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Hydrology and Earth System Sciences Discussions

Interactive comment on "Sand box experiments to evaluate the influence of subsurface temperature probe design on temperature based water flux calculation" *by* M. Munz et al.

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The authors would like to thank Christian Anibas for his detailed review and constructive comments on the initial manuscript. He raised a number of interesting questions and made helpful suggestions to which we respond below.

General comments:

In respect of the reviewers major remark we will remove the calculated Darcian flow velocities based on piezometer probe measured temperatures from figure (6). We agree that the table (3) is meaningful enough to show the big differences between Sediment

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Probes and PBS-PCS. In the text we will clearly state that “a quantitative flux calculation based on Piezometer Probe-derived diurnal amplitude ratios and time lags is not possible when temperature gradients within the piezometer pipes were diminished by the onset of free convection.” (at P 6176-L3). We will present more results of calculated Darcian flow velocities of the MLTS probe to focus on the performance of the new MLTS instrument, as these results are regarded to be of particular interest for potential readers of this article. Therefore we will add the calculated fluxes of the sensor pair0.365-0.015 and sensor pair0.165-0.065 of the MLTS to figure (6), involving some changes of the referencing in section 3.5. in the revised version of the manuscript. Furthermore we will follow the suggestion of the reviewer to call the whole instruments “probe” (Sediment Probe and Piezometer Probe as described in section 2.3.) and to refer single temperature sensors as “sensors”. The term “probe pair” will be changed to “sensor pair” in the whole manuscript.

In the following, we will discuss the specific comments.

Specific Comments:

P 6158-L 14: You should mention which method, Keery and Hatch, or just one of them, you applied.

Authors response: Flow velocities were calculated using the analytical method of Keery. That will be clarified in the corresponding sentence in the revised version as: “Measured temperatures were analyzed using the time series method introduced by Keery et al. (2007)”

P 6159-L 15, Eq. 1: In Keery et al. 2007 the formula uses the effective thermal conductivity _e as parameter.

Authors response: The formula will be changed using the effective thermal conductivity as in Keery et al. (2007) to avoid any confusion. For consistency it will also be changed in Eq.(2).

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As the thermal dispersivity was set to 0 for the corresponding analyses, effective and baseline thermal conductivity would be equal (cp. Eq. (3)) and formula (1) could be changed without any changes within the analyses and result section. The potential error in neglecting thermal dispersivity was evaluated for all experimental fluxes in section 3.6..

P 6162-L 23 et seqq.: You should consider to move the whole paragraph into the results section.

Authors response: The whole paragraph will be shifted to the result and discussion section at the end of section 3.1.. A reference to table (4) will be added as this table presents corresponding results. All values of the table, like ne and ksat will be referred in the text. To highlight the data important for the analytical solution and used for most following analyses the authors decided to keep table 4. Numbering and order of tables will be rearranged. Former table 4 will become table 1. Therefor all other tables need renumbering. This will be implemented for the revised version of the manuscript.

P 6165-L 24: In general I would call the whole instrument 'probe' and not just a single sensor. Here, 'Each profile probe setup, having four temperature sensors: : :.'.

Authors response: Cp. The authors general comments.

P 6164-L 3: Describe which kind of temperature sensors are used in the MLTS. Are they themistors like for the TidBiTs, or do they have another working principle (for example resistance thermometers).

Authors response: Temperature sensors used in the MLTS are TSIC-506 sensors. The IC sensors are based on a semiconductor resistor embedded in an integrated circuit for conversion to a linear electrical output. The type of temperature sensor and the corresponding working principle will be specified in the revised version of the manuscript.

P 6166-L 7-11: Reformulate the paragraph. The content is correct, but I find the for-

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mulation intricate.

Authors response: The paragraph will be reformulated for the revised version of the manuscrpt as follows: “To calculate vertical flow velocities based on Eqs. (1) and (2) thermal properties of the saturated sediment need to be determined. As these values are difficult to measure outside the laboratory (Stonestrom and Blasch, 2003) we used literature based values and additionally refined them by calibration.”

