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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Winter stream temperature in the rain-on-snow zone of the Pacific northwest: influences of hillslope runoff and transient snow cover" *by* J. A. Leach and R. D. Moore

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We have pasted the comments from the reviewer below. Our responses are inserted to follow each comment.

Comments from Referee #2

General Comments

This study seeks to investigate the relative importance of hydrology and surface (and

friction) heat exchange in controlling winter stream temperatures in a small catchment in the Pacific northwest. Net heat input for specific river reaches is estimated based on differences in discharge and stream temperature between gauging stations that constrain the study reaches. Surface fluxes are modelled using data from weather stations located in the study catchments and in the case of radiation spatially adjusted using hemispheric photography. The effect of hyporheic exchange is also estimated using piezometer data, averaged across the reaches. Substantial differences between estimated net heat input and calculated inputs from surface, bed friction, conduction and hyporheic exchange suggest that surface exchanges are not the dominant control on heat inputs. Back calculation of the temperature required of runoff inputs over the reach (given spatial variation in discharge) matched well with measured groundwater temperatures in the riparian zone suggesting that hydrological controls from catchment runoff processes are the dominant control on stream temperature during the winter (at least under higher flows). The temperature of hydrological inputs is also shown to vary depending on precipitation conditions and the presence of snow pack thereby suggesting potential important impacts of changing precipitation type and snowpack distribution under climate change. This study represents a very significant field and modelling effort, where the authors have attempted to represent as many hydro-meteorological processes as possible within their study framework. As such the study adds significantly to what has been reported previously and I have no major issues with the field or modelling approaches or the substantive conclusions. I therefore consider that the study should be considered for publication following minor revision. Because the study contains such a breadth of data collection and modelling, the methods section requires an additional introductory paragraph to provide an overview of data collection and modelling approaches and how these combine to answer the questions posed in the introduction prior to launching into the detail currently provided. The authors also need to clarify some additional assumptions within the paper and to frame the discussion in the context of these assumptions. I have also posed a number of additional questions and made some suggestions that the authors may wish to consider during their revision.

RESPONSE:

We thank Referee #2 for her/his review and comments.

We will revise the manuscript to guide the reader through the detailed field and modelling methods more effectively, and to place the methods within the overall purpose of the manuscript. We will include a reference to the corresponding sections (3 and 4) on page 12955 where we first mention how the study hypotheses are addressed with the energy budget (Section 3) and historical (Section 4) approaches. We will also include further text at the beginnings of Sections 3 and 4 to assist the reader through these separate but complementary approaches.

General response regarding hyporheic energy exchange

Many of the reviewer's comments concern the estimate of hyporheic flow and its associated energy flux. We agree that estimating hyporheic energy exchange is challenging. However, based on our understanding of the study site and previous studies conducted at our study reach and another nearby headwater stream (Moore et al. 2005; Guenther 2007), we expected that the heat flux associated with hyporheic exchange would be a secondary heat budget term during winter storms. Therefore, we focused most of our field monitoring efforts on characterizing the lateral advective flux and surface energy exchanges, which we expected to dominate the thermal regime. We made spot measurements of vertical hydraulic gradients and temperatures of the streambed in order to evaluate our assumption that hyporheic energy exchange is a secondary heat budget term.

In Section 3.2.3 we outlined two key assumptions regarding the characterization of hyporheic exchange rate: (1) all water infiltrating the bed within the reach follows subsurface flow paths that discharge within the same reach; (2) the fraction of the total bed area that experiences downwelling is equal to the fraction of the piezometers with downwelling flow. If assumption (1) were false, our approach would tend to overestimate hyporheic heat exchange. Because we distributed the piezometers at roughly

equal intervals, and the bed morphology did not display periodic variations at the same interval, the sample of locations should yield a relatively unbiased estimate of the fraction of the bed with downwelling flow.

An additional assumption was that hyporheic flow is primarily Darcian flow driven by longitudinal changes in hydraulic head across steps of the step-pool morphology, as suggested by Buffington and Tonina (2009) for streams dominated by step-pool morphologies (which is the case for Griffith Creek). Furthermore, valley confinement, the shallow layer of sediment in the stream and near-stream zones, and steep channel gradients likely inhibit lateral hyporheic flow (Buffington and Tonina 2009). In addition, the well armoured and poorly sorted bed material likely limit the amount of turbulence-driven hyporheic exchange, especially considering that the temperature differences between the stream and shallow stream bed (< 5 cm below the surface) were often less than 0.2 deg C. Therefore, turbulence-driven hyporheic energy exchange is likely small compared to the other heat budget terms. In addition, shallow bed material depths and exposed bedrock along the channel limit the total volume available for hyporheic exchange.

