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.

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Dear Editor,

hereby we submit our comments that address how we think to handle the comments of the three reviewers. This is not a complete revision, because the ongoing vacation period hindered the complete revision of the manuscript "http://www.hydrol-earth-syst-sci-discuss.net/8/6257/2011/hessd-8-6257-2011.pdf" by T. Vogt, M. Schirmer and O.A.

Cirpka. We will assign a new title. Our current favorite title is: Investigating shallow riparian groundwater flow at a losing stream by means of diurnal temperature oscillations. As soon as possible we will submit a complete revision of the manuscript. We thank the reviewers for their constructive comments which will help to improve the paper. In the following, we discuss our responses to the individual comments that we plan to do. Page and line numbers in our comments refer to the system that the reviewers have used.

Anonymous Referee #1 The main hypothesis of the paper is that the unsaturated zone influences the diurnal temperature signal. Field data is presented, and a 2d numerical model is constructed to quantify the influence of the unsaturated zone on temperature dynamics. While the paper discusses important subjects, I think it requires a significant amount of work that probably exceeds a major revision. My points of concern are:

- The conceptual model to interpret and model the data seems to be incomplete. 900 mm of rainfall will certainly influence the heat transport in the unsaturated zone but this component is not considered. Also, oscillations of the river will cause water to infiltrate to the bank and re-enter the stream as the water table falls, but these fluxes are not discussed.

AC: We discussed on p. 19 l. 21-23 the influence of infiltration/recharge through the unsaturated zone, because we simulated the temperature distribution with several infiltration/recharge rates applied to the top op the model. As stated on p. 19 l. 21-23 the strong attenuation of this signal in the upper part of the unsaturated zone can be explained by pure conduction without infiltration/recharge in the unsaturated zone. For the observation period presented in the paper, oscillations of the river causing water to infiltrate to the bank and re-enter the stream as the water table falls is not relevant, because this was not the case for the bank (see Fig. 4B). Effects of river-stage fluctuations on heat transport in groundwater are discussed on p. 20 l. 25-29.

- The goal of the model is unclear. It incorporates some measurements from the field-

site (the temperature of the river), but then assumes a static water table and is based on (hard to defend) assumptions of the velocity profile. As a tool to understand the field data, this model is not suited, given the large amount of (not discussed) simplifications. If it is constructed to understand the general influence of the unsaturated zone, a much more detailed analysis should be carried out, e.g. in the form of a sensitivity analysis and a subsequent interpretation of the results.

AC:The goal of the model is to get a better understanding of the observed heat transport processes in the riparian zone (p.2, l. 16-18) and to demonstrate how transport of diurnal temperature signals within shallow groundwater in the river bank is affected by heat exchange with the unsaturated zone (p. 5, l. 26-28). We agree that a quantitative sensitivity analysis would be nice, but is out of the scope of this paper. Of course the model is simplified like most models in complex hyporheic/riparian settings are. As the aim of the paper is not a pure modeling study, we believe the model is detailed enough to get a better understanding and an estimation of groundwater flow velocities. Also the model set-up reflects the field-conditions (0.5 m vadose zone). In addition, the input of the left model boundary is not the temperature of the river (as Referee 1 stated), but the amplitude and time-shift profiles of the high-resolution temperature profiler installed at the shoreline. As we simulate temperature distributions for stable conditions namely for 22.09.2010, a static water table is a simplified but justifiable boundary condition.

- I find the assumptions on flow velocities (uniform and depth-varying) not satisfactory. It is easy to calculate more realistic flow distributions, so why make such an assumption?

AC: Without detailed knowledge of the hydraulic conductivity distribution in shallow riparian groundwater, uniform flow velocities are a common starting assumption. The depth-varying velocity profile is calibrated/adjusted to get a good match with the observed temperature profiles. So we do not understand the criticism.

- The paper states that the unsaturated zone has an influence on the heat exchange.

C4042

While I readily believe this, I would like to know how large this influence is, and what the controlling parameters are. When does the influence of the unsaturated zone undermine the available approaches (not considering the unsat zone) used to interpret such data? This question could be answered with a systematic variation of the parameters in a numerical model. I think if the authors can provide a framework on the importance of the unsaturated zone and what the sensitivity to the relevant hydrological parameters is, the paper will make an important contribution. Given that this will still require a significant amount of work, the paper should be returned to the authors for rewriting. The authors have written highly impactful and quality papers previously, and clearly have the tools and skills required to bring this paper to the same high level.