P 6166-L 14: I see that you estimated first the thermal properties of the sediment based on literature values and volume-weighted sums of the parameters of the constituents. For the flux calculation you used calibrated thermal properties. It is however not clear for me why it was necessary to calibrate the estimated values? This should be clarified in the document. The calibrated values, which are important, are not mentioned in Table 2. They should thus be added there.

Authors response: It is not necessary to calibrate thermal properties; but as the experimental data provide the possibility to partly calibrate these values we wanted to show how that could be done based on Eq. (1) and how it changed the literature based assumptions which are used in most current studies dealing with heat transport. As volumetric heat capacity and thermal conductivity appear as quotient (reciprocal of thermal diffusivity) in Eq. (1) they are dependent on each other and thus it was not possible to calibrate explicit values of both, heat capacity and thermal conductivity. Therefore we needed the initial assumption of the volumetric heat capacity. Showing the difference between literature based assumptions and calibrated values of thermal conductivity and thermal diffusivity we clarified potential inaccuracies introduced to our analysis by using literature based values only. To make this point more clear, we will rework that section of the manuscript. Cp. Authors response to P 6166-L 22.

The calibrated values will be added as additional line to table (2) for the revised version of the manuscript.

P 6166-L 22: In connection with the former question, what do you mean with ‘calculated

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flow velocities and zero'. This sentence should be clarified.

Authors response: In order to clarify the whole section P 6166-L 7-11 (see above) and P 6166-L 19-25 will be reworked and reformulate these parts for the revised version as follows:

"The literature based thermal properties were refined during calibration. The calibration was restricted to experimental data for no flow condition, e.g. delta h = 0 and vertical flow velocity = 0 as there will be no uncertainties introduced to the calibration procedure due to uncertainties of experimentally measured water flux. As volumetric heat capacity and thermal conductivity appear as quotient; reciprocal of thermal diffusivity in Eq. (1); they are dependent of each other. Therefore the thermal conductivity; as it is seen to be more uncertain in its literature based assumption; was calibrated exclusively in order to minimize the root mean square error (RMSE) between observed and calculated vertical water flux. Calibrated thermal properties were used for all flux calculations."

P 6168-L 18: I don't understand the use of a 5% confidence interval here. Shouldn't it be 95%? If not please explain this in the text. When I look at table 1 I see that differences in averaged q seem to be significant for all _h. When I have the averaged q of _h = 0.008 which is -0.48 and I subtract 0.06 from the confidence intervals the value is -0.54. For _h = 0.013 the respective value is -0.64 then, still much higher then the former value. Can you therefore clarify your statement?

Authors response: It should be the 95% confidence interval. Checking the data within Table 2 confirms that differences in averaged q are significant for all delta h. This will be corrected in the revised version of the manuscript.

P 6168-L 25: You explained the use of Rayleigh numbers for your work, but I miss a similar explanation for the published values (Tab.1) of the Peclet numbers in the text

Authors response: We think that the Peclet number is adequately explained in the text;

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cp. P 6161-L 3-6: “The Peclet number is the ratio of energy transported by advection to the energy transported by conduction (Domenico and Schwartz, 1990). Peclet numbers greater than one indicates that advective heat transport dominates over conductive heat transport.”

P 6169-L 19-20: In the text you cite sensor pair0.165-0.065 as Fig. 2b and pair0.365-0.015 as Fig.2c whereas in Fig.2 it is actually the opposite. I think that figure 2 should be changed according to the order mentioned in the text.

Authors response: We decided to keep the current figure (2) and to change the order of figure reference in the text as the same order of subfigures is shown in figure 3.

P 6170-L 3: Can you specify what ‘higher downward fluxes’ are? A range of _h seems to be more useful than the vague statement _h <-0.026m.

