HESS-2014-195. Response to review by Anonymous Reviewer 2

We thank the Reviewer for her/ his thoughtful and constructive comments on our manuscript. The Reviewer considered the study to be novel and our conclusions to be reasonable, but recommended we made revisions to the results presented and how they are presented so that our conclusions are better supported. We address the Reviewer's comments below and make suggestions for proposed revisions.

1. *The manuscript 'HESSD-11-6441-2014' details a field and modelling study that aims to explain the energy processes driving observed instantaneous longitudinal negative water temperature gradients in a semi-forested stream reach downstream of an open area. In contrast to previous studies, the authors conclude that advection due to groundwater discharge and hyporheic exchange are not needed to produce negative temperature gradients. Instead, negative temperature gradients can be explained by water parcel travel times along the reach and the associated advection of relatively cooler water from upstream to downstream.*

The Reviewer's description of our study is broadly correct, however it is crucial that she/ he recognises that we conclude that the observed temperature gradients are generated from the combination of advected heat and substantially lower heat gains in the shaded reach. It is unclear to us what the Reviewer means explicitly by their suggestion that water parcel travel times contributed to the longitudinal gradients, we do not state or suggest this anywhere in the manuscript.

2. *This study addresses an important stream temperature management topic and employs a good use of field and modelling approaches. The primary novelty of this study is that the authors put an emphasis on the role of longitudinal advection to explain instantaneous negative temperature gradients between an upstream open site and a downstream forested site. As Dr Westhoff's review has outlined, previous diagnostic water temperature model studies have included longitudinal advection, although that has not been the emphasis of those studies because longitudinal advection alone could not explain downstream cooling. Although the conclusions of this study are reasonable, revisions are needed on what results are presented and how those results are presented to support the conclusions. My primary comments concern evaluation and uncertainty of the water temperature and net radiation models, providing more convincing evidence on the role (or lack thereof) of advection associated with groundwater and hyporheic exchange, and improving the figures so that readers can better interpret the results. I would also like to note that I am in agreement with Dr Westhoff's review. Therefore, I have tried to not duplicate any of his comments here.*

Thank you, we are delighted that the Reviewer recognises the importance of the topic and the novelty of our study. We also thank her/ him for their compliment regarding our methodological approach. We have responded in detail below to their specific comments regarding what results are presented, how these results are presented, and regarding evaluation of the model and any uncertainties in our approaches.

3. ***Water temperature model***

The water temperature model is critical to this study in order to establish that longitudinal advection explains the negative temperature gradients observed at *the site.*

The water temperature model is important to this study, but not only to demonstrate the importance of advection. We use the model to demonstrate that observed temperature gradients are generated by a combination of advected heat and substantially lower heat gains beneath the forest canopy (please response to Point 5).

4. *The water temperature model relies on field measurements and other models and estimates (flow routing, net radiation, turbulent energy exchanges) that are all associated with errors and uncertainties, yet the manuscript does not address the issue of model uncertainty at all.*

Please see Point 6 for a discussion of uncertainty in our estimates of net radiation.

The turbulent energy exchanges were calculated from measurements using commonly used and previously published methods (e.g. Webb and Zhang, 1997; Hannah et al., 2004, 2008; Leach and Moore, 2010, 2104; MacDonald *et al.*, 2013; Garner *et al.*, 2014). Although the equations used are empirical, they were not parameterised or calibrated for this specific site and so it is unclear to us how we could representatively quantify uncertainty in these variables without direct measurements of the turbulent fluxes e.g. from an eddy covariance tower. Furthermore, previous studies using models of this type have not employed sensitivity analyses on any variable and have typically used a single AWS (whereas we have used multiple AWSs to spatially scale our observations).

The velocity estimates used to drive the flow routing were compared to measured values collected throughout the reach during flow accretion gauging. Values corresponded well (Page 6447, Line 23). Maximum discrepancy in cross-section averaged values did not exceed +/- 6.3 %. Given that velocity was highly stable during the study period and average travel time through the reach was 7.5 hours (please see response to Point 27), potential uncertainty in flow routing would therefore be of the order of +/- 10 minutes.

We do not believe that it is necessary to conduct sensitivity analyses since the model performs well in comparison to others within the published literature and can accurately reproduce the observed gradients (see response to Point 5). Thus our interpretations and conclusions are fully supported.

5. *The only presented evaluation of the model is from page 6456, lines 2–3, 'predictions of downstream water temperature change were typically good' and Figure 6. Examining Figure 6, there appears to be periods when the model over- and under-predicts water temperature by 1 to 2 °C at the downstream locations, mostly during the clear sky days. This error is of similar magnitude to the observed negative temperature gradient signal that this study is trying to explain. Are these errors due to uncertainties in the net radiation model (see below), not including groundwater or hyporheic advection (see below), discharge errors, or uncertainties in the flow routing model (how uncertain are the travel times?). In order to have confidence in the conclusions of this study, a more robust evaluation of the model is needed. Some report of the error statistics (e.g. RMSE) for the*

model would be helpful. One suggestion could be evaluating the downstream predictions for all time periods during the study week (not just the four times per day examined here) and plotting the model residuals against time. This would be valuable in determining how prevalent the prediction errors are and whether they are systematic (and associated with misrepresentation of a certain process) or noise.

Following the advice of Dr. Westhoff we updated the structure of both the flow routing and the water temperature models, and changed the way we identified our validation data. This yielded improvements to model performance (please see our response to Dr. Westhoff, Point 11).

