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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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We wish to thank the referee for his/her comments that will contribute to improve the quality of our manuscript. Below is our response to the remarks and issues raised by the referee.

General Comments

The referee questions our general statement that the model is physically-based, pointing at the fact that it actually involves regression. We certainly acknowledge this comment and fully agree with the referee. The analytical solution to the heat-balance equation leads to an expression, certain terms of which cannot be readily evaluated. These terms are approximated using linear (and nonlinear) regression expressions of known physiographic properties of the catchments, hereby introducing statistical aspects into the model. Moreover, among the different regression expressions used to approximate each unknown term, the best one is selected using the Akaike information criterion, which is a well-established statistical technique. To answer the referee's comment, we therefore propose to modify the general tone of the manuscript. For example, we intend to refer to our model as "physics-derived statistical model" instead of "physically-based model" — unless the referee has a better suggestion. We will also remove the assertion that our model avoids the typical limitations of statistical models. More generally, the main message of the article will be changed; it will be presented as investigating the potential gains resulting from the derivation of the structure of a statistical model based on physical considerations.

Although probably a bit overstated, our claim that "the selection of predictor variables relies on the experience of the authors in statistical models" is, according to us, justified. We agree with the referee's comment regarding the fact that objective or semi-objective methods can be used for variable selection. This is exactly what we did when selecting the best model among the different variants considered. However, these methods can only be applied to a predefined set of variables to be tested. And the definition of this latter set actually relies on the experience and understanding of the author. Moreover, the use of these automated techniques should, in our opinion, not be substituted to physical understanding whenever possible. Some variables may be selected which actually appear to incidentally have some correlation with the predictand, but do not have any physical relationship with it. One might rightfully argue that our discourse is ambiguous, criticizing the statistical techniques on the one hand, but relying on them on the other hand. In the present case, we are limited by the fact that we aim to develop a model for prediction in ungauged basins, and we are hereby limited to the use of the only variables which are available from classical spatial data sets. Not knowing the relationship between these regional scale variables and the unknown terms we wish to compute, we have to rely on statistical techniques.

From our understanding, the referee's statement that "parcimony was only used for the model variants" refers to the variants of the physically-based model. If this is indeed what (s)he meant, we would like to kindly mention that this is actually not the case. As the referee pointed out in line 530 (see Specific Comments below), we limited the number of parameters also in the statistical model in order to avoid equifinality issues. The maximum number of parameters was fixed to 6 in order to match the maximum number of parameters in the "physically-based" model and hereby provide a more even comparison between the two models. It is however true that parcimony is not discussed in Section 6; we propose to add a few lines on this subject in the manuscript to address the referee's comment.

Specific Comments

L. 78: The referee considers our statement to be too strong. We will modify it slightly, emphasizing that our conclusion was based only on the few intercomparison studies that have been conducted on this subject.

L. 185: We propose yet another formulation: "They computed the characteristics of the typical thermal regime of each group."

L. 210: See our response under Gerenal Comments.

L. 236: We will change this paragraph. We actually do not use only linear relationships of available quantities to approximate all unknown variables. A power function was tested to express the fraction of discharge originating from lateral inflow (Eq. (17)). It is however true that the remaining unknown quantities (source water temperature, lateral inflow temperature and solar radiation heat flux) have been estimated using linear functions. This choice was motivated by our lack of knowledge regarding the nature of the relationships between the predictand and the predictors, and by our wish to avoid unnecessary complications. We have tried power functions as well, but the non-linear solver failed to identify the parameter values due to convergence issues. Given that power expressions were about the simplest functions we could think of after linear expressions, we did not attempt at investigating more complex relationships. To further justify our choice of linear functions, one might consider that the main non-linearities have already been captured by the general form of the model, namely the analytical formula obtained by solving the heat balance equation. Such non-linearities appear mainly in the respective expressions of the weighting factors ω_1, ω_2, ω_3 (Eqs. (9)–(14)), and in the definition of the distance-weighted averaging operator $\langle \cdot \rangle_{\mathcal{L}}$ (Eq. (A9)). Based on this, it might be assumed that the specific form of the expressions used to approximate the unknown terms would have a minor effect compared to the general structure of the model.

L. 315: We will add the following sentence to discuss the potential repercussions of our simplification: "This approximation is expected to be less restrictive in larger catchments, due to the greater distance between the sources and the outlet point, and to the averaging effect resulting from the large number of sources."

L. 404: The referee raises an interesting point. While computing air temperature as a function of altitude in each catchment, we limited ourselves to meteorological stations located at less than 20 km from the watershed outlet. On the other hand, $L_c = 32$ km in the model used to estimate the standard deviation of water temperature (see Table 6). This apparent discrepancy is actually not considered to be a limitation, due to the fact that only 2 of the 26 selected catchments have an area extending beyond the 20 km-radius disk centered on the catchment outlet. As such, increasing the range of considered meteorological stations would not improve the quality of the interpolation but in 2 cases. Moreover, the stream networks in those last 2 catchments have a marked sinuosity, implying that all their stream segments at a streamwise distance less than 32 km from the outlet are actually located within the 20 km-radius disk centered on the outlet point. The proposed methodology for air temperature interpolation is therefore not considered to be at odds with the rest of our work.

L. 421: By "setting at least two [...] to zero", we meant that all linear functions involving any possible combination of either one or two of the predictors variables $\{\phi_{sw,inc}, f_s, \theta, f_f\}$ were considered for approximating the term $\gamma\phi_r$. The arbitrary choice to consider expressions with at most two terms (plus the intercept) was made in order to restrict the number of model parameters. We acknowledge the sentence was not very clear and will change it in the manuscript. We will also emphasize the fact that the restriction to a maximum of two terms is arbitrary.

L. 530: While determining the structure of the multilinear model, we restricted ourselves to a maximum of 6 predictor variables. This choice was made in order to avoid over-parametrization, but also to ensure that the number of parameters in both models (the physically-based one and the statistical one) were about the same. This guaranteed a more even comparison between the two modeling techniques. We also discarded the step-wise approach, since it may fail to identify the best set of predictor variables (Miller, 2002) and is subject to some criticism (Harrell, 2001).

L. 595: We thank the referee for the correction and will propagate the change throughout the manuscript.

L. 620: We agree with the referee that stream temperature may reach 30°C in shallow, open areas. However, it was rather clear for some temperature series that such high values were reached as a result of the temperature sensor being out of water. We

therefore preferred to remove all data points above 30°C, potentially discarding correct data, instead of calibrating the model over erroneous data.

L. 669: The referee suggests doing a bootstrap on the validation stations. We acknowledge this would be an improvement over the current validation procedure. We actually tried it, but had to abandon it because of too high computational requirements. As a matter of fact, Burnham and Anderson (2002) state that bootstrapping, whenever used in association with model selection, should be performed at the level of model selection itself. We therefore tried to perform bootstrapping on the stations used to select the best model according to the modified Akaike information criterion. However, this proved to require too much computational resources due to the high number of models to be evaluated and to the large number of bootstrap samples to be considered (Burnham and Anderson recommend considering at least 10'000 samples for bootstrapping).

L. 713: We followed Burnham and Anderson (2002) in computing the value of the corrected Akaike information criterion over the entire data set (calibration + validation sets). The reference is cited a few sentences above in the paragraph, but we acknowledge it is not clear that we followed their recommendation in doing so. We will cite their work at the end of the sentence to make it clear.

LL. 1015–1020: According to the referee's suggestion, we will move these few lines into the discussion (Section 6).

References

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

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