Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2
https://doi.org/10.5194/hess-2021-450-RC2, 2021

Comment on hess-2021-450

Anonymous Referee #2

Referee comment on "Regional, multi-decadal analysis on the Loire River basin reveals that stream temperature increases faster than air temperature" by Hanieh Seyedhashemi et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-450-RC2, 2021

General comments

In this work, the authors used hydrological and thermal models to derive long-term (57 years) time series of discharge (Q) and stream temperature (Tw) across a wide European basin, the Loire River. Subsequent analysis of these time series revealed that rates of Tw increased faster than those of air temperature (Ta), and, importantly, that the highest increases in Tw were observed in correspondence with the highest increases in Ta and lowest increases in Q. The authors also observed a considerable mitigating effect of riparian shading in the upstream part of the basin.

The manuscript addresses a key concern in ecohydrology, namely the assessment of increasing trends of Tw in the last decades. Its novelty resides in the use of mechanistic hydrothermal models to derive trends of Tw and Q across a whole basin. Analyzing Tw and Q trends directly from the measured data series at the gauging stations would have indeed been possible, but it would have been limited to the few (if any) reaches were both Tw and Q were monitored for a sufficiently long duration (by the way, this is a remark that the authors might consider adding).

The presented analyses are very detailed and robust, and I particularly appreciated that the findings of this study were put into context with other recent studies on Tw trends across European rivers (Table 3). Moreover, the manuscript is well structured and well written, and hence I only have some minor specific comments to it. The only main concern I have is some lack of detail on the description of the hydrological and thermal models (see below). I also find that the manuscript is a bit too long, and I believe that its readability would increase if some of the less relevant results (say e.g. Figs. 6, 7, 9 and respective paragraphs commenting them) were moved to the supplementary material.

Specific comments

- L. 95-112: Many details on the implementation of the EROS model are missing. What are the free parameters of the model? What method for model calibration was used? How were Nash-Sutcliffe index and overall bias combined in the objective function? What was the rationale of choosing sqrt(Q) instead of Q in the computation of the Nash-Sutcliffe index? Was the output of this model validated with respect to Q time series not included in the calibration dataset?
- L. 113: If I understood correctly, the T-NET model does not have any free parameter, hence it does not require calibration. This should be stated explicitly.
- LL. 115-119: The authors state that the first step for Tw estimation is the computation of the equilibrium temperature (Te). This is the stream temperature at which the net heat flux across the surface of the water body is null, and is a useful concept when one aims at producing a simplified, (semi-)mechanistic temperature model where Te is expressed e.g. as a linear or logistic function of Ta, which allows discarding the exact computation of the various non-advective heat fluxes (latent, short-wave radiation, etc..). However, here the non-advective heat fluxes are exactly computed (this is not explicitly mentioned here but is reported in Beaufort et al., 2016), and Te is calculated as the Tw value that nullifies the sum of non-advective heat fluxes. This is technically equivalent to a fully mechanistic model where non-advective heat fluxes are included in the energy budget at a reach (or cross-section) scale. Thus, I find it a bit confusing to invoke the concept of equilibrium temperature here.
- L. 174: It is unclear why 72 stream temperature stations are mentioned here, but then validation is only performed on the 14 stations with long-term continuous data. Are the other 58 stations ever used in the analysis? If not, this information should be discarded.
- Figure 2. It would be interesting to have an estimation of the performance of the T-NET model in terms of RMSE (or MAE), in addition to the mean bias (as the latter can obviously be close to zero even when absolute errors are very high).
- LL. 260-261: "Indeed, where … for all seasons". This is an interesting observation, but, for the sake of fairness, one would need to check whether the majority of reaches where Ta trend > Tw trend also showed a decreasing Q trend.
- L. 274: "shifting by approximatively +2 ^{o}C". I find this unclear. Is the shift observed by comparing current Tw and Ta values with those observed at the change-points (~1988)? Please state this more clearly.
- LL. 288-289: Strahler order is significantly and positively correlated with Tw only in spring (and in summer only for HER A). The way this sentence is written, one is led to think that this correlation is strong more often than it is the case by looking at Fig. 9.

Technical corrections

- L. 58: "in meeting these goals"
- LL. 92-93: I think it would be clearer to mention "meteorological seasons", and then use the season names in lieu of the acronyms DJF, MAM etc. in the figures. This would make the figures much more intuitive.
- L. 108: if 1971-1974 is the warm-up period, then the calibration period should be 1974 (or 1975)-2018
- L. 109: Why is the number of subcatchments here (368) higher than 352 (mentioned in

L. 104)?

- L. 112: “were discarded”. Moreover, “(1958-1962)” is four (or five) years, not three years.
- LL. 140-143: Eqs. (3-5) can be condensed in one equation: SF = max((W_shaded)_left * vc_left, (W_shaded)_right * vc_right)
- L. 178: I suggest indicating the range rather than SD for drainage area values (when SD > mean, this does not make much sense)
- L. 201: verb missing. Perhaps “used to detect/assess synchronicity”.
- L. 227: “The trends of both modeled and observed Q”
- L. 247: “The highest (resp. lowest) Ta trend values”
- L. 248: In Fig. S6, the season with lowest Ta values seems to be fall, not spring, with 0% of reaches showing values > 0.4 ^{o}C/decade.
- L. 257: “The medians of Tw… than those of Ta”
- L. 268: “they are either warm and wet”
- L. 308: “strongly suggesting an effect on Tw” or similar.
- L. 319: “comparing trends… gives us a comprehensive view”
- L. 364: “The warming effect… seems more significant”
- L. 370: “an increase of >25%”. Increasing from 15% to 40% (this is what I believe the authors are referring to, see LL. 299-301) is actually a 267% increase. Perhaps it would be best to reformulate as “increasing riparian shading from 15% to 40%”.

References

Beaufort, A., Curie, F., Moatar, F., Ducharne, A., Melin, E., and Thiéry, D.: T-NET, a dynamic model for simulating daily stream temperature at the regional scale based on a network topology, Hydrological Processes, 30, 2196–2210, https://doi.org/10.1002/hyp.10787, 2016.