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Hydrology and Earth System Sciences Discussions

Interactive comment on "A distributed stream temperature model using high resolution temperature observations" *by* M. C. Westhoff et al.

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General comments:

Summary: The paper presents a stream water temperature model, which is calibrated with a stream water temperature data set obtained from distributed temperature sensing (DTS) using a fiber optic cable. DTS provides temperature measurements with exceptionally high spatial and temporal resolution. The presented model is based on the energy balance of individual stream reaches, taking the radiation balance, river bed conduction as well as lateral inflows into account. From the calibrated model locations and volumes of lateral inflow along the stream are determined. A good fit was achieved between observed and simulated stream water temperature time series at two different locations along the stream.

The use of DTS in hydrology is new and provides rich data sets of temperature, which can be used qualitatively and quantitatively to assess the hydrologic dynamics of stream-aquifer exchange and to delineate different runoff components. Because such tasks are not trivial the presented work is of scientific relevance and should be of great interest to the hydrologic community and the readers of HESS. The manuscript is generally written and structured well. However, the manuscript could be improved by elaborating some points in more detail:

I have been missing a more detailed review of existing literature on the use of heat as a natural tracer to assess surface water groundwater exchange. Despite the fact that the use of DTS in hydrology is new there is a long history of the use of temperature measurements and heat as a natural tracer to determine hydrologic fluxes in river systems (e.g. Stallmann 1965, Lapham 1989, USGS circular 1260, Constantz 1998, Niswonger et al. 2005). I believe the manuscript would greatly benefit from a brief review of that literature as it would help to put the presented work in a broader perspective.

A weakness of the manuscript in its present form I see in the lack of discussion of the assumptions and the limitations of the methodology that follow from them. Although the assumptions are stated their implications are not elaborated clearly. If the main objective of the paper was to just present a first application of a new methodology that might be appropriate, but the tenor of the manuscript suggests that the presented methodology holds much more potential (e.g. quantification of runoff components and lateral inflows etc.). For example the assumption is made that the stream can be modeled as a series of well mixed reservoirs and that the DTS system accurately records the temperature in those reservoirs. For what size of stream is that assumption appropriate? It is probably appropriate for a stream as small as the one studied. However, it would be inappropriate for a river of larger dimensions, where seepage flows can be variable across the channel (e.g. due to geologic heterogeneities) and were the assumption of well mixed reservoirs is questionable. That also hints at a limitation of the DTS system, spatial resolution is large along the cable, but to get lateral resolution one

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would have to employ several cables. Neglecting seepage losses in the model would cause problems in rivers that spatially and/or temporally alternate between gaining and losing conditions. All those assumptions constrain the broader applicability of the presented methodology. A sensitivity analysis of key parameters, best with a model that includes seepage losses, could provide more insight into which fluxes can justifiably be assumed negligible. If the claim is made, that DTS allows us to verify/quantify things we were not able to verify/quantify before, it needs to be discussed more clearly under what circumstances that is actually the case and what the limitations of the methodology are. Hence sections 3.4, 4 and 5 should be extended.

Specific comments:

P128 L6-7: How were those sources found, by the temperature survey? If that is the case this sentence belongs into the results section.

P128 L14-15: What method was used for the flow measurements?

P128 L22-24: 7 km seems quite far away, given the scale of the study site. Why were the two parameters not measured at the site (e.g. above the stream water surface). Perhaps make a brief statement on potential errors introduced by using data from a remote station.

P129 L8: The statement in this line seems to be at odds with what was stated in the introduction that previous studies have neglected dispersion and diffusion (P127 L7-8). It would be good also to clarify what exactly is meant with diffusion/dispersion here, the conductive component in the heat transport equation, which is a diffusive process, or dispersion of water in the channel? Both processes, however, would already be excluded by the assumption of a well mixed reservoir.

P129 L10-17: In think most of this text block with the descriptions of variables and parameters would better be placed right after equation 3, because most variables and parameters have been introduced by then.

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P133 L5-6: How adequate is that assumption? This assumption seems appropriate for a river system that is dominated by groundwater discharge (which seems to be the case in the Maisbich), but is probably questionable in systems with reaches and/or periods dominated by groundwater recharge. Studies in intermittent and ephemeral rivers have shown significant temperature variations below the channel (e.g. Bartolino and Niswonger 1999). This question should be discussed in the discussion section.

P137 L12-13: “make this assumption valid” seems too strong, I would prefer: “suggests that this assumption is valid”

P137 L17: again, the wording in this sentence is too strong. The small variance in discharge alone does not make the assumption of constant Q valid, but rather suggests that this assumption is acceptable.

P137 L19-23: Some of these assumptions should be elaborated more. They might all hold true for the Maisbich, but may be inappropriate for other systems. In particular the assumption of no diffuse sources and no water losses need more justification. Many streams, even in temperate climates, show significant spatial and temporal variability of seepage and shifts between gaining and losing conditions. This might be insignificant over the time scale considered here, but for a longer term quantification of lateral inflows it would be important.

P140 L1-4: This seems to be a crucial point of the study, partly also because seepage losses will affect the temperature of the stream bed layer and hence the conductive flux.

P140 L4-5: Not a trivial task. Which volumes should be better quantified the flows at an up- and downstream site?

P140 L14-15: I would rather argue that the same data set could not be obtained as conveniently and accurately with traditional techniques. Using simple temperature sensors with data loggers (e.g. tidbits) one could obtain a similar data set, but at a relatively

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high cost (for 0.5 m resolution over a 600m reach) and with probably less accuracy ($>$ 0.01 °C).

Technical corrections

P127 L5: better “heat as a tracer” as temperature is just the indicator of heat or energy transport

P127 L6: “temperature as a tracer” see previous comment

P128 L1-2: perhaps better: “the stream originates in a swampy area”

P128 L5: “V-notch weir”

P128 L6: perhaps better: “the schist crops out at the surface”

P128 L9: “V-notch weir” (see earlier comment)

P128 L14-15: perhaps better: “manual discharge measurements were conducted on”

P128 L25: “was assumed” instead of “is considered”

P130 L9: “heat transport” instead of “temperature transport” (see earlier comments)

P129 L16: “gives” instead of “give”

P130 L12: this reference should also appear in the reference list and not just as a foot note.

P131 L17-18: “TTools, which was developed by Boyd and Kasper..was used for the topographic angle calculations.” The reference should also appear in the reference list and not just as a foot note.

P132: references “Boderie and Dardengo” and “Boyd and Kasper” should be listed in the reference list at the end.

P137 L14: “has been made” instead of “have been made”

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P138 L13: the wording here seems awkward: “the other method returns into daily average temperature”

P138 L18-23: this paragraph seems to better fit in the methods section, as it describes how parameters were assigned and does not report results.

P139 L2: “damping” instead of “dimming”

P139 L10: “conduction of the riverbed”

P139 L8: “especially for the first 3 days.”

P139 L18: “is taken as constant”

P139 L24 “inaccurately” instead of “inaccurate”..however this statement seems to contradict the statement made on page 128 L26-28 that the assumption of a constant low wind velocity is acceptable. Perhaps better: “The lack of accurate measurements of wind velocity”

Figures and tables

The legend in Figure 1 is very small and should be improved for readability

Figure 2 only shows the heat transfer processes by radiation and conduction. A conceptual figure that includes all the energy fluxes in and out of a stream reach, including the convective fluxes (e.g. lateral inflows) would be helpful here.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 125, 2007.