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Interactive comment on "Climate and basin drivers of seasonal river water temperature dynamics" *by* C. L. R. Laizé et al.

Anonymous Referee #1

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This manuscript aims to develop a better understanding of the climatic controls on stream temperatures. Based on empirical models, long-term data from 35 sites across Britain are analysed. The objective is interesting and the paper in general worthwhile. I would have, however, appreciated a bit more clarity in the description of the modelling approach and somewhat more depth in the discussion of the results (e.g. what do the results imply? What can be learned from the results?). In places the manuscript comes across as rather sloppy (wrong numbering of tables and figures, missing explanations, incomplete referencing and very little information/discussion about the comparability of data from different sources). Please find below some detailed comments:

(1) P.2,l.26-27: irrelevant. Can be condensed.

(2) P.3,l.19-20: possible, but please be more specific and add some reasoning.

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[Discussion paper]

(3) P.3,l.21, figure 1: perhaps add the symbols from equation 1 to highlight more which process is related to which heat flux

(4) P.3,l.31: Hrachowitz et al. (2010) would also fit in nicely here.

(5) P.4,l.4-6: I found this a bit exaggerated. There are in fact quite some studies that consider a range of catchment properties (e.g. Isaak and Hubert, 2001; Scott et al., 2002; Moore, 2006; Nelitz et al., 2007; Hrachowitz et al., 2010; Isaak et al., 2010). Please tone down and add at least these references.

(6) P.4,l.25: table numbering is wrong. Table 2 is not referred to at all in the manuscript.

(7) P.4,l.29: "addresses" is unclear, maybe better to use "limits" or something similar

(8) P.5,l.20: figure numbering wrong: figure 3 referred to before figure 2. Please also make this figure a bit more informative. Provide basin/river names and potentially include elevation information. Please also clarify why some observation sites are far from streams (e.g. in insets 2 and 3).

(9) P.5,l.26: please provide more information on the actual data acquisition. Were the recorded values instantaneously measured temperatures or the averages over the logging intervals? How were the different sensors from the different studies placed and protected against radiative overheating? What about systematic uncertainties introduced by differential vegetation- and/or topographic shading at the different sites? Were the recorded data from the different studies pre-processed differently (e.g. filtering out overheating extremes)? What do different measurement precisions and accuracies of these different data sources imply for the analysis here? any systematic errors to be expected? And if not, why?

(10) P.6,l.6-7: Please be a bit more specific. How was precipitation regionalized based on rain gauge data? Kriging? IDW? Thiessen? Other methods?

(11) P.6,section 2.2: what about the uncertainties arising from the modelled climate data? How do they propagate through the temperature analysis here? Do they affect

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the overall interpretation?

(12) P.6,l.20-22: what is the reasoning behind investigating seasonal averages? why only these? What is their ecological relevance? What about seasonal average daily (or 7-daily) maxima and minima? Would these not be more instructive? Just wondering.

(13) P.7,l.4-5: where is this section? I cannot find it. This is relevant information and needs to be shown.

(14) P.7,l.5: what is meant by "permeability"? permeability of what? How was it determined?

(15) P.7,l.6: not clear what is meant by this sentence

(16) P.7,l.23ff: how was the spatial correlation structure between sites along the same rivers accounted for? What was the flow distance between the sites closest to each other?

(17) P.7,l.24: it should at least be mentioned that linear models, in particular for the air-water temperature relationship, are oversimplifications and that for example logistic models can much better account for effects such as evaporative cooling (e.g. Mohseni et al., 1998)

(18) P.7ff, sections 3.1,3.2: I found this quite hard to follow. I would like to encourage the authors to invest some more effort to describe this critical part of their analysis more clearly.

(19) P.8,l.21-25: so how were the various combinations tested? Stepwise regression or best sub-set regression or some other method? What is AICc? How was it corrected for small-size datasets?

(20) P.10, section 4.1: not clear which explanatory variables were used in the individual models. All?

(21) P.11,l.1: what does "adequately" mean? Please provide R2 and p-values.

(22) P.11,l.3ff, section 4.2: one thing that is completely missing here but that may be of considerable relevance is the potential collinearity (or correlation) between the predictor variables, which can potentially result in highly unstable and misleading model results. It will therefore be necessary to quantify the collinearity and evaluate to which degree it actually influences the results.

(23) P.12,l.2: please clarify: are the percentage contributions in fact the proportions of the explained variance?

(24) P.12,l.4, figure 5: please provide a unit for the y-scale in the figure. The unit of the x-scale (%) seems to be wrong here. In addition, please be more specific: % of what?

(25) P.13,l.19ff: this needs to go into the methods section. Please also clarify why exactly these properties were chosen and provide a table with the relevant values.

(26) P.13,l.20: elevation not only related to wetness but clearly also to air temperature

(27) P.13,l.26: area is proxy for discharge and thus for thermal capacity, but is also linked to elevation

(28) P.13,l.27: what is the reasoning behind using HOST/permeability? What is it expected to explain?

(29) P.14,l.8: please also provide the individual p-values!

(30) P.14,l.14-15: this is a sweeping generalization which needs to be toned down

(31) P.14,l.16: why should there be more small basins at higher elevations? Channel formation does not have anything to do with elevation, but rather with contributing area and local slope. There may be some correlation with elevation but it is not generally valid as posed here. what, however, is true is that, necessarily the opposite is true: there are more larger basins at lower elevations.

(32) P.16,l.5-6: this is possible, but not sufficiently substantiated by data here. I would argue that it is equally likely the indirect correlation is merely a model artifact without

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physical meaning (and potentially related to collinearity).

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