The Reviewer asked that we report error statistics for the model, and so we will include the following tables and some interpretation of them: Table 1 provides model evaluation statistics for the temperature of water parcels released at 06:00, 07:00, 08:00 and 09:00 on each day of the study period, and Table 2 states observed and modelled gradients for each water parcel, and the absolute error in the difference between these values.

Table 1: Model evaluation statistics for water parcels on released from AWS_{Open} at hourly intervals between 06:00 and 09:00 on each day of the study period

Day	R^2	Bias (%)	Root mean square error (°C)
01/07/13	0.98	1.1	0.2
02/07/13	0.71	0.8	0.3
03/07/13	0.99	0.3	0.2
04/07/13	0.97	1.5	0.3
05/07/13	0.98	1.1	0.3
06/07/13	0.99	0.2	0.3
07/07/13	0.97	0.6	0.4

Table 2: Absolute errors modelled instantaneous water temperature gradients modelled for water parcels released from the upstream boundary between 06:00 and 09:00 on each day of the study period

Day	Time water parcel released (GMT)	Observed gradient (°C)	Modelled gradient (°C)	Absolute error (°C)
01/7/2103	06:00	1.4	1.6	0.2
	07:00	1.2	1.6	0.4
	08:00	1.4	1.1	-0.3
	09:00	1.5	1.1	-0.4
02/07/2103	06:00	0.2	0.7	0.5
	07:00	0.1	0.5	0.4
	08:00	0.1	0.6	0.5
	09:00	0.1	0.6	0.5
03/07/2103	06:00	1.0	1.2	0.2
	07:00	1.0	1.0	0.0
	08:00	0.7	0.7	0.0
	09:00	0.5	0.3	-0.2
04/07/2103	06:00	1.6	2.1	0.5
	07:00	1.7	2.1	0.4
	08:00	0.9	1.2	0.3
	09:00	0.6	0.4	-0.2
05/07/2103	06:00	1.6	1.9	0.3
	07:00	1.6	1.4	-0.2
	08:00	1.3	0.5	-0.8
	09:00	2.1	1.4	-0.7
06/07/2103	06:00	2.0	1.7	-0.3
	07:00	1.4	1.6	0.2
	08:00	1.1	1.0	-0.1
	09:00	0.4	0.3	-0.1
07/07/2103	06:00	1.5	1.8	0.3
	07:00	1.6	1.2	-0.4
	08:00	1.4	0.2	-1.2
	09:00	1.1	-0.9	-2.0

Table 1 demonstrates that RMSEs are much better than those observed by Westhoff *et al.* (2011) in their model that omitted hyporheic exchange processes, and extremely similar to those observed by Westhoff *et al.* (2011) in their full model that included hyporheic exchange processes. Patterns of heating and cooling (as indicated by R^2) were typically predicted with high accuracy. The model is biased towards very slight over-prediction, but in all cases this was ~2.0 % or less. Table 2 demonstrates that there is no evidence of consistent bias in the predicted gradients. Modelled error in the predicted water temperature gradients was relatively small. Most importantly Table 2 demonstrates that our model that omits heat gains/ losses associated with hyporheic exchange and groundwater inflows does predict longitudinal instantaneous cooling gradients, and with good accuracy in most cases.

We believe that the error statistics in Tables 1 and 2 qualify the statement on page 6456, lines 2–3 that 'predictions of downstream water temperature change were typically good'. We address the Reviewer's comments regarding sources of uncertainty in detail below. However, given the good performance of the model we do not believe that uncertainties are large, consistent, or that they have affected the interpretation of the results or the conclusions to the study. Furthermore, if hyporheic exchange specifically was a major control on longitudinal gradients then we would anticipate large downstream gradients at night and consistent over-prediction during daylight hours. We refer the Reviewer to Figure 3b in our original manuscript that demonstrates very small gradients overnight (i.e. < 0.5 °C as stated on Page 6454, Line 22), and to Table 2 (above) that demonstrates inconsistent bias in predicted gradients, both in magnitude and direction.

6. ***Net radiation model***
   How uncertain are the estimates of modelled net radiation? Was the net radiation model evaluated against observed net radiation at the site? What threshold value was applied to the hemispherical images to convert them to binary images within Gap Light Analyzer? How was this threshold selected?

The threshold value was determined in three steps: (1) applying values of 120 to 190 at 10 unit increments to the hemispherical photograph at AWS_{FUS}, (2) modelling net shortwave radiation for the seven day study period, and (3) comparing quantitatively with values measured at AWS_{FUS} by calculating RMSE. The threshold value that minimised RMSE at AWS_{FUS} was chosen and applied to all hemispherical photographs. We will describe this procedure in our methodology. Additionally, this is the approach used by Leach and Moore (2010), who assessed the sensitivity of the solar radiation modeling approach to the threshold value; they concluded that 'modelled solar radiation [is] relatively insensitive to variation in threshold values'.

The radiation model has been used by other Authors without site specific validation (e.g. MacDonald et al., 2014a and b) because, unlike more empirical approaches (e.g. Westhoff *et al*., 2007; 2011), the model requires no calibration and, other than the hemispherical images (which were taken as described in Leach and Moore (2010)), no site-specific parameterisation. Consequently, the net radiation model has already been proven to be a useful approach and we demonstrate its abilities further by generating a dataset of radiative fluxes at extremely high spatial and temporal resolution.

7. *I have some concern over the selected threshold, considering Figure 4a has some noticeable riparian vegetation in the top left-hand corner, but was classified as having 0.0% canopy density.*

The vegetation the Reviewer refers to in Figure 4a is beyond the field of view considered by Gap Light Analyser. We understand that this is a source of potential confusion for the Reader and will overlay the photographs with the hemisphere used by Gap Light Analyser.

