Hydrol. Earth Syst. Sci. Discuss., 8, C3040–C3046, 2011
www.hydrol-earth-syst-sci-discuss.net/8/C3040/2011/

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.

C. Anibas (Referee)

canibas@vub.ac.be

Received and published: 27 July 2011

I think this paper is a significant step forward in handling problems related with field installations in the area of hydrological heat transport research. Therefore this paper emphasizes on questions existing quite a while; the verification of measurement set ups and flux estimates in a controlled environment. The article describes the most common configurations used to measure surficial temperature profiles, usually installed in rivers and lakes, in a sand box experiment. Additionally the authors apply a newly available instrument the 'Multi Level Temperature Stick'.

The article therefore clearly fits into the scope of HESS and is generally spoken of good quality, well structured and concise. The major remark I have is that the authors use some data for flux estimation which is not representative with respect to the applied method. I describe my problem in detail in my specific comments below. These results however can be presented in this article but I suggest focusing more on other results the authors have also used for their analysis. I regard these results as more interesting for the future readers of the article.

Specific Comments:

Page (P) 6157-Line (L) 8: I suggest adding a reference since it is discussing important aspects for the numerical and analytical modeling of temperature profiles:

Anibas C., Buis K., Verhoeven R., Meire P., Batelaan O.: A simple thermal mapping method for seasonal spatial patterns of groundwater-surface water interaction, Journal of Hydrology, 397, 93-104, doi: 10.1016/j.jhydrol.2010.11.036, 2011

P 6157-L 29 et seqq.: I think that beginning from 'This experimental study. . .. . .' can stand in a separate paragraph since there you describe the content of your article.

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

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

P 6159-L 20: Must be ' is the density of the saturated sediment (kgm-3),. . ..'

P 6159-L 21: I would add the unit for all parameters , w and c and cw separately.

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

P 6163-L 11: Must be 'USA' instead of 'US'.

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).

P 6164-L 7: I suggest changing the sentence like '. . ., so that four sensors are in the sediment and two sensors log the...'.

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. . ..'.

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

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.

P 6166-L 22: In connection with the former question, what do you mean with 'calculated flow velocities and zero'. This sentence should be clarified.

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 Δh. When I have the averaged q of Δh = 0.008 which is -0.48 and I subtract 0.06 from the confidence intervals the value is -0.54. For Δh = 0.013 the respective value is -0.64 then, still much higher then the former value. Can you therefore clarify your statement?

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.

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.

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

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.

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

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

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.

P 6174-L 9-27: I know the phenomenon from yearly thermal time series data of piezometers. When the temperature gradient in depth is almost zero the water column in the piezometer becomes unstable and starts convecting vertically in the pipe blurring most remaining vertical temperature differences. When the temperature gradient within the piezometer is reaching a certain value, the convection suddenly stops and a stable stratification is establishing again. This happens both for rising as for falling temperatures; the instabilities however are more sustained when surface temperatures are falling with respect to the groundwater temperature.

In your case these instabilities occur during around 11pm and 11am (Fig.5). In these times the thermal gradient in the piezometers apparently does not represent the water flux in the sediment. The strong thermal gradient measured must lead, as you state, to high flux estimates (Fig.6) but whatsoever doesn't represent any real flux.

I know that from the setup of your analytical analysis it is not possible to use just the data between 11am and 11pm for the flux calculation. With just this information I guess the flux estimates would be much more accurate; however probably still significantly less good then from the MLTS or sediment probes.

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 13: As long as there is a stable thermal stratification in the piezometer, the measured temperatures are not meant to be influenced by vertical heat transport via the piezometer pipe but by horizontal transport from the sediment via the pipe and water towards the sensor at a certain depth.

P 6175-L 16: Add the unit of RMSE.

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.

P 6176-L 10-11: Add units of RMSE values.

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.

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.

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.

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

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

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 , λ, c, etc. of the parameters.

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.

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. . ...'.

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.

Fig.3: The graph (b) and (c) should be replaced according to the order in the text.

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)'.

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

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