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What causes cooling water temperature gradients in a forested stream reach?

G Garner, IA Malcolm, JP Sadler, DM Hannah

Dear Professor Cloke,

Please find attached a revised manuscript by Grace Garner, Iain A. Malcolm, Jonathan P. Sadler and David M. Hannah titled "What causes cooling water temperature gradients in a forested stream reach?". Revised text is underlined and coloured red.

We include below a point-by-point reply to the Reviewers' comments. We are grateful to both Reviewers for their constructive comments. We hope that we have made the revisions necessary to warrant publication of our manuscript in *Hydrology and Earth System Sciences*.

Sincerely,
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Referee #2

Referee #2 recommended the following minor revisions prior to the manuscript being accepted for publication:

1. Equation 1: define Qbhf.

We have defined Q_{bhf} in Equation 1 as bed heat flux.

2. Section 3.3.4: This is a limited sensitivity analysis and does not add much to the story. I understand that the only 'user-defined' parameter in the model is the threshold used in Gap Light Analyser. However, there is uncertainty in all the energy exchange estimates, the flow routing model, and the flow accretion surveys, even though the uncertainty may not be quantifiable. How sensitive are the main conclusions to these errors? I think Section 5.3 may be sufficient to highlight issues of model limitations and uncertainty if the authors add some discussion about how errors in the flow routing model and flow accretion surveys may impact their conclusions. If a more thorough sensitivity analysis is to be included, I suggest focusing on how sensitive the conclusions are to errors in net radiation modelling (not just the threshold value), the flow routing model, and flow accretion surveys.

We have added to section '*5.3 Limitations*' further discussion of uncertainties associated with net radiation modelling and also discussion of uncertainties associated with the flow routing model and flow accretion surveys.

3. Section 3.4.1: The analysis outlined in Section 3.4.1 could be removed. This analysis adds little to the story and only serves to create some confusion. As far as I can tell this analysis is only conducted in order to make a 'cleaner' Figure 6. Statistical inferences are not drawn from this analysis and implausible artifacts are created (e.g. negative canopy densities for the first few meters). If measured canopy density and corresponding modelled net radiation are variable in space time, it would be valuable to show this variability.

The 2nd objective of the study is to ‘Explore the effect of changing riparian vegetation density on heat fluxes within the reach’. These analyses target this objective specifically by demonstrating broad patterns in canopy density and heat flux to the reader, which are difficult to identify without application of these methods.

4. Section 5.2: When placing this work within the broader literature, the authors need to highlight that the negative longitudinal temperature gradients observed in this study were modest compared to those observed in other studies. Especially since the focus of the study was on the one week during the ~1.5 year study period with the largest instantaneous negative gradients. Also in this section (lines 6 to 9), it is not clear if the authors are suggesting that temperature gradients observed in previous studies were falsely attributed to cool groundwater inputs. Please clarify.

We state in section ‘*5.1 Micrometeorological and landuse controls on energy exchange and water temperature*’ that “[the decreases in temperature we observed] were much less than those observed by McGurck (1989), Keith et al. (1998), and Story et al. (2003)…”. Furthermore, in this section we hypothesise a number of reasons for this: “Variability in cooling gradients at and between sites may be attributed to differing climatic zones, prevailing weather conditions (Rutherford et al., 2004), riparian vegetation density and orientation, channel orientation and subsurface hydrology; all control the magnitude of energy exchange and consequently water temperature (Poole and Berman, 2001; Webb and Zhang, 1997)”. Therefore, the manuscript does highlight that the gradients observed were modest in comparison to previous studies, and suggests reasons why this may have been the case.

We chose the language used in section ‘*5.2 Re- conceptualisation of processes generating longitudinal water temperature gradients*’ very carefully because we did not intend to suggest authors of those studies have falsely attributed cooling gradients to groundwater inputs. Notably, the information presented in Story et al. (2003) does not allow readers to decide conclusively whether (or not) their conceptualisation of that system was correct. As Reviewer #3 suggests (please see Referee #3 point 5), we present an alternative conceptual model pertaining to an alternative system, and so have renamed this section ‘*5.2 Alternative conceptualisation of processes generating longitudinal water temperature gradients*’ and restructured this section of our discussion to represent this.