8. *In addition, how representative are the hemispherical images of lateral*

variations in canopy cover structure since photographs were only taken in the centre of the stream? Do canopy, terrain, and bank shading vary laterally across the stream?

Bank shading should be considered in narrow and incised streams, as should topography in narrow valleys (see Moore *et al.*, 2014). The banks of the Girnock are not incised and the stream is reasonably wide (water surface width averaged 9.5 m in this reach during our study period). GIS provided evidence that topography did not need to be considered, because the reach is not located in a narrow valley (see Fig. 1). It was therefore unnecessary to consider these effects in our models. Lateral variation should be considered for wide rivers, but vegetation on both banks is visible in our hemispherical photographs, and so width-averaged values were unnecessary.

9. *I agree with Dr Westhoff, in that I do not understand why a smoothing procedure was used on the canopy density and the energy fluxes at the stream surface. The water temperature model can be coded to include the spatiotemporal heterogeneity, and the smoothing procedure creates undesirable artifacts such as the negative canopy density for the first few downstream metres (Figure 5a) and interpolating between distinct riparian vegetation conditions.*

Please accept our apologies for this confusion. The smoothed data were used only to identify broad patterns in spatial and temporal variability in net energy. Raw, unsmoothed values were used for water temperature modelling. Please see our response to Dr. Westhoff, Point 14.

10. Groundwater/hyporheic exchange

The authors position the findings from this study, that longitudinal advection drives observed negative temperature gradients, as an alternative explanation to cooling caused by groundwater discharge and hyporheic exchange, citing the work by Brown et al. (1971) and Story et al. (2003).

Please see our response to Points 1 and 3; it is longitudinal advection in combination with reduced energy inputs below the forest canopy that produced the gradients we observed, and the water temperature modelling confirms this (see Table 2 in response to Point 5).

11. I am surprised that only minimal efforts to characterize hyporheic exchange and groundwater discharge were made to reject this competing explanation. The authors conducted differential streamflow gauging and cite previous research by Malcolm et al. (2005). However, differential streamflow gauging on its own is known to have limitations on characterizing groundwater and surface water interactions (e.g. Payn et al. 2009), particularly for water temperature modelling (Leach and Moore, 2011). The authors cite Malcolm et al. (2005) to justify that groundwater discharge and hyporheic exchange is minimal in the study reach. However, upon a quick review of that article, it was difficult to confirm whether the study reach here is influenced by hyporheic exchange, since some of the sites which appear to be located in the study reach of this paper (numbered 13-15 in the Malcolm et al. (2005) study) did appear to have distinct surface water and hyporheic water qualities (particularly for DO). Also, I cannot find mention of *hydraulic gradient measurements or downwelling patterns in Malcolm et al. (2005) as mentioned on page 6445, lines 18–19. In fairness, I did give Malcolm et al. (2005) only a cursory read; therefore, if more convincing evidence is provided in that paper I would recommend that it be specified and elaborated on in this manuscript. Of course, the water temperature modelling provides a means to evaluate whether groundwater and hyporheic energy exchange processes influence the thermal regime; however, since there may be some errors in the modelling (see comment above - although it is difficult to tell from the limited model evaluation), it would be important to elaborate on these topics in the discussion.*

As the Reviewer alludes, groundwater inflow and hyporheic exchange are different processes and we will respond to her/ his comments about each separately.

Regarding groundwater, sites 12, 13, 14 and 15 in Malcolm *et al.* (2005) are within the study reach. There are no substantial differences in alkalinity or conductivity between these sites, which would indicate groundwater discharge (please see Table 1 and Figure 5 in Malcolm *et al.*, 2005) and there are no rapid downstream changes in surface water chemistry (unpublished data), which would be expected to arise from groundwater inputs. Additionally, we measured bed heat flux at three locations within the reach and energy exchange at all of these sites was miniscule in comparison to other fluxes (see Fig. 2). Therefore, considering the evidence provided by the water chemistry, alkalinity, conductivity and bed heat flux data in combination with flow accretion gauging and the performance of the temperature model, we have high confidence that no substantial groundwater inflows occurred within the reach and thus that they did not cause the longitudinal patterns we observed.

We acknowledge that hyporheic exchange occurs within the reach (Page 6445, Line 18). Furthermore, quantifying heat exchange associated with hyporheic exchange is extremely challenging since it would involve quantifying the volumes, residence times and temperatures of down and upwelling hyporheic water, and additional assumptions would need to be invoked to do this. Consequently, we used bed heat flux as an aggregated measure of conductive, convective, advective and radiative heat exchanges between the streambed and the water column (as demonstrated by Evans et al. (1998)), and this value was extremely small. We have high confidence that these processes did not contribute substantially to the longitudinal temperature gradients we observed (please see response to Point 5) because: (1) bed heat flux was minimal at the three sites at which it was measured, and (2) the water temperature model predicted observed temperature patterns adequately (and there is no consistent bias, which as the Reviewer suggested would indicate the omission of an important process) without invoking additional processes, including hyporheic exchange.

We propose that we state explicitly in our methods that we did not attempt to quantify hyporheic exchange. We are also happy to include discussion of the potential influence of hyporheic exchange and also the challenges associated with quantifying these processes. Additionally, we thank the Reviewer for their thought provoking comments regarding hyporheic exchange; there is clearly much work to be done on identifying a suitable method for quantifying spatio-temporally distributed gains and losses of heat associated with these processes.

