7. For the 2-D heat transport model are the heat properties of the overlying clay aquitard specifically included? What about increasing water content close to the boundary with the water table due to capillary action? The more water in the soil matrix the higher the heat capacity which can have a relatively large influence on heat conduction.

8. Page 11 line 17: Why not calculate the theoretical precision of the specific setup used here with the available software (PerfectCalc 2.0) from the manufacturer? Using this software, estimating an attenuation rate, and assuming the firmware onboard the DTS was 3.3 or newer, I get a precision of 0.115 K at the end of 2 km of fiber.

9. Page 11 line 20: Impressive you were able to maintain an icebath for 22 days! Did you keep the bath mixed in some way or was it always packed top to bottom with ice? We have had some problems with stratification of such baths.

10. Page 12 the statement: “We installed the three high-resolution temperature profilers in the riverbed, at the shoreline, and in the riparian bank along a presumed subsurface flow path (Fig. 2).” makes it sound like there were 9 sensors installed in total.

11. Page 13 line 1: Why not use the observed temperature signal as the boundary condition at the land surface? You seem to have the data to easily so this, and would seem to make more sense as you use the observed signal from the bank profile as the left hand boundary condition.

12. How did you evaluate the DTS “noise range” (stated as 0.02–0.2 K)? Was this some high frequency component of the signal identified during the DHR process? Standard deviation of the ice bath?

13. It seems clear through your conceptual domain that there is essentially an integration of two different diurnal signals at the unsaturated zone/GW interface. One conducted from above and one advected from the river. In that case does it make sense to use DHR on this portion of the temp record where they “interfere” as stated in line 6 page 15? This is why plots 5 C,D do not make sense to me. You are essentially plotting the 1-D time shift of two different signals on the same line, connecting them in the zone of interference at the top of the water table.

14. page 12, line 17: “For investigations of river-water infiltration in the hyporheic and riparian zone, the diurnal temperature signal is usually used.” Hyporheic seepage that is expected to be vertical the diurnal is used yes, but lateral GW transport through the bank away from the river the authors note the seasonal signal is usually used.

15. page 21 “In particular, the s-shaped vertical profile of groundwater temperature time shift could be attributed to two different factors. One is the retardation of the temperature signal in shallow groundwater due to heat exchange into the unsaturated zone and the other is vertical variation of the horizontal groundwater flow velocities.” Could this s-shaped vertical profile not simply be caused by the integration/interference of two signals, one propagated horizontally and one vertically?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 6257, 2011.