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

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It is worth clarifying that we did not aim to produce a better predictive model of water temperature, but rather, the modelling exercise was a mean to gain better understanding of the large-scale spatial and temporal variability in climate–WT associations, and of the influence of basin properties on these associations. The modelling techniques (multi-level modelling and multi-model inference) are definitely novel in their application to water temperature. In particular, we could analyse data both from at site scale and at national scale at once with multi-level modelling. The sites covered a reasonably wide range of catchment types. The combined wider spatial patterns and site-specific responses related to basin properties help unraveling the relative influence of climate vs land surface control across scales.

(1) Results, especially in tables and figures, are not presented in a way that enables easy interpretation by the reader. Table 6 means nothing to anyone but the authors. Table 5 – why is the FEH descriptor included, except for reference to Table 6? Why were the selection of descriptors used? Greater insight on which descriptors were included would be helpful. Section 4.3 for instance, refers to the abbreviations of FEH variables, but it would be much fewer words to just state the actual variables in text, and indicate FEH variables in parentheses

We think this comment, and two others below, as well as some comments from Reviewer #1 stem from the way we introduced the basin properties. As we stated in response to Reviewer #1 comment 25, we would move the bulk of Section 4.3 back to Methods, thus streamlining and clarifying which basin descriptors we used. Re Table 5, we included the FEH descriptor to highlight the fact that the basin property (eg elevation) is characterised by a specific descriptor (eg ALTBAR); many different descriptors could be used to characterise elevation. Then, since these descriptors are indeed used for the results featured in Table 6, we thought it would help readers to make the connection. We believe we explain Table 6 clearly enough in the text. Regarding the latter point of FEH descriptors abbreviations, they are actually not abbreviations as such but short names (except for BFIHOST), so their explicit names is already given (eg ALTBAR = mean basin elevation above sea level). To clarify things, we swapped order of full name and FEH short name.

(2) The introductions to each section are not helpful, but I leave this up to the authors. I find that they detract from the reading of the manuscript.

Experience with past papers showed that some readers benefit from these section introductions. We are inclined to keep them unless there is a need to reduce the length of the manuscript.

(3) Sites with very different time scales of measurement where included. I get why this was done – there is not a lot of stream temperature data (a problem I am also having!).

However, I'd like to know more about what is the effect? Were sites with 15-minute versus weekly and monthly data treated differently? With so many sites, it would be worth testing if 15-minute data were treated in the same way as weekly or monthly sites, what the effect on conclusions would be? If sites from weekly/monthly data were excluded, are conclusions different?

The discrepancy in data time scales is handled by using multi-level modelling. So, for example, if sites based on 15-min data behaved differently from sites based on weekly data, the model would correct for that. However, it would not explicitly investigate what the effect of one over the other. We added a mention of this as possible future research.

(4) Unclear what kind of variability in terms of basin/river properties your paper explores – a figure to this effect would be a good contribution. For instance, where else would your results be comparable to? This would be helpful to know both in terms of stream temperature regime and basin properties.

In Section 4.3, we have included a paragraph giving the ranges of basin properties for the 35 sites. They do provide a fairly wide range of basin types in the UK. The original data sources include a mix of lowland permeable (eg from LOCAR, Tadnoll; the UK has most of its aquifers in lowland regions) and impermeable basins, upland impermeable (eg form Plynlimon), as well as small to medium basin sizes. Sites from the AWMN cover all types. The gap in coverage may be in terms of large basins (ie >1000 km2) but these are far less common in the UK than in other countries. If needed, we could provide the detail properties per site (table) to be included as appendix or supplemental material.

(5) Magnitude of fluxes depend not only on climatic variables, but also on water temperature. Is model able to include this interaction, as it is a key determinant of evaporation/condensation and convection/conduction?

As it stands, the models are at their core linear regression with water temperature as the dependent variable. Feedbacks cannot be built-in. This would require a different

type of method, possibly bespoke models handling iterative calculations. However, we explored this with the outputs from Fig 5 showing how the contributions of different predictors change with water temperature.

(6) Need more information on descriptors. They're included haphazardly. Don't even know which predictors are included in the model.