12. Figures and data visualization

I think the choice of what data is displayed in figures and the visualization approaches used inhibit full interpretation of the study results. In particular, the 3D plots are aesthetically pleasing; however, I feel that they fail to communicate the rich dataset and modelling results produced by this study. In Figures 3a, 3c-d, and 7a-c, it is difficult to read the absolute temperatures from the figures, and for Figures 3a and 7a, most of the rising and falling diurnal periods of the signal are hidden behind the diurnal maximums. For Figures 5b-d, it is difficult to tell when the net energy fluxes are above or below zero. I suggest more 2D heat plots (aka image or raster plots, as used in Figure 3b) for showing model output when interpolation could be warranted or using time series line plots when showing observed data.

Thank you for these suggestions. We will replace Figures 3 and 5 with the following figures, respectively:

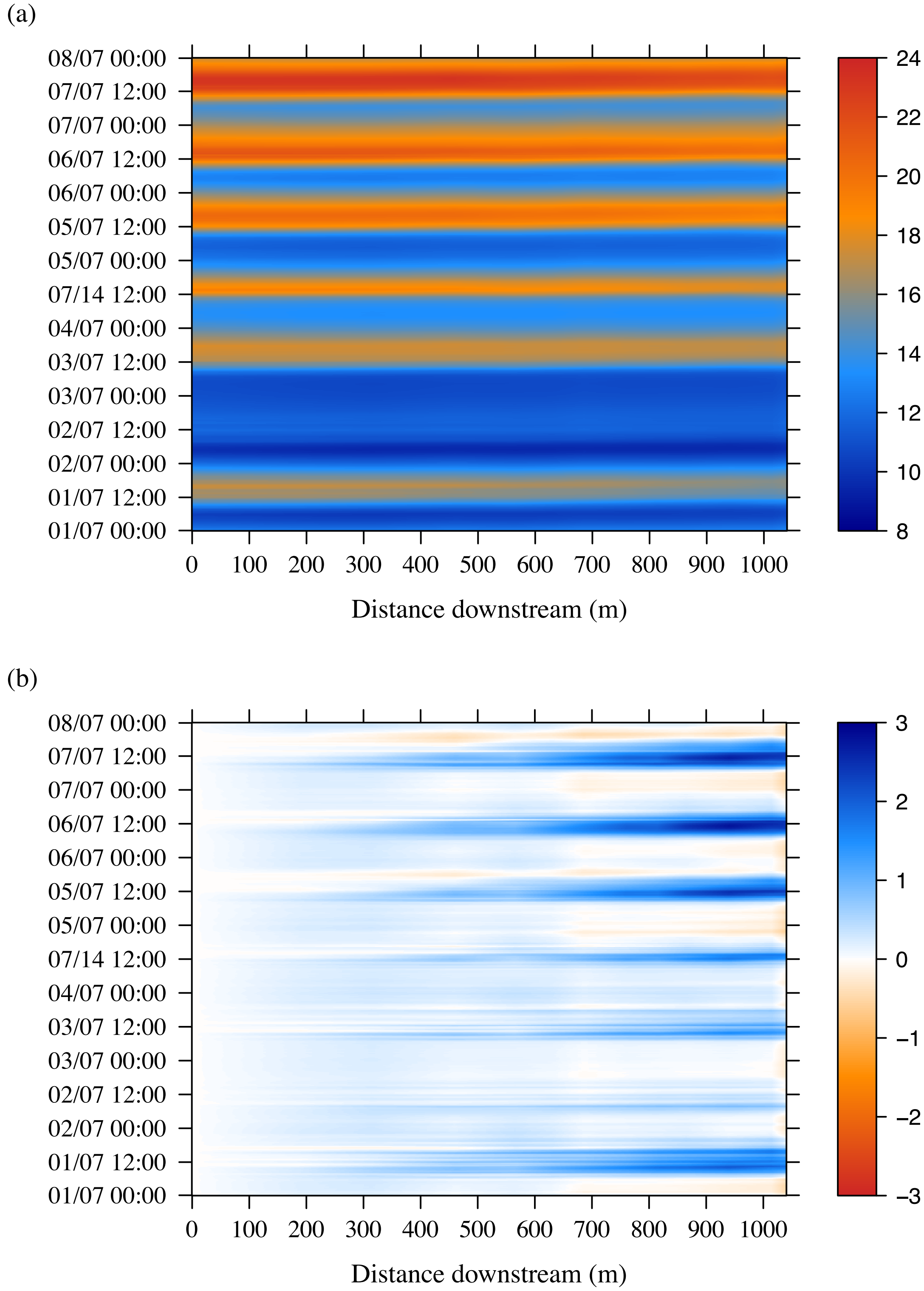

Figure 1. Figure 3. Spatio-temporal patterns in instantaneous water temperature measurements (a) absolute values (°C) and (b) Differences between AWS_{Open} and each monitoring location within the reach (positive values indicate instantaneous cooling gradients). Values were interpolated linearly from observations at 1 m intervals.