Although we acknowledge that there is considerable uncertainty in our estimate of hyporheic exchange rate, a critical factor that suggests the hyporheic energy flux is a secondary heat budget term is that temperature differences between the stream water and bed were small, which acts to limit the magnitude of the hyporheic energy flux, despite potentially large hyporheic exchange rates.

It is almost certain that water discharging from the bed at our piezometer locations showing upwelling is some combination of hyporheic water and hillslope throughflow discharging through the stream bed. This conclusion is reinforced by the increase in number of upwelling sites with increasing streamflow. This problem with equating upwelling flow with hyporheic discharge is why we focused on the number of downwelling sites to estimate the hyporheic exchange rate.

In Section 5.1 we noted that 'Further research is required to clarify the role of hyporheic heat exchange and its influence on stream temperature dynamics.' This statement is consistent with the general direction of the reviewer's comments. The reviewer suggests that tracer modelling may be an approach to better quantify the hyporheic energy flux. However, it is difficult to separate hyporheic flow from storage in backwater areas, dead zones and pools without additional information. Working at another headwater stream near Griffith Creek, Scordo and Moore (2009) demonstrated that transient storage was dominated by in-channel storage zones at low flows rather than hyporheic exchange. The problem of interpreting tracer breakthrough curves would be even further complicated during rain or rain-on-snow events, which would have unsteady flow and associated changes in the nature and magnitudes of in-channel transient storage and hyporheic exchange.

In the revised manuscript, we will add details in Section 2 on the stream morphology. We will also provide further details in Section 3.1.5 on the piezometer measurement methods, and include additional discussion in Section 5.1 about the uncertainty and challenges associated with estimating the hyporheic energy flux.

– Specific comments

12956, Line 25. Can you bring in information on the range of wetted widths (from section 3.1.2) to give the reader a feel for the channel sizes? Are the channels influenced by substantial topographic shading as well as shading by vegetation at the time of the study? Are the channels incised, if so, does this also affect shading?

RESPONSE: We will include the range of wetted widths in this section. We will mention the high relief of the catchments, which provide substantial topographic shading of the stream. However, we note that the use of hemispherical photographs to model net radiation implicitly accounts for terrain shading. We will also mention that the channels are not heavily incised and shading from channel banks is minimal.

– 12957, Methods. This is a comprehensive, and as such, necessarily complicated

study. It would be very useful for readers if you provided an overview of the experimental design / overall approach for data collection and analysis at the start of the methods, highlighting how this allows you to address the two hypotheses identified in the introduction. At present the methods section goes straight into the detail of data collection and modelling without providing an overview of how it all fits together and why it is all necessary. As such it is difficult for the reader to readily understand the overall approach.

RESPONSE: As mentioned above, we will include further text at the beginnings of sections 3 and 4 to assist the reader through the methods and results of the energy budget and historical studies.

– 12959, section 3.1.3 Were the loggers cross calibrated to check for logger bias? If so, was any bias accounted for in calculating temperature differences over reaches. This would be more important in winter where temperature variability tends to be lower. Were the spot measurements with a hand held thermometer? Were these in good agreement with logged values for the same time? Did they identify any bias or drift?

RESPONSE: We will include the following information in section 3.1.3 and 4.1: "Temperature loggers were cross-calibrated at 0 deg C (ice bath) and room temperature, before and after field deployment. The stream temperature loggers were also routinely checked in the field using a WTW 330i handheld conductivity and temperature meter and found to be within the stated accuracy range of the loggers."

– 12961, installation of piezometers. How were the locations chosen for the piezometers? Did you standardise on morphology or was the location random. Were piezometers located near the bacnks or in the centre of the channel? How did you measure water depths inside and outside the piezometers? Did you place a stilling well around the piezometers at the time of measurement? Did you install nests of piezometers at any locations? Did VHG vary with depth? What sort of range of head differences (mm's or cm's) were you getting between the stream and streambed? Given the depth of the piezometers these could be rather small and difficult to measure accurately. Over what range of hydrological conditions were the head data obtained? Could this data also be presented?

RESPONSE: As noted in Section 3.1.5, piezometers were installed at approximately even spacing along the study reach. However, at four step-pool sequences, a piezometer was placed upstream of the step and downstream in the pool. Piezometers were located in the centre of the channel. Head measurements were made with an electronic beeper, which has previously been found to be accurate to +/- 0.05 m/m (Guenther 2007). We will add this information to the manuscript. Piezometer nests were not installed. Table 1 includes summary statistics for the VHG measurements and the hydrologic conditions during which the measurements were made.

– 12961, Line 25, what depths were the thermocouples installed at? Did it vary by location? Did all locations have 3 thermocouples?

RESPONSE: We will clarify in the revised manuscript that twenty-two locations had three thermocouples and three locations had two thermocouples, due to the shallow depth of bed sediment, and thermocouple installation depths varied by location.