Authors response: We will remove the vague statement of delta h smaller than 0.026 m and will add the phrase “than experimentally generated” to the corresponding sentence. We think that this should clarify what is meant by using the term “higher downward fluxes”.

P 6170-L 12: Please mention that pair0.365-0.015 and pair0.165-0.065 are shown in Fig. 3c and Fig.3b respectively.

Authors response: The figure reference will be mentioned in the text for the revised version of the manuscript.

P 6170-L 20: Check and eventually reformulate the sentence ‘Thus, higher probe distances: : :: : :significant to hydraulic settings’.

Authors response: Sentence will be changed to: “Thus, higher sensor spacing lead to higher time lags and to significant different time lags for a wider range of hydraulic head settings.”

P 6172-L 2: I think this must be ‘the results will be affected by thermal skin effects’.

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Authors response: Thermal skin effect: An additional time lag and dampening will be introduced to the temperature signal measured by a single sensor due to the material surrounding the sensor. As each sensor of each temperature probe design would be affected in the same manner any effects arised due to thermal skin effects would be cleared if differences or quotients of sensor pairs are used for interpretation of measured temperatures. We will rephrase the paragraph to clarify that statement (P 6172-L1-4).

P 6172-L 7: I suggest adding the unit of the RMSE value; degreeC and h respectively. P 6172-L 10: I suggest adding the unit of the RMSE value; degreeC and h respectively, as it is also done later in the document.

Authors response: The units will be added according to the reviewers suggestion.

P 6174-L 9-27: [. . .]Is it possible to distinguish amplitude ratios from 'good' times (11am and 11pm) in contrast to 'bad' times (11pm and 11am) in Fig. 4b and c? I could imagine that lesser dampened values, grouped together at the lower left side of Fig 4b and c indicate such 'good' times. It seems however that the signals at the sediment probes are less dampened. See also my comment on Fig. 4. In the text however you should clearly state that a quantitative flux calculation on a diurnal bases with the PBS and the PCS probes is not possible under the described circumstances. P 6175-L 26 et seqq.: Is, beside the fact that both are not practicable, the PCS or the PBS approach better? In principle I imagine that PCS has less problems with the instability of the water column in the pipe since it has a better thermal and hydraulic connection with the sediment. When I compare Fig. 4 b and c it however looks that the values of PCS are even more spread. What can be the explanation? Also Fig. 4 e and f show similar differences.

Authors response: The experimental design clearly outlines this question. Planning the experimental design, we thought that there will be detectable differences between PCS and PBS as supposed by the reviewer. But such differences could not be proven. Both,

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the PCS and PBS approach reveal comparable results. Differences between figure 4 b and c and between figure 4 e and f were caused by different times of temperature measurements within the bottom screened and complete screened piezometer pipes. The TidbiT logger chain was switched between the PBS and PCS after half of the time of equal hydraulic gradient due to a limited number of loggers available (cp. P 6165-L 1). Both temperature probe designs, PCS and PBS, were compared using the additionally installed TidbiT temperature logger suspended at a depth of 0.165 m in the piezometer actually not instrumented with the complete logger chain. (cp. 6165-L 24 – 6166-L 2).

P 6175-L 16: Add the unit of RMSE. P 6176-L 10-11: Add units of RMSE values.

Authors response: Units will be added.

P 6176-L 22-24: The sentence 'The calibration of thermal properties: : :: : :' should be reformulated. For me it is not clear what is meant with the RMSE between calculated flow velocities and q=0. Since you use md-1 as unit for fluxes in the whole document, I suggest use the same unit in this sentence as well.

Authors response: The values will be converted to m per day. The unclear part of the sentence was removed as this will be addressed more exclusively in the methods section. Cp. Authors comment to P 6166-L 22.