AC: This is a good point that is worthwhile to be followed up. However, for this paper we want to focus on groundwater flow. The quantitative importance of the unsaturated zone might be the focus of future work.

Other comments: - The title can be much more specific

AC: We will assign a new title (see above).

- The difference to the Molinea-Giraldo paper should be stated more explicitly

AC: The difference to the Molina-Giraldo paper was stated already on p.6 l. 2-3, but we will amend the difference in the text somewhat more. While they did a pure modeling study focusing on the seasonal signal, we combine field measurements and modeling focusing on the diurnal signal and the riparian zone close to the river.

- The paper was hard to read. For example, it was not clear for a long time whether the authors talk about the unsaturated zone in the bank or under the river. The use of the term sediments for the river bank further added to the confusion. In Figure 2 3 DTS are shown, but then in Figure 4, 4sets of data are shown. Is the third one split up into two?

AC: We do not agree with the points mentioned here. On p. 14 l. 6-10 we state that

C4043

the pole in the riverbed is analyzed, but that we focus on the two poles at the shoreline and in the bank. We also believe that figure 4 and its caption are clear enough to understand that figure 4 shows the poles at the shoreline (A) and in the bank (B) and that C and D show magnified/zoomed in sections of A and B. The term sediments in C helps the reader to understand that only the sediments are shown here and that air temperature is not shown (see black rectangle in A).

- Is there an upper temperature boundary representing the atmosphere, or is the exchange only one way (from above) as indicated in figure 3?

AC: As state on p. 13 l. 1-2 on the top of the model/unsaturated zone the temperature oscillations and no exchange fluxes are specified as boundary conditions.

Anonymous Referee #2

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 highfrequency 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 temperature 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 us-

ing 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 highresolution 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.

AC: The points of the review are well taken, we will focus on diurnal temperature oscillations as tracer for the determination of groundwater flow and move away from heat exchange.

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 (Cardenas, 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.

AC: We will modify the sentence about the DTS into: "The DTS control unit sends laser pulses into the optical fiber and the backscattered light is detected and analyzed." However, we do not agree with the reviewer statement concerning the "additional lag time", because we believe the paper of Cardenas (2010) deserves attention in an introduction about temperature measuring techniques in groundwater.

2. Line 6 page 5: "Suarez 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.

AC: Suarez et al (2010) refer to standard deviations and spatial repeatability when they talk about variability of temperature measurements. We use here the same terminology and will amend this sentence.

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?

AC: We will clarify this.

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.

AC: We do not agree, because we stated already in the introduction (p.3 l. 16-17) that quasi-transient analytical solutions of one-dimensional (1-D) heat transport with sinusoidal fluctuations of heat-input on top and constant groundwater temperature at bottom are most commonly applied. In addition, we believe that one needs the basic heat transport equation (eq. 1) to understand how to reach the 1-D model.

5. After reading the paper through it is actually unclear why this section is even neces-

sary. 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."

AC: We still believe this section is necessary but will modify it.

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.

AC: See reply of 5. above.

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.

AC: For heat conduction the heat capacity is not relevant, but thermal conductivity is differing with varying water content. We assume that the 0.5 m thick layer of alluvial fines (not clay) have the same mineralogy as the aquifer, which is a realistic assumption. Therefore, the thermal sediment properties are the same for the aquifer and the vadose zone. Of course we included the capillary fringe into the model and assigned for that the van Genuchten parameters for sandy loam after Carsel and Parrish (1988)

as stated already in Tab. 1. However, it turned out in our model that the capillary fringe has a minor impact on the thermal exchange.

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.

AC: Thanks for the suggestion. We did not know the software before. We will modify the sentence to "The theoretical temperature resolution of this specific set-up is 0.18 K based on the software PerfCalc 2.0 (AP Sensing GmbH)."

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.

AC: As long as the cable is crossing the different "temperature layers" in the icebath and you monitor temperature with other temperature sensors, it is not a big deal.

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.

AC: We will modify this section to: "One in the riverbed, one at the shoreline, and one in the riparian bank..."

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.

AC: We will try to look into this issue more closely.

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?

AC: We will add "standard deviations 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.

AC: In river-groundwater interaction field studies using heat as tracer this is the standard set-up. The river signal is input and groundwater signal is output signal. So the river signal is always the reference. Therefore, we choose a 1-D analysis related to the river signal here. In addition, one need to know the time shift between river and air signal for the modeling.

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.