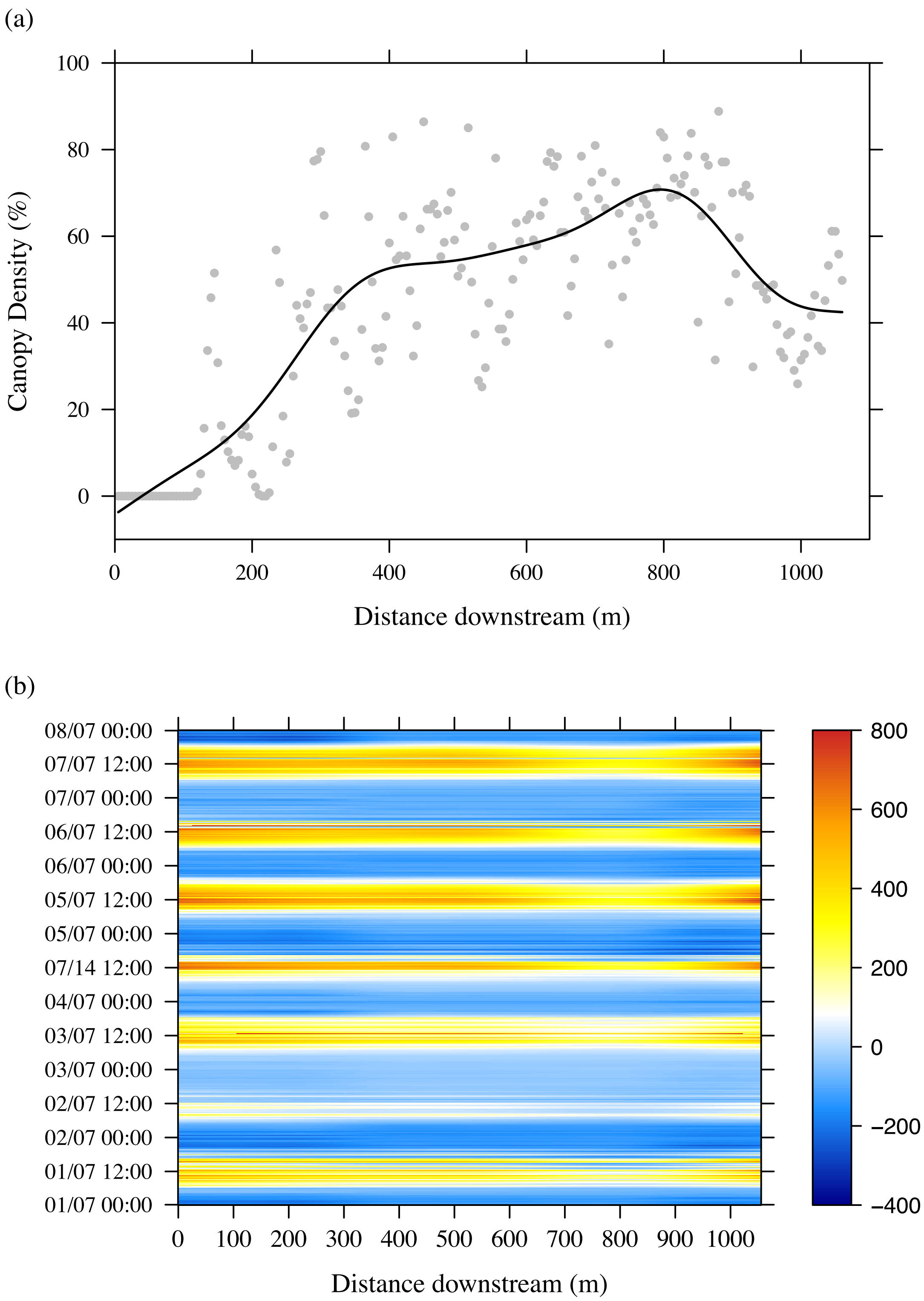

Figure 5. Patterns within the reach in (a) canopy density and (b) net energy flux (MJm^2d^{-1})

To

We have drawn an alternative figure for Figure 7 (below), but worry that it is difficult to interpret given that we are representing 3 dimensional data on a 2 dimensional surface. We understand the Reviewer's point that much of Figure 3a is hidden. We propose that we include the original Figure 3 with only panels b and c as examples.

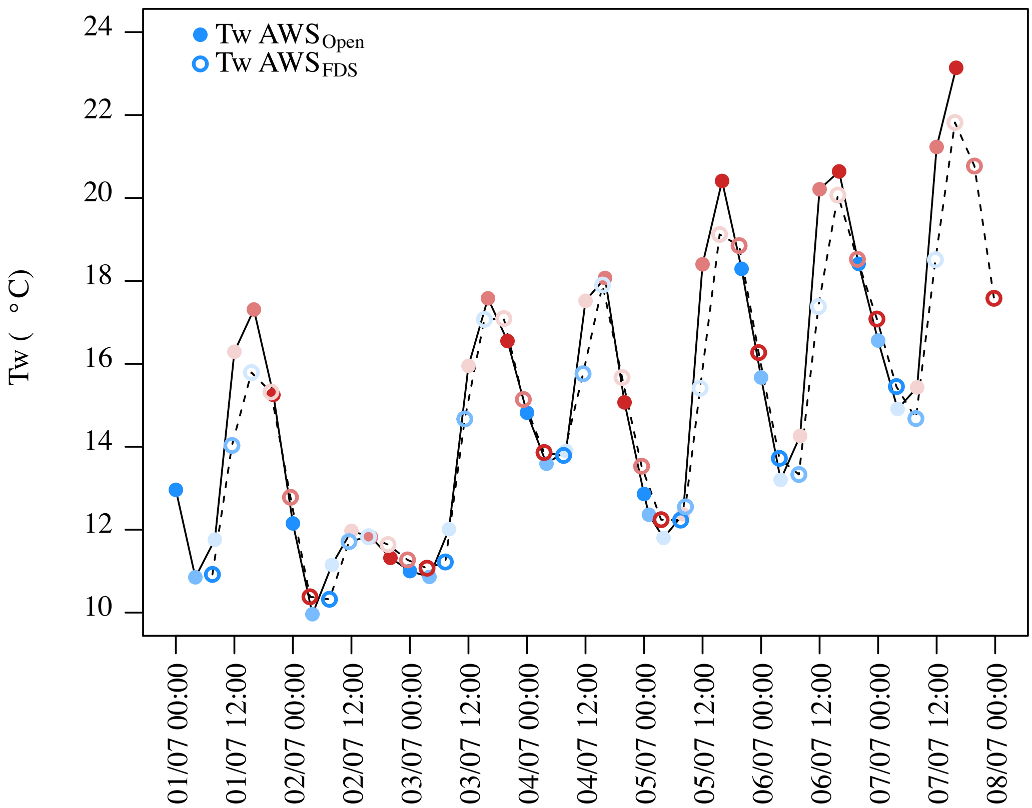

Figure 7. Temperature of water parcels routed through the reach at at AWS_{Open} and on arrival at AWS_{FDS} [discrete parcels of water are represented by unique colours on each day. For clarity, parcels released from AWS_{Open} at 4 hourly intervals are presented].