See previous responses to comments (also to Reviewer #1) about Section 4.3 (we believe the proposed changes to the way basin properties are presented will address this).

(7) Pg 3, Lines 10 – 20 – variables should have subscripts

Text amended as suggested.

(8) Pg 3, Line 28 – misplaced comma

Comma moved to its proper position.

(9) Pg 4, line 6 – consider the role of basin properties with respect to what? There's several papers in the US that have investigated the role basin properties may play in determining the stream temperature regime – they do so from an empirical perspective

Sentence amended page 4 l 7-9 (also response to see Reviewer #1 comment #5).

(10) Pg 5, line 3 – it's not clear to me what you mean by 'not losing any information'

This refers to the loss of information due to class-level averaging with classification-based analyses (already covered in more detailed a few paragraphs before on page 4). We inserted a reminder/clarification page 5 line 5.

(11) Pg 6, section 2.2 – what impacts do you think using a 1km square meteorological dataset may have on your proposed conclusions? Are there any sites where microclimate could play a role?

We agree there may be micro-climate effects (one co-author published several papers

based on field site monitoring showing the impact of shading, etc.), but the focus of this study was the wider spatial patterns, which is quite novel, so we did not investigate this further. In addition, based on the information we had, we do not think there was any site where micro-climate effect was conclusively present. Obvious. However, we mention this in the discussion (for example, highlighting how shaded river may behave differently from non-shaded). We propose to add a mention of this point in the conclusion.

(12) Section 3.2 was difficult to follow and written confusingly. Comments were included in parentheses and not explained fully. The importance of AIC weights was introduced, but there was little explanation of what this value tells the reader (does 'relative importance' mean a better model? More trustworthy model?)

This particular section has been revised and streamlined to provide more clarity (see reviewer #1 comment #18).

(13) Some missing words in section 3.3

Section has checked and revised.

(14) Page 10 line 24: why was no predictor included for spring?

As explained in that paragraph, for spring, the model getting the lowest AICc (the best model) was the model with random intercept only (ie the only difference between sites is with regards to the mean water temperature; all sites have the same response slope for all predictors; hence "no predictor was included" in the random effects). We added a clarification that spring is a random intercept only ML model (page 10, l30). We also expanded section 3.1 (page 8, l14-19) to give more clarity regarding multi-level modelling, which in turns would help readers to understand what is meant in this paragraph.

(15) Abbreviations make the results difficult to adjust – I know what short wave radiation is, but every time I see SWR, I get confused!

We appreciate this problem, but we had to use abbreviation for the sake of conciseness (there are many references to the model predictors in the text), and following that,

consistency forces us to use the abbreviations all through out.

(16) Pg 17 line 1: Most other studies only use AT because it so well predicts stream temperatures. While your models demonstrate association, how much better do they predict stream temperature than air temperature alone? Furthermore, you use gridded AT data, which is available everywhere. I find it much less likely that AT is unavailable at a site with a suite of other climatic variables.

As stated, our objective was not to build a better predictive model but to understand the various climate-stream temperature associations. As such, we do not claim that these particular models do necessarily better than AT-WT models (and did not investigate this), but that using other climate predictors in addition to AT could be informative in some cases. We added a few words to avoid any misunderstanding on that point. Regarding the comment about AT being not available, it is true that if one uses gridded climate data, then AT is probably more likely to be available than some of the other variables: what we had in mind were field sites where air temperature measurements may be missing. This was a minor point however, so we deleted it from the sentence.

(17) Pg 17, line 27 on – please rephrase out of list form

Done.

(18) Figure 4 should be improved – it is difficult to read axes. Model fits should be included.

The size of the figure has been increased slightly to improve readability (figure has also been edited to amend one label that was not displayed properly). Model fits (conditional R2) have been added to text; given the conditional R2 are strictly speaking calculated for each model in a model set rather than for the average model (ie the average of all models in a model set), we think it is more accurate to leave the reader to gauge the average model fits visually than to add a mean R2 to the plots (see Reviewer #1 comment #21).

(19) Figure 5, please label the y-axes

The y-axis label ('%') was erroneously placed on the x-axis; figure amended.

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