P 6178-L 23 et seqq.: I suggest to cancel the graphical presentation of fluxes calculated from PBS and PCS in Fig. 6. I think table 3 is meaningful enough to show the big differences between sediment probes and PBS-PCS. In Fig. 6 therefore I would present more results for the MLTS probe, since it is a promising new instrument. Beside the measured fluxes and results of the sensor pair0.165- 0.065 from the sediment probe and MLTS, calculated fluxes for sensor pair0.365-0.015 and sensor pair0.165-0.065 of the MLTS could be added. This would show the performance of the new MLTS instrument and the dependence of the flux result regarding sensor depth and distance more graphically.

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Authors response: cp. The authors general comments

P 6180-L 21-24: I recommend reformulating the paragraph. It's not clear what exactly you want to say with. I also suggest writing 0.05 m instead of 5 cm.

Authors response: The announced paragraph will be deleted for the revised version of the manuscript. It was thought as a short statement to the plausibility of the used variation in probe spacing regarding to the sensor pair0.165-0.065 and sensor pair0.365-0.015. As these sensor pairs were not discussed in the preliminary section we decided that this paragraph is not essential for the context and for this reason difficult to reformulate.

P 6181-L 3: I think it must be 'thermal dispersivity'

Authors response: P 6181-L 3: Thermal diffusivity will be changed to thermal dispersivity.

Table 1: The 'period length' is not well explained in the text.

Authors response: The period length will be added at P 6162-L16 as notation, we think that this will explain the period length sufficiently.

Table 2: I think the table should include also the calibrated values for the saturated sediment, since they are actually used for the flux estimation. Please add the symbols , _, c, etc. of the parameters.

Authors response: Calibrated values of the saturated sediment and symbols of all presented parameters will be added to the table.

Table 4: Some values of the table like ne and Ksat are not well referred in the text. See P 6162 L 29 et seqq. The table could be even canceled, since all values can be sufficiently described in the text.

Authors response: cp. Authors response to P 6162-L 23 et seqq..

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Fig. 1: I suggest to change the legend in a way that 'B: Multi-Level-Temperature-Stick' is written without hyphen like 'B: Multi Level Temperature Stick (MLTS)'. For C: and D the acronyms '(PBS)' and '(PCS)' should be added. The caption should be reformulated ': : :.with temperature loggers at four different depths: : :..'.

Authors response: The figure legend and caption will be changed as suggested by the reviewer.

Fig. 2: The graph (b) and (c) should be exchanged according to the order in the text. To increase the accessibility of the figure vertical arrows could be added at the right end of (b) indicating upward and downward flow. This then should be also done for Fig. 3. Alternatively the notice (last sentence in the caption) should be moved forward to a more prominent place, let's say as the second sentence. On top of (a) two horizontal arrows could indicate 'complete dampened' on the left side and 'no damping' at the right side of the graph. This would enhance the readability of Fig. 2 dramatically. The data distribution by the box plot is not explained in the text. You should mention what the box plot shows, i.e. smallest and largest observation and the the lower and upper quartiles for example.

Authors response: Arrows indicating no and complete dampening on top of the figure and arrows at the right end of subfigure b indicating upward and downward flux will be inserted according to the reviewers suggestion. In respect to the order of subfigures: cp. the authors response to P 6169-L 19-20.

Fig. 4: Is it possible to graphically indicate areas of downward and upward flow in the graphs (a)-(f)? This would greatly enhance their readability! I recommend using the abbreviations of the probe designs in the captions like 'bottom screened Piezometer probes (PBS)'.

Authors response: An indication of downward and upward flow in the graphs (a)-(f) would be possible by using different symbols. Therefore we would need at least three different symbols (downward, no-flow, upward). We tested this but in our view this

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does not enhance the readability of the figure. On the contrary it was really hard to differentiate the symbols as they mostly overlap (especially in figure 4a, d and e bottom left). Therefore we decided to keep the figure 4 in its original version. The abbreviations of the probe design will be included in the caption of figure 4 for the revised version of the manuscript.

Fig. 6: Add b) to the lower graph.

Authors response: The graph will be changed according to the reviewers comment.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 6155, 2011.