Review of: What causes cooling water temperature gradients in forested stream reaches?

In this study, the authors investigate the causes of often observed, negative longitudinal water temperature gradients under dense canopies. They do this by combining a steady state flow model with an energy balance model and came to the conclusion that the negative longitudinal temperature gradient observed at a moment in time is caused by advective heat transport of cooler upstream water.

I do agree with their general findings, but there are several issues that need more attention before publication is warranted. The most important three are missing literature, the development of the temperature model and the way the data is smoothed or interpolated

Missing literature

There is a whole bunch of literature on stream temperature models, while only a very small amount has been mentioned. Especially the study of Roth et al. (2010) deserves some attention since it also uses an energy balance based stream temperature model to investigate the effect of riparian vegetation.

In the introduction, the authors mainly focus on Brown (1971) and Story et al. (2003) who attribute cooling gradients to groundwater input. It is correct that these studies did not explicitly explained their observed cooling gradient to advection of cooler upstream water, but because they included an advection term in their models, this effect was already implicitly taken into account (but apparently advection alone was not enough to explain the complete cooling gradient). Many other temperature models also have this advection in and thus take this effect implicitely into account as well (e.g. Bartholow, 2000; Boyd and Kasper, 2003; Foreman et al., 1997, 2001; Sinokrot and Stefan, 1993; Kim and Chapra, 1997; Westhoff et al., 2007; 2010; Younus et al., 2000 and several others).

Besides these, Westhoff et al. (2011) also showed that in their case study, the longitudinal gradient could not be explained by the energy balance (and since advection of cooler water could not explain the complete bias either, they attributed it to hyporheic exchange).

So the advective cooling has already been taken into account several times and the authors have to make that clear. So the novelty is not the fact that this manuscript is the first to have that included, but merely that it is the first time that this effect is emphasized.

Temperature model

I also have several question marks about the temperature model and about how the meteorological and validation data is used:

- First of all, a description of Q_bhf is missing, while in figure 2, it seems to be zero anyway.
- The net radiation is described in one equation (Eq. 2), but at the end of section 3.4.3 it appears to me that this equation is split up in shortwave and longwave radiation.
- Eq. 4 is not a penman style equation, but more a wind function equation. Why not using the full penman equation? The net radiation is determined anyway.
- P6452, L12: It is better to use the average velocity between x and x+1. This numerically more robust. The easiest way is to use (u(x)+u(x+1)) /2, with u being velocity.
- P6452, L24: Similar to above: the heat fluxes should have been determined as the mean fluxes between t and t+Δt
- Are the longwave and turbulent fluxes determined with modelled or observed water temperatures?
- Be aware that the Bowen ration goes to infinity when e_a = e_w (Eq. 8)

Smoothing and interpolating

Apart from the fact that I do not understand how the GAMs work (which degrees of freedom are used, to which data are the models fitted and to which combination of terms is referred to), I do not understand why the spatial canopy data should be smoothed anyway. Since a numerical model is used, this spatial heterogeneity can easily be handled. And, as can be seen in Fig 5a, the smoothing has unwanted effects at the first 150 m (the open part of the stream).

The authors also spatially interpolated meteorological data between two AWSs (P6453, L10). This seems completely inappropriate to me, since each of the stations is representative for a certain land use. Thus all numerical grid points in the open field should use data from AWS_open and not an interpolated value between AWS_open and AWS_FUS. This way you get similar effects as in Fig. 5a.

On the other hand, the authors do not interpolate data over time, which, in my opinion should be done. Not only for the meteorological data, but also for the simulated temperature at x+1: this data should be compared to the interpolated observed temperature at t + Δt, instead of to the nearest 15 min (P6452, L14).

Line by line comments:

P6444, L23: Make clear that longitudinal gradient at a moment in time are considered here.
P6447, L20: According to Fig 1, the gauging station is located just downstream of the point where a tributary comes in. So, how accurate is the discharge used for the model?
P6449, L19-25: The description of *g* is a bit fuzzy to me: isn't *g* not simply a function of *t, Θ and ψ*, instead of breaking it up in a *g** and a *g(t)*?
P6453, L3: I thought that each water parcel was model for each Δt interval.
P6456, L8: It would be helpful if also a couple of such lines are added to figure 6.
P6459, L16-18: This statement has also been made in Westhoff et al. (2011).

Minor comment:

P6445, L25: Also give estimates of water depth, discharge and velocity here.
P6447, L6: Add accuracy and precision of the thermistors.
P6448, L6: I suppose incoming shortwave radiation is meant here? Is rainfall also measured?
P6448, L16: write the symbol for net radiation also with a subscript (instead of *
P6456, L5: correct 'reac h'.
Fig 1: I guess the names of the AWS are switched. It is helpful if also land use is added to the figure.

References:

Brown (1971) and Story et al. (2003), Roth et al. (2010)

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