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Hydrology and Earth System Sciences
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.

A. Gallice et al.

aurelien.gallice@epfl.ch

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We wish to thank the referee for his/her comments that will help sharpening our manuscript, and for his/her interest in our work. Below is our response to the remarks and issues raised by the referee.

1. We thank the referee for the list of references. We were actually already familiar with these studies, as we are currently working on the development and application of a spatially-distributed hydrological model with stream temperature prediction abilities.

When presenting the results of our current work in a publication, we will certainly not omit to cite the suggested models. In the context of the present paper, we however think the focus should remain on stream prediction in ungauged basins using statistical techniques. This also fits with the revisions we intend to bring to the paper, namely describe our work as a statistical model whose structure derives from physical principles, rather than as a physically-based model (see points 4 and 9 below, and our response to Anonymous Referee #1).

2. We acknowledge that a section about Switzerland might seem a bit awkward in the general introduction. We therefore propose to broaden the scope of this section to the entire Alps. This will certainly be of more interest to the hydrological community.

3. We thank the referee for this suggestion. In the revised manuscript, we will add a few sentences about the availability and reliability of data, and discuss it in Section 6 in the context of riparian shading.

4. We acknowledge that the reference to Social Sciences is unnecessary. We will modify the text to motivate our work as the referee suggests, also changing the perspective on our work so as address the referee's 9th comment and the general comment of Anonymous Referee #1. We will thus introduce our goal as the investigation of the potential gains in integrating physical principles into the statistical model structure, rather than qualifying our work as being physically-based.

5. The referee raises an interesting point about the implications of the simplifying assumptions. We intend to follow his/her advices and discuss the validity and limitations of each approximation. Regarding point c), we acknowledge that interaction terms should be included in the regression model. However, we aimed at limiting the number of model variants to be evaluated, so as to keep the problem tractable from a numerical point of view. This motivated our choice to discard interactions.

6. We thank the referee for pointing out an erroneous statement. Citing the work of Johnson (2004), Webb et al. (2008) indeed report that in a watershed in Oregon, USA,

“an open reach under full sun [was measured to] experience[. . .] a net energy gain of 580 Wm–2 but a reach under full shade [. . .] a net loss of 149 Wm–2” at midday in July. We will suppress the erroneous sentence.

7. We appreciate the referee’s comment and thank him/her for this proposition. It would indeed be valuable to calibrate the models on a particular period of the year and evaluate it on another. This would however be relevant only for the “physically-based” model, since the “statistical” one simulates the entire annual curve of stream temperature at once. Calibrating the “statistical” model on given months of the year would introduce a strong bias in its estimation of the annual mean and standard deviation of the monthly mean stream temperature. As such, this procedure would penalize it because of its structure rather than reveal its actual performances, and might therefore erroneously make the “physically-based” model appear better in comparison.

8. The same question has been asked by Anonymous Referee #1 in his specific comments. We actually followed Burnham and Anderson (2002) in using both calibration and validation data sets to select the best model. The citation is present a few lines above in the manuscript, but we will repeat it for the sake of clarity.

9. Anonymous Referee #1 raised the same issue in his general comments. We propose to change the nomination of the model from “physically-based” to “physics-derived statistical”, unless the referee has a better suggestion.

10. a) We thank the referee for this suggestion regarding notation consistency, and will follow his/her advice.

b) We acknowledge the model description might be difficult to follow. We will perform the changes as proposed by the referee, namely describe the data sources prior to explaining the models. We will also attempt to clarify the model description.

c) Following the referee’s advice, we will change “infiltrating” into “discharging”.

REFERENCES

Burnham, K. P. and Anderson, D. R.: Model selection and multimodel inference: a practical information theoretic approach, Springer, 2002.

Johnson S.L.: Factors influencing stream temperatures in small streams: substrate effects and a shading experiment, Canadian Journal of Fisheries and Aquatic Sciences, 61, 913–923, 2004.

Webb, B. W., Hannah, D. M., Moore, R. D., Brown, L. E., and Nobilis, F.: Recent advances in stream and river temperature research, Hydrol. Process., 22, 902–918, 2008.

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