13. A really useful plot, particularly to support Section 4.2, would be a simple time series line plot of the water temperature at AWSopen and AWS_{FDS}. You should also include the instantaneous difference between the two sites. This will allow the reader to see more clearly (than is provided in Figures 3a-d) the diurnal patterns, the lag in daily maximum temperature, and difference between these two sites.

Thank you for this suggestion, we will update Figure 2 as follows:

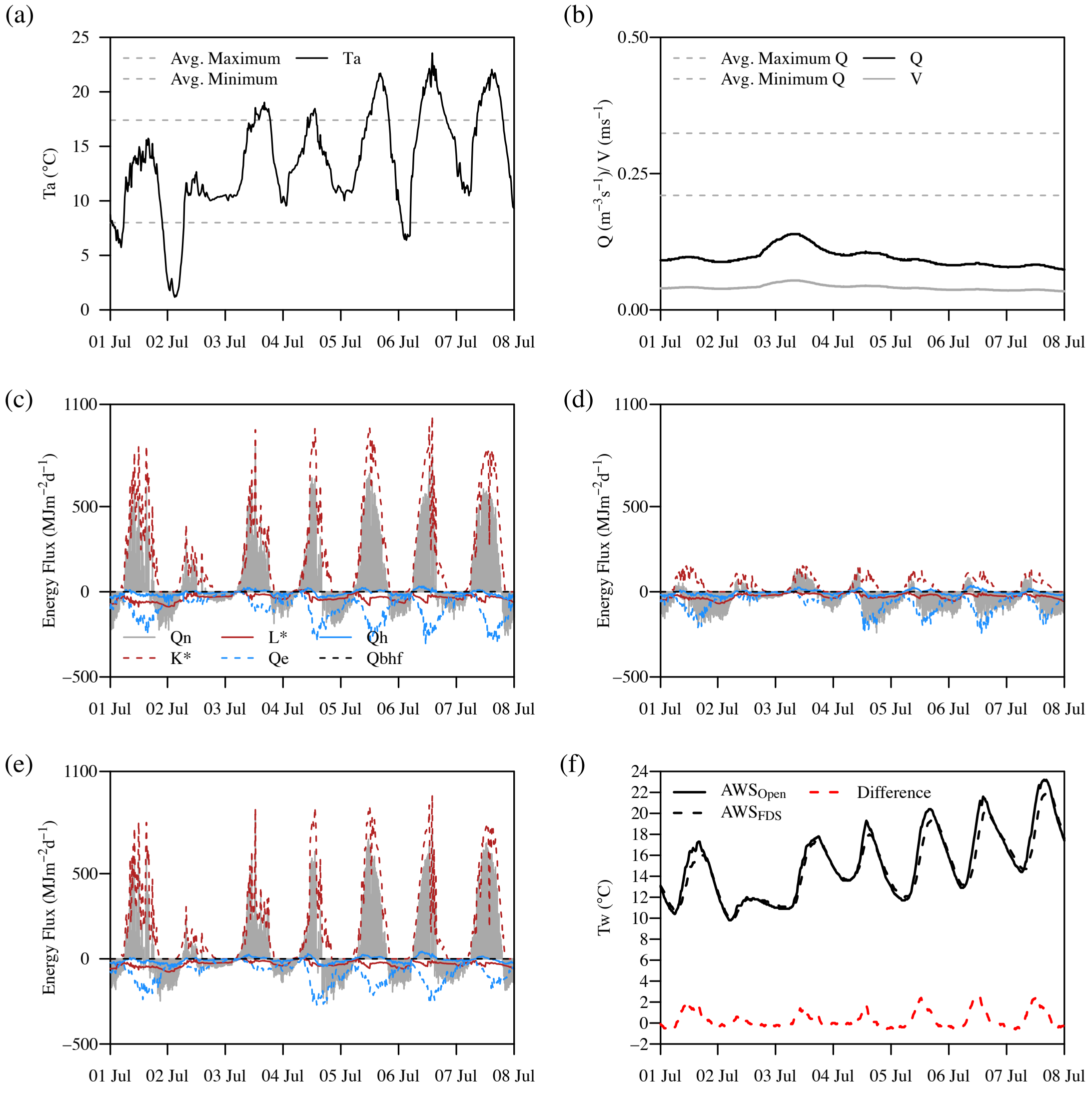

Figure 2: Study period (a) air temperature (b) discharge, and energy fluxes at (c) AWS_{Open} (d) AWS_{FUS} (e) AWS_{FDS}, (f) and water temperature at AWS_{Open}, AWS_{FDS} and AWS_{Open} minus AWS_{FDS} (positive values indicate that temperature AWS_{Open} was greater than temperature at AWS_{FDS}). Averages represent values for DOYs 183 to 289 in the 10 years preceding 2013.

14. I assume that Figures 3a-d are presenting the measured water temperatures? If so, what kind of interpolation approach was used to generate these plots? Is interpolation of these data warranted?