– 12962, ca. L20, so it was assumed that the point measure of net radiation was 'correct'ÂÎ and the hemispheric images calibrated to obtain best agreement with the point measurement. Could the point measures have been biased compared to more average conditions potentially represented by the photographs? For example the sensor shaded or in direct snlight for more or less time than the reach more generally (which the hemispheric photographs would aim to capture)?

RESPONSE: It appears that the reviewer did not fully understand our approach to calibrating the threshold values used to binarize the hemispherical photographs within the Gap Light Analyser software. We will revise the manuscript to try to make more clear that the threshold value was calibrated by comparing observed incoming solar radiation from the two above-stream pyranometers with modelled incoming solar radiation using

hemispherical images captured at the pyranometer locations. The comparison used data collected over the entire study period and included a range of sky conditions.

After determining the optimal threshold value using the approach outlined above, net radiation was modelled using the approach of Leach and Moore (2010) and compared to the measured net radiation at fourteen different sites along the reach. For each site, radiation was modelled using a hemispherical photograph taken at that location. The net radiation measurements sampled a range of terrain and vegetation conditions, as well as a range of sky conditions. Thus, our test data set based on net radiation measurements is independent of the data used to calibrate the threshold.

– 12964 section 3.2.3, was bed heat conduction calculated for every 15 minute period?

RESPONSE: No, only spot measurements were made. In Section 3.1.5, we state that 'Vertical hydraulic gradients, bed temperatures, and hydraulic conductivity were measured once a month during winter site visits.' We will clarify this point in Section 3.2.3.

Based on previous studies reported in the literature and at sites within Malcolm Knapp Research Forest (Moore et al., 2005; Guenther, 2007), we anticipated that bed heat conduction would be a secondary term in the energy budget and therefore devoted more resources to estimating the other fluxes. We performed the spot measurements to provide a basis for confirming that bed heat conduction is indeed a secondary term.

– 12964, Line 9, hyporheic exchange. What mechanism is being investigated in this section? Is hyporheic exchange here only referring to short residence exchange where water travels from the stream to the bed and returns to the channel. Is it explicitly excluding groundwater exchange? Is it assumed that that water moves into the streambed at the surface water temperature and returns (on average) to the reach at the mean temperature measured in upwelling piezometers (or measured at thermocouples near the piezometers, is so, at what depth)? How can you be sure that the upwelling piezometers are not influenced by groundwater discharge and groundwater temperatures? Is your measure of hyporheic exchange static (some sort of average condition) or does it vary over time? If the latter, how is temporal variability estimated? Estimating hyporheic zone processes is clearly extremely challenging, especially over larger spatial and shorter temporal scales and it is extremely interesting that it has been attempted here. However, a bit more additional detail in the methods and a bit more discussion of the hyporheic exchange component of the energy budget in the discussion section would be useful. I would have thought that this would be one of the least certain of your energy exchange estimates given all these difficulties. It is also important to note that these estimates of hyporheic exchange will be based on assumptions of darcian saturated flow and will not include turbulent exchanges which can be large at shallow depths.

RESPONSE: See general response above.

– 12966 When you calculate Jadv, are you calculating this based on instantaneous differences in discharge and temperature (minus surface and friction energy exchange) between gauging stations that contain the study reaches or is there some sort of lag to account for travel time across the reach?

RESPONSE: We will clarify that Jadv is calculated based on the difference in J at the lower and upper reach boundaries calculated based on ten minute values. We did not explicitly account for travel time along the reach, but we did calculate the change in storage heat within the reach over those ten minute time intervals, which was found to be negligible. We will revise the ms to clarify this point.

– 12968 lines 8-12 Is there a risk in assuming that the measured values from a single point are 'the correct values'ÂÍ when the hemispheric photographs and associated modelling could be assumed to provide a more spatially averaged value than a fine scale point measure?

RESPONSE: As we pointed out in response to an earlier comment, the reviewer does not appear to have fully understood our approach to radiation modelling. Therefore, we

will revise this section to make our approach more clear to readers.

The net radiation model evaluation reported here is based on measured net radiation at each of the fourteen evaluation locations (note that we need to change the reference to 'eight' sites/locations in this section, since it was actually fourteen sites, as outlined in section 3.1.4). Net radiation was modelled for each of these sites using a hemispherical image that was taken at the same location. Therefore, what is being evaluated are observations and modelled net radiation at fourteen individual points. For the energy budget calculations, we modelled net radiation using the 34 and 48 hemispherical images captured at the harvest and forest reaches, respectively. We then averaged the modelled values for each reach for use in the reach-scale energy budget analysis.

