

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

**Anonymous Referee #2**

Received and published: 12 November 2016

This article explores basin and climatic drivers of stream temperatures across the UK. While the authors do a nice job throughout stating what is novel with respect to the study, I have a hard time finding some of their results novel. They show that air temperature, and solar radiation, drive heat fluxes throughout the year. Their findings fall in line with 30+ years of stream temperature research. A potentially novel result is the inclusion of different climatic factors, and the modeling style that they use to include these factors. However, it is not clear what this information adds to predictive capacity for stream temperature across the UK. Does including these variables mean there is greater explanatory power? Tertiarily, they also relate models to basin properties. However, the basin properties that were included are not well described in the paper, and end up feeling tangential to the other results. I'm left wondering where the model(s) perform(s) well, and where they performs poorly, and how performance changes across

[Printer-friendly version](#)

[Discussion paper](#)



different scales. Can this approach we used to improve modeling of stream temperatures? This is mentioned briefly at the end. As it stands, showing that models identify climatic variables as important seems to confirm what we already know. Showing, again, that basin properties influence these results is also potentially not new. I'm also left wondering about some of the implications of their data (in terms of temporal and spatial extent) for their conclusions. Overall, this is clearly a well-developed idea that will advance stream temperature research, but I am left feeling confused about broader implications, the sites in question, and whether this type of approach gets us any closer to improving our empirical modeling of stream temperatures.

Major comments: -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

-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.

-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?

-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

[Printer-friendly version](#)

[Discussion paper](#)



your results be comparable to? This would be helpful to know both in terms of stream temperature regime and basin properties.

-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?

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

Minor comments: -Pg 3, Lines 10 – 20 – variables should have subscripts -Pg 3, Line 28 – misplaced comma -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 -Pg 5, line 3 – it's not clear to me what you mean by 'not losing any information' -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? -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?) -Some missing words in section 3.3 -Page 10 line 24: why was no predictor included for spring? -Abbreviations make the results difficult to adjust – I know what short wave radiation is, but every time I see SWR, I get confused! -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. -Pg 17, line 27 on – please rephrase out of list form -Figure 4 should be improved – it is difficult to read axes. Model fits should be included. -Figure 5, please label the y-axes

---

Interactive comment

[Printer-friendly version](#)

[Discussion paper](#)



---

Interactive  
comment