5. Tables 1 and 2: I think these can be removed in order to streamline the manuscript. The authors should mention the range of error statistics in the text, but I think the tables are unnecessary. They could be replaced with a simple

time series graph similar to Figure 2f. The figure could compare observed temperatures at AWS_FDS and modelled temperatures at that site. This would give the readers a nice idea of how well the model is performing.

We have removed Tables 1 and 2. We have mentioned the range of error statistics in the text (see '*4.4 Modelled sptaio-temporal water temperature patterns*') and included Figure 7 which shows observed and modelled temperatures at AWS_{FDS}.

6. Figure 2f: If there was not a cooling mechanism (as stated on page 15, lines 38-39) would not the daily maximum stream temperature at AWS_FDS be the same as AWS_open? Instead the daily maximum stream temperature at AWS_FDS is consistently about 1 degree C lower than the daily maximum at AWS_open during clear sky days. This appears to be driven by a loss of energy at the stream surface (Fig 2d). The authors need to be careful to distinguish between diurnal warming and cooling patterns, and general statements made about longitudinal gradients (page 15, lines 38-39).

The maximum temperature at AWS_{FDS} may be lower than that at AWS_{Open} because the temperature of water advected into the reach changes over the day, as does energy input while water is travelling through the reach. Maximum temperature at AWS_{Open} is driven by high energy inputs that parcel of water that has received while travelling through the moorland upstream of the reach. In contrast, the parcel of water present at AWS_{FDS} when maximum temperature occurred at this site left AWS_{Open} 7.5 hours earlier when water temperature at AWS_{Open} was much lower (driven by lower energy gain conditions/ over night energy loss conditions in the moorland). These processes are explained in section '*5.2 Alternative conceptualisation of processes generating longitudinal water temperature gradients*'.

We have clarified the meaning of the text on Page 15, Lines 38-41 to read: 'our key finding is that water does not cool as it flows downstream under a semi-natural forest canopy. Instead, energy gains to the water column are reduced dramatically in comparison to open landuse, which reduces the rate at which water temperature increases as it travels downstream'

7. Figure 5: Why do the Gap Light Analyser grids not extend to the edges of the fisheye photos? Does the lens have a view angle greater than 180 degrees?

The view angle of the lens used is 180 degrees with the lens hood attached. The photographs were taken with the lens hood detached.

8. Figure 8: Could be presented as a 'heat-plot'?

We researched the possibilities for this figure thoroughly while preparing our response for the interactive discussion. However, we did not find a graphics package that would allow us to produce a heat-plot *and* plot the black lines representative of travel time through the reach.

Referee #3

Referee #3 recommended that the manuscript be accepted for publication subject to the following minor revisions:

1. The first paragraph in the Results section - please ensure the descriptions of each section match the headings.

Done.

2. Line 28, Page 10 - Please start the sentence with "Days" 10/07...

Done.

3. Line 7, Page 11 - The words "received" and "receipt" are redundant.

Done. The sentence now reads "… the water column received lower solar radiation, maximum temperatures".

4. Line 11, Page 11 - Do you mean highest?

Yes, changed.

5. Section 5.2 - This should be structured to highlight the differences in conceptual models. This is not necessarily a "new" conceptual model, rather it is a different conceptual model relative to previous work done in different systems.

We have incorporated this suggestion within section '*5.2 Alternative conceptualization of processes generating longitudinal water temperature gradients in forested river reaches*'.

6. Lines 38-41, Page 15 - This is the key finding and should be the main focus of the discussion. However, this is not presented well until the conclusion. Please consider re-structuring the discussion to have more of a focus on this key finding. Describe the key findings at the beginning of the discussion and build on this using your data analysis and modelling results.

We have re-written section '*5.2 Alternative conceptualisation of processes generating longitudinal water temperature gradients*' to convey the main finding of the study explicitly.