– 12968, line15. Did VHG vary with depth? Wad there any spatial pattern associated with the VHG measurements? Could you plot temporal variability in estimates of hyporheic and resulting energy exchange or is this just presented to show that it was a small effect?

RESPONSE: See general response above. The primary purpose of these data were to show that the hyporheic energy flux was a secondary heat budget term. In order to keep the focus of this paper on the energy budget analysis, we have decided not to include these additional VHG data.

– 12968, line 24. Useful to remind the reader that these were temperatures observed up to 0.3m depth?

RESPONSE: We have chosen not to include a reminder here, as it breaks up the flow of the sentence. We believe that Thyp is adequately defined in section 3.2.3.

– 12968, lines 26-27. Could this not just be due to increases in turbulent exchange at shallow depths under higher velocities?

RESPONSE: It is possible. However, piezometers were installed to depths of at least 20 cm and due to the well armoured and poorly sorted bed material, turbulence-driven hyporheic exchange at these depths is likely minimal.

– 12969, lines 5-8. How was the bed conduction calculated? Was it calculated for each bed temperature location and then averaged over days and locations? Were there any temporal (including daily) patterns in bed conduction? What was the range of estimates of the effect of bed heat conduction?

RESPONSE: Section 3.2.3 outlines how bed heat conduction was calculated. We have revised the manuscript to emphasize that only spot estimates of this term were made during the study, since this seems to be a primary source of confusion. Bed heat conduction was calculated for each location. To section 3.2.2, we have added 'For bed heat conduction calculations, temperature gradients between the stream and bed were small during all spot measurements and across all locations.' to clarify that bed heat conduction is based on spot measurements. Because these were spot measurements, we did not document diurnal patterns. However, in the discussion, we did draw upon previous studies to help estimate realistic bounds on the bed heat conduction, which supported our argument that it was a secondary term in the energy budget.

– 12969 Lines 17-20. Do you think these modelled values of energy flux due to friction are realistic? If so, surely you would see warming under higher discharges?

RESPONSE: The energy flux due to friction is realistic because Equation (10) is a physics-based relation that represents the rate at which potential energy is dissipated per unit length of channel. There will be some uncertainty associated with the slope of the reach; however, the theory behind the calculation of this term is sound. We did observe some downstream warming under higher discharges (Fig. 3). In addition, although the friction term was large compared to the sensible and latent heat fluxes, and comparable to net radiation, it is largest during high discharge, which is when lateral advection overwhelms both friction and net radiation.

– 12969, line 21, Why are hyporheic flux and bed friction excluded from these plots?

RESPONSE: Friction is included in the plots. Hyporheic flux and bed heat conduction are not included since only spot estimates of these fluxes were made.

– 12971, Do you think that the study of historical data adds a lot to this paper? The paper would be sufficient without section 4. If you are to include this section, then sections 4.1 and 4.2 should be included in the earlier methods section. At present this feels like a new study starting towards the end of the paper.

RESPONSE: We believe that the historical study is an important complement to the energy budget study. The energy budget study provides the process-based understanding to establish the importance of lateral advective fluxes associated with runoff (sourced from rain, snowmelt or both) as critical controls on stream temperature dynamics during winter. A limitation of most detailed field studies, including ours, is that they are short-term studies, typically covering one or two years, and thus include a limited sample of precipitation events. With the historical study, we see a consistent difference in stream temperature response to rain-on-ground versus rain-on-snow events across thirteen winters with variable snow and weather conditions. This consistent pattern, combined with the energy budget study, provides robustness to our conclusions about the role of snow cover on stream temperature during rain events. We have added the following to section 4 to highlight the complementary nature of the historical study: 'The historical study complements the energy budget study by addressing the role of snow cover on stream temperature during rain events by extending the limited period of study of the detailed energy budget analysis and sampling more precipitation events over variable snow and weather conditions.'

We considered structuring the manuscript as suggested, but the methods and results felt disjointed. We will add text earlier in the revised manuscript to help guide the reader through the manuscript structure (as suggested in an earlier comment).

– 12975 While it is interesting that this study has attempted to quantify the effects of hyporheic exchange on heat fluxes, clearly this is extremely challenging and the results that are shown are dependent on particular definitions of hyporheic exchange(surface water that passes into the streambed and returns to the stream), assumptions that have been made, simplification of processes and limited consideration of spatial and temporal heterogeneity. It would therefore be useful to qualify the statements re: hyporheic exchange and remind readers of the assumptions that underpin these findings. Presumably this is at best a very crude first approximation assuming darcian saturated flow, averaging effects over the reach, assuming a simple distribution of flowpath lengths constrained within the reach etc etc. It may also be worth discussing options for improving estimates of this flux in the future through tracer modelling and observation approaches?

RESPONSE: See general response above.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 12951, 2013.