Review of: Modeling insights from distributed temperature sensing data

by C. R. Buck and S. E. Null

General comments

This study re-evaluates a temperature model from 2001 using DTS observations. The 2001 model is a large scale (65km) model and the evaluated section is about 700 m. Due to the large scale of the 2001 model, the length of the grid cells are in the order of 70 m leading to the somewhat peculiar situation that the resolution of the observations are much higher than what is modelled.

With this study, the authors claim that 1) DTS measurements improved model performance, 2) DTS can be used to post-process existing model results, 3) air temperature may be a more important driver for water temperature than solar radiation and 4) this research contributes to the literature by demonstrating the value of long-term DTS observations for model calibration and increased confidence in simulated temperatures. However, I have severe doubts by these claims as will be explained below. Given these doubts I advise to reject the paper.

1: DTS measurements improved model performance

The authors claim that the model performance improved from a RMSE of 2 C for the 2001 model to 0.35 C when using DTS. These results are presented in section 4.2: Calibration results. However, to me it seems that no model parameter has been calibrated. The main difference with the 2001 model is the location of the upstream temperature boundary condition. Which caused indeed the improved model performance: the 2001 model had its upstream boundary condition about 10 km upstream of the currently investigated stream reach, while, when using DTS the upstream boundary was ~70 m upstream of the first node. With a longitudinal heating gradient of ~1 C/km one should have a really bad temperature model to have a RMSE of 2 C for the first km.

2: DTS can be used to post-process existing model results

I am not sure what the authors exactly mean with post-processing, but as far as I understand, the authors mean 'observing sub-grid heterogeneity'. This can indeed be done in this study since the model grid cells are much larger than the resolution of the observations. However, this is in most studies the other way around.

3: Air temperature may be a more important driver for water temperature than solar radiation

The authors come to this result by water temperature with both solar radiation and air temperature, where the latter correlates better. However, the authors say that understanding the causation is outside the scope of this article (P10015, L10) they have mentioned the reason for the good correlation already in Line 2 of the same page: 'solar radiation is the major factor influencing <u>both</u> air and water temperatures'. Air temperature is thus not a driver for stream water temperature, but subject to the same driver. The conclusion of the authors is thus erroneous.

4. This research contributes to the literature by demonstrating the value of long-term DTS observations for model calibration and increased confidence in simulated temperatures

This claim is stated on P10002 L17-19, but nowhere in the manuscript the authors 'proof' this or even refer to it. In my opinion this claim may be only correct when over the cause of the observation period different processes occur that can then be parameterized separately. I do not see the benefit for the current study.

Specific comments:

Besides these four main points some other issues need clarification:

Temperature model:
Some aspects about the temperature model have either not been explained or were used in a non-optimal way:

- Is RQUAL based on the advection-dispersion equation?
- Are the locations of the nodes the centre of the grid cell or are they located at the upstream end of the grid cell? This may not be so important if the nodes would only be a few metres apart, but in the current setup it is important. It determines in which grid cell lateral flows come in and which DTS points should be averaged to have an observation for each grid cell.
- I have some concerns about the 1 hour time step of the model. Flow velocities or water depths are not given, but if I assume a flow velocity of 0.1 m/s (water depth ~1m), the Courant number is 4 or 5 (based on a 80 m distance between the nodes). This means that on each time step, a water parcel (with a certain temperature) will, in reality, travel over 4 or 5 nodes, while in the model it only moves to the next downstream grid cell. Although the model may numerically be stable, the results may be less accurate.

P10005, L16: Are the DTS measurements single or double ended?
P10008, L9-10: Why are the DTS measurements not averaged over the full length of the grid cell?
P10009, L14-17: This inflow may also be estimated by using a simple mass balance for temperature: $Q_{down}\ T_{down} = Q_{up}\ T_{up} + Q_{PCO}\ T_{PCO.}$
P10010, L22: The difference of 0.2 C can also be caused by the fact that the Hobo temperature logger that measures the upstream boundary condition has an error of 0.2 C compared to the DTS measurements.
P10010, L16: The initial stream temperature is not important if the warming up period is long enough. For this model a warming up period as long as the travel time of a water parcel may already be long enough (except from some longer memory of the riverbed temperature).
P10011, L26-27: The claim that the authors were able to quantify the size of the mixing zone is a bit too strong: the stream is about 11 m wide and only one point over the width has been measured. Also: downstream of the PCO there is a curve in the river which may cause the plume to go from the left to the right bank. Could it be that downstream of this curve the observed temperature returns to 'normal' values (see Fig 5a, at ~750 m).
P10016, L20-21: Refer to Krause et al. (2012) who used two DTS line in one cross-section.
P10018, L1-4: I do not agree: measurements are needed to setup a model anyway, so why not immediately applying DTS to obtain these measurements.

Figure 3: This figure is not of much use.
Figure 4: Also add the time series of the boundary condition. This may explain the good fit and maybe even the differences in observed and simulated daily minimum temperatures.
Figure 9: Which DTS points were used to represent node 9? Were some of these points already influenced by the PCO water?

References:
Krause, S.; Blume, T. & Cassidy, N. J. Investigating patterns and controls of groundwater up-welling in a lowland river by combining Fibre-optic Distributed Temperature Sensing with observations of vertical hydraulic gradients, Hydrol. Earth Syst. Sci., 2012, 16, 1775-1792