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Discussions

Interactive comment on "Stream temperature prediction in ungauged basins: review of recent approaches and description of a new physically-based analytical model" *by* A. Gallice et al.

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There has been increasing interest in and concern about stream temperature among both researchers and aquatic resource managers, particularly in relation to the potential effects of climate change, water management and land use on habitat for cold-water species such as salmonids. Given the relative dearth of stream temperature records, particularly multi-year data, there has been interest in the development of models for predicting stream thermal regime at both catchment and regional scales. Given the challenges associated with data requirements for physically based models, most efforts have focused on the development of statistical models. Although statistical models are

often adequate for characterising current thermal regimes, there are questions about their validity for estimating the effects of climate change, water management and land use.

Within this context, the manuscript by Gallice et al. represents a solid contribution to the literature on stream temperature prediction that is worthy of publication in HESS following some revision. They have introduced a stronger process basis into a predictive model, and the lessons learned will be a valuable point of reference for future work.

Following are some comments the authors should consider as they revise the manuscript.

1. Section 1.1 and 2.1. I recommend that the authors broaden the context by referring to studies that have applied deterministic models at the scale of a medium-scale catchment, including SNTEMP (Bartholow, 1991; Mattax and Quigley, 1989), a model based on the HSPF (Hydrological Simulation Program – FORTRAN) model developed by the US Environmental Protection Agency and the US Geological Survey (Chen et al., 1998a, 1998b) and CEQUEAU (St.-Hilaire et al., 2000). These models all have the capacity to generate spatially distributed predictions of the annual cycle of stream temperature. Another study that should be referenced is Allen et al. (2004), who developed a deterministic model for spatially distributed stream temperature prediction. Although Allen et al. focused on summer maximum temperature, the approach could be adapted for year-round application.

2. The review of stream temperature modelling in Switzerland seems a bit out of place in an international journal. I recommend that the authors add a sentence or two to set the Swiss experience into a broader international context. For example, what is different about Switzerland that sets it apart from other geographic settings in terms of what can be learned about stream temperature variability and modeling?

3. Section 1.5. It should be mentioned that a major constraint on the inclusion of predictor variables is the availability and reliability of data sources. It is particularly

challenging to quantify riparian shading based on the coarseness of vegetation data sets that are available in many jurisdictions, and the difficulty of representing the seasonally changing effects of deciduous vegetation. This point could also be reflected on in the discussion in the context of the model's inability to distinguish among different buffer widths for characterizing riparian vegetation.

4. p. 4091, line 6ff. This reference to social science seems unnecessary. I would argue that the need to calibrate any model in any discipline is an admission of inadequate knowledge about a system or inability to characterize its boundary conditions. I recommend that the authors begin the paragraph with a statement that expresses the motivation for this work in the context of the relative strengths and weaknesses of statistical models (e.g., see my introductory comments, above).

5. A number of assumptions are made that are likely not to be valid based on a priori reasoning. To what extent might the lack of validity of the assumptions have limited the model's performance? Three examples follow.

a. p. 4094, point (v). In fluvial geomorphology, stream width is commonly modeled as a power-law function of discharge, with a typical exponent of 0.5, which would generate different scaling than the assumed proportional relation between w and Q.

b. The lateral inflow rate, q_l, would be expected to increase with elevation, especially during the spring, when upper elevations experience snowmelt while the lower elevations do not. This would introduce a covariance between q_l and T_l that would not be included in the spatial average along the stream network.

c. The radiation term is a linear function of several components, but process-based reasoning indicates that there should be interactions (i.e., product terms).

6. The authors state that monthly net radiation is dominated by solar radiation (p. 4100, line 1ff). Is this true? For example, in a heavily shaded stream, I would expect incident longwave radiation to be larger than incident solar radiation at the stream surface even

during summer.

7. I like the authors' approach to model testing, which, according to the hierarchical approach promoted by Vit Klemes (1986), could be termed "split sample," "proxy basin" and "proxy basin split sample." The authors could consider including a differential split sample and a proxy basin differential split sample approach to provide a further assessment of model robustness. In the differential split sample approaches, the record is split based on climatic conditions. For example, the model could be calibrated for wet/cool conditions and then tested on dry/warm conditions. This test might be revealing about the relative merits of the analytical and empirical models.

8. The authors used all data for model selection, including the test data set (p. 4113, line 8). Would different models have been selected had only the calibration set been used in this first step?

9. The authors refer to their model as "physically-based." Given the high degree of parameterization and the many simplifying assumptions, it is probably more accurate to refer to the model as "analytical."

10. Finally, I have some editorial comments.

a. p. 4093, line 3. The authors use sigma to represent the stream surface heat flux, and later for the standard deviation of monthly stream temperatures. I suggest using a different symbol for stream surface heat flux (e.g., phi, to be consistent with later usage).

b. I found it difficult to follow the description of the model and had to read it several times. For example, the authors refer to measured solar radiation but do not explain where it was measured or how it was processed, except by saying it was assumed to be a linear function of elevation. Also, the authors present Eq. (16) and then discuss parameter estimation before explaining how the variables were quantified. I recommend that the authors attempt to make the model description simpler to follow as they revise the manuscript, perhaps by changing the order of presentation (e.g., describe data sources prior to describing the models).

c. p. 4410, line 22. The authors refer to groundwater "infiltrating" – "discharging" would be more appropriate.

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