Data used in updated Figures 3a-b was interpolated linearly at 1 m intervals (we will state this, see Figure 3 in response to Point 12). We believe that this is warranted given the high spatial resolution at which we collected these data (i.e. 50-100 m).

15. *For Figures 7a-c, it mentions the black lines represent the water parcels. Because of the 3D plot, it is very difficult to determine the travel time for one parcel to travel from 0 m to 1000 m, and my best guesses from the figures suggest anywhere between 4 to 6 hours although the manuscript reports that travel times were on average 7.5 h. Am I misreading these figures?*

Regarding accuracy of our statements concerning travel time please see our response to Point 5, Figure 7. Travel time ranged from 7.25 hrs to 7.75 hrs. However, travel time was predominantly 7.5 hrs, owing to low variability in discharge and therefore water velocity (see Fig. 2 in the original manuscript). We printed the manuscript on A4 paper and estimated for Figures 7 b and c that an hour was equivalent to 0.75 mm on the x-axis, thus taking the example of the water parcel released from AWS_{Open} at 6 am in Figure b then we calculated travel time to be around 7.5 hrs.

16. ***Specific comments***

Title: I would consider revising the title. I appreciate the succinctness; however, it gives the impression that the study will provide a generalized explanation for the drivers of negative temperature gradients in (multiple) forested reaches. Instead, the study reports on a specific case study where groundwater discharge and hyporheic exchange are assumed to have no impact on the thermal regime. Therefore, at best the findings of this study are limited to reaches that meet these conditions.

Thank you for this comment. We will change the title to: 'What causes cooling water temperature in a forested stream reach?'.

17. *Page 6442, line 14 and page 6446, line 10: '> 200 hemispherical photographs'; exactly how many hemispherical photographs were taken?*

We took 211 hemispherical photographs. We will state this number instead of > 200.

18. Page 6444, lines 8–10: What is 'point-scale' defined as here? Story et al. (2003) and Leach and Moore (2011) were conducted over reach lengths of about 250 m and 1500 m, respectively. Are these considered point-scale studies?

This statement has been misinterpreted. It does not concern the scale at which those Authors studied spatial variability in temperature, but rather the representativeness of point-scale measurements of bed heat sources and sinks in reaches with substantial heterogeneity in groundwater inflows.

19. *Page 6445, line 25: Is 9.5 m the channel width or the wetted width?*

The stream surface width, we will state this.

20. *Page 6446, line 19: You have field data from October 2011 to July 2013,*

perhaps you can use these data to calculate instantaneous longitudinal temperature gradients for the whole record and highlight the distribution of gradients and show the frequency and magnitude of negative gradients. This would put the detailed week long study into broader context. This is just a suggestion.

Thank you for the suggestion. Continuous water temperature data was available from February 2012 to July 2013. We will include the following figure as Figure 3 and use it to demonstrate on Page 6454, Line 1 that: (1) large instantaneous longitudinal cooling temperature gradients occurred frequently during the entire monitoring period, (2) were largest during spring and summer months, during which time warming gradients were limited (i.e. < 0.5 °C), and (3) gradients during the study period were very large.