AC: The banks where fresh river infiltrate is flowing horizontally can also be part of the hyporheic zone when it is returning into the river. Close to the river you will find (as we did) diurnal temperature oscillations in horizontally flowing groundwater. If one finds diurnal temperature oscillations in horizontally flowing groundwater, the advantage compare to the seasonal signal is the shorter measurement time covering several cycles, which would be several years for the seasonal signal. In addition the major nutrient cycling happens close to the river and not 50m away where only the seasonal signal is detectable.

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?

AC: No, because the attenuation of the diurnal oscillations entering the unsat. zone from top is too strong to reach groundwater. That is by the way also one difference to the Molina-Giraldo paper, because their saesonal air temperature signal penetrates through the entire (in their case 2m thick) unsat. zone into groundwater.

J. Constantz (Referee) jconstan@usgs.gov Received and published: 28 July 2011 The paper has a novel instrument/design approach for measuring vertical temperatures in the streambed and bank, and interesting results as well; however, there are several areas that could be improved with minor modifications and expansions of the text. First, as indicated by the title, the researchers state that they are investigating heat transport using the fiber-optic technique. Actually, I believe they are more interested in heat as a tracer to examine water fluxes, so I suggest changing the title to reflect this and carefully reviewing the text for this same issue.

AC: We agree with the reviewer, we will focus on diurnal temperature oscillations as tracer for the determination of groundwater flow and move away from heat exchange. (See also reply to general comment of reviewer 2).

Second, the very tight spatial resolution of the coiled wire does not seem to be necessary for the level of analysis, so this should be discussed in some detail, i.e., would evaluating the data at a smaller spatial intervals provide more information or not.

AC: As referee#2 stated the high-resolution temperature data showing the "S-shaped" patterns along vertical transects is likely only possible using DTS in this kind of setup. We discussed the same on p. 6277 l. l27 - p. 6278 l. 1. With decreased resolution

uncertainty is increasing. A quantitative uncertainty assessment would be nice, but is out of the scope of this paper.

Page 6261, the authors state their main hypothesis but not reasons for suspecting this hypothesis.

AC: We will work on this.

Page 6266, is the aquifer confined or unconfined?

AC: We will add: "During low flow the aquifer is largely unconfined, but turns confined during higher river stage."

Page 6268, the boundary conditions are not specified.

AC: We will add: "...by means of fixed head boundary conditions."

Page 6270, the calculated seepage range should include the depth for this range as well.

AC: We will look into this issue.

Page 6273, how do applied velocities correlate with deduced velocities?

AC: We will add: "These velocities are slightly higher than the results of the one-dimensional analytical expression used to estimate the apparent advective velocities v_T shown in Chapter 2.3, where we estimated for the pole in the bank a velocity profile of 0.35 to 0.45 × 10-4 m/s in 0.7 and 1.4 m depth and fastest velocities with 0.55 to 0.60 × 10-4 m/s in 0.9 - 1.0 m depth."

Page 6274, text related to Fig. 6d needs to show depth and horizontal distances.

AC: As already written in the text, the horizontal distance (2.5m) is the same for Fig. 6d. However, we will amend the text concerning the depth.

Page 6276, the sentence starting with 'Given a shallow unsaturated zone' is very difficult to follow.

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AC: We will modify this section.

Page 6277, again some discussion of the applied vs. deduced velocities is warranted.

AC: We will add: "Compared to a rough estimation for the pole in the bank based on the 1-D analytical expression, the simulated flow velocities are two times higher. The range of both simulated and field data based flow velocities with 0.35 to 1.04 × 10-4 m/s is typical for pre-alpine gravel aquifers."

Page 6277, Regarding the statement 'model does not exactly reproduce the spatiotemporal temperature distribution', has this evidence been shown earlier?

AC: Yes on p. 6274 l. 10-13: "The model reproduces the general trend of linear increase with depth in the unsaturated zone and vertical time-shift variations in groundwater. But the vertical variations of the measured groundwater data are stronger."

Page 6278, The results seem to indicate that the model can reproduce the flow system, but there is some concern that the flow system is still not well understood.

AC: We do not understand to which text section the referee refers. We use a periodic heat signal as natural tracer. Therefore it is impossible to understand the flow system completely. We only can determine travel times and attenuation of the diurnal temperature signal, but exact flow path remains unknown as we state on p. 6271 l. 24-28. That is why we present in the result section only travel times and no flow velocities or streamlines.

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

C4053