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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

Anonymous Referee #2

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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.

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?

HESSD

10, C6834–C6839, 2013

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.

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?

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?

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

12962, ca. L20, so it was assumed that the point measure of net radiation was "correct" and the hemispheric images calibrated to obtain best agreement with the point

HESSD

10, C6834–C6839, 2013

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)?

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

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.

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?

HESSD

10, C6834–C6839, 2013

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

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?

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

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

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?

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?

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

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.

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

HESSD

10, C6834–C6839, 2013

(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?

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