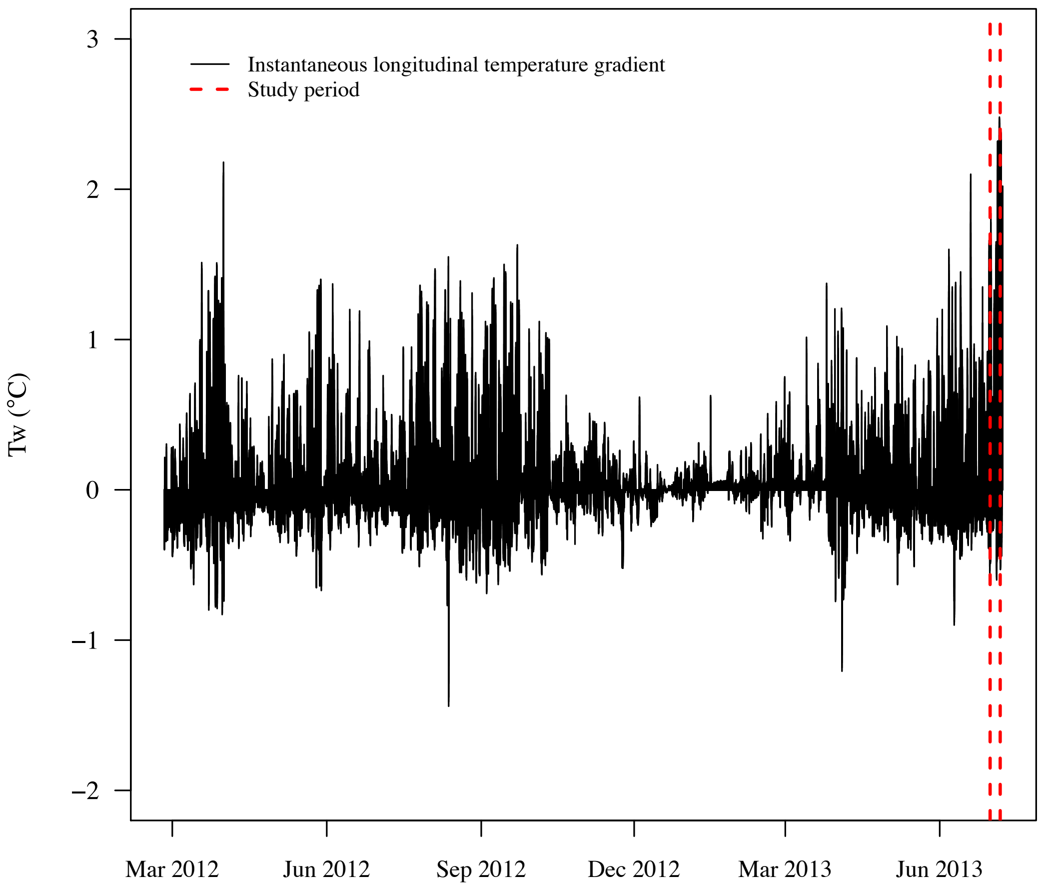

Figure 3. Instantaneous stream temperature gradients during field data collection. Positive values indicate that temperature at AWS_{Open} was greater than that at AWS_{FDS} (i.e. instantaneous cooling gradient) while positive values indicate that temperature at AWS_{Open} was less than that at AWS_{FDS} (i.e. instantaneous warming gradient).

21. Page 6447, section 3.2.1: How much lateral variability was there at installation locations of the temperature loggers?

Previous research in the Girnock Burn has demonstrated lateral variability in stream temperature below measurement accuracy of the water temperature loggers used in this study (see Imholt *et al.*, 2013).

22. *Page 6447, section 3.2.2: How many discharge and stream surface width surveys were conducted during the study week?*

Flow accretion gauges were conducted on two days during the study-period. Stream surface width was measured once at all locations. Given that discharge (and therefore water levels) was extremely stable during the study period (see Fig. 2 in original manuscript) this was sufficient.

23. *Page 6447, lines 15–16: Is the error in the discharge measurements asssumed to be ~10% or was this error quantified for these measurements by using replicated measurements or some other approach? Does this uncertainty impact your flow routing and water temperature model results?*

Leach and Moore (2011) state that uncertainty in gauges using the velocity-area method in channels with appropriate characteristics have uncertainty of +/- 5% and that accounting for this then 'differences between pairs of repeated measurements should thus range up to +/- 10%'. The gauges we conducted were used for flow accretion surveys and for comparison with values measured at the Scottish Environmental Protection Agency Weir at Littlemill only. Discharge values at Littlemill (scaled by catchment area) were used for flow-routing and stream temperature modeling, so the uncertainty the Reviewer refers to here did not impact the modelling.

24. *Page 6448, section 3.3: How was the bed heat conduction flux estimated? What field data were used?*

We did not measure bed conduction flux, rather bed heat flux which is an aggregated measurement of convective, conductive, advective and radiative heat exchanges between the atmosphere and the riverbed, and the riverbed and the water column. Please see our response Point 11 and to Dr. Westhoff, Point 7.

25. *Page 6453, line 2: How and why is the discharge scaled by catchment area?*

Discharge was scaled to remove the effects of runoff potentially entering the channel between each node and at Littlemill, and to remove discharge added by a very small tributary that enters the Girnock between the lower limit of the study reach and Littlemill. Please see our response to Dr. Westhoff, Point 18 for our methodology and its accuracy.

26. *Section 4 'Results': Please consider using more specific quantitative language when describing the results. There is considerable usage of terms such as 'very low', 'lower', and 'high'.*

We will update the manuscript with quantitative examples to qualify our statements.

27. *Page 6456, lines 4–5: The flow routing model suggests a mean average*

travel time of 7.5 h. How much did this vary during the study period?

Discharge and thus velocity were very stable during the study-period (see Fig. 2 in original manuscript). Travel time was 7.25 hours at minimum and 7.75 hours at maximum, however travel time was predominantly 7.5 hrs (see updated Fig.7 in response to Point 6) thus the average value is representative.

28. *Page 6460, line 12: This modelling approach requires considerable field data collection and parametrization to run the model. I question whether it is a realistic tool to be used for areas where observational datasets are unavailable.*

This sentence refers to using models such as this one to improve our understanding of processes and associated effects for which observational datasets spanning the range of potential conditions are unavailable. We will change the sentence to read: 'Future research should utilise tools such as this one to understand the effects of climate, hydraulic conditions, channel orientation and shading scenarios on water temperature processes'.

29. Technical corrections

Page 6443, line 23: Perhaps replace 'decreases in temperature' with 'negative instantaneous differences in temperature'.

We have decided to leave the text as originally provided in this case because we feel the proposed alternative is difficult to readily interpret. We have also used the term instantaneous in previous sentences, so the meaning should be clear.

30. *Page 6444, line 4: Replace 'Storey' with 'Story'.*

Please accept our sincere apologies for this mistake; we will amend the spelling to 'Story'.

31. *Page 6445, line 21: 'Dominated predominantly' is redundant.*

We will change this to read '…heat exchange within the reach was anticipated to be dominated by…'

32. *Page 6452, line 18: What does the '900' refer to in '_900'?*

900 refers to the number of seconds in 15 minutes, which is the temporal resolution of the model. We have been more explicit about what this means in our response to Dr. Wetshoff regarding our updated flow routing methods. Please see our response to him, Point 10.

33. *Page 6466, Figure 1: The AWS labels are incorrect. Also, please add the forest cover to the plot.*

Thank you for pointing this out. We will correct the naming of the AWSs and add landuse (forested or moorland) to Figure 1.

34. *Page 6471, Figure 6: Add letters to the plots for reference. Also, perhaps use different symbols or different line types for the four time periods examined, since it is difficult to tell the colours apart when printing in greyscale.*

Please accept our apologies for omitting the letters in our original submission. We did consider changing the line types for each time period but it was difficult to distinguish between modelled and observed values on the resulting plot. We think this will suffice, especially as this Journal is published only online and the Reader may refer to that colour version.

References

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