

Interactive comment on “Anthropogenic Influence on the Rhine water temperatures” by Alex Zavarisky and Lars Duester

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In this study, the authors analyze the effects of Nuclear Power Plants on river water temperature of the Rhine. The authors propose a multiple linear regression model where river water temperature is simulated based on air temperature and streamflow as predictor variables. Air temperature is evaluated through an averaging procedure that accounts for the geomorphology of the hydrological catchment. The intercept of the multiple linear regression models is used as a proxy for the anthropogenic impact

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on river water temperature and is compared to the time series of GDP and heat input from NPPs. The presentation of the methodological approach and of the results should be improved, both in terms of clarity and quality. In my opinion the robustness of some methodological aspects is weak (e.g., the use of a constant flow velocity, the interpretation of the multiple linear regression intercept as "indicator for industrial heat input") and the discussion of the results should be expanded and deepened. The literature review on modeling of river water temperature and assessment of anthropogenic impacts should be updated and the grammar and syntax of the manuscript should be checked carefully. Please, find below some specific comments.

Introduction by the authors: We would like to sincerely thank both reviewers for the comments and thoughts about our work and this manuscript. We think that the input significantly improved the manuscript.

Based on the reviewers comments and by reviewing the code once again transposed numbers were found in the coding. By correcting the calculation method ACC*w provides the lowest RMSE and largest NCS in three out of four station. At the same time we were able to further decrease the RMSE for the ACC*w calculation method. The reasons for the ACC*w resulting in lower RMSE compared to ACC or w only, is now described in detail in the methods section. Overall, the results (correlations, RMSE, NCS, ΔRBT_{calc}) changed only so slightly, that the scientific conclusion and the key messages were not influenced. This also visible in the attached track changes version of the manuscript.

Specific comments

Introduction:

The literature review on modeling of river water temperature should be expanded and updated including the most recent studies in this field. Besides "classical" deterministic and statistical models, there is a wide range of models based on machine learning techniques or hybrid physically-based/statistical approaches (e.g., Sahoo et al.,

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2009; Toffolon and Piccolroaz, 2015; Sohrabi et al., 2017), which have been emerging in the last years. Despite it is not recent, I suggest giving a look to the review paper by Benyahya et al. (2007), which provides a good overview of deterministic and statistical models used in the field of river water temperature prediction. Another useful and more recent paper is that by Gallice et al. (2015).

Comment: Another thorough literature search was undertaken and we added among other references, the references proposed the reviewers. The overview of water temperature models was extended in the introduction.

In addition, the authors should refer also to existing literature on the assessment of anthropogenic impact on river water temperature (e.g., Cai et al., 2018; Gaudard et al., 2018; Raman Vinna et al., 2018, just to cite some recent papers).

Comment: The publications were cross-checked. The input was included in the revision of the manuscript.

In general, I believe that the paragraph from P1, line 19 to P2, line 8, should be thoroughly restructured and revised, and the authors should be more precise throughout the text (e.g., at P1, line 22: I believe that the authors intend deterministic and statistical models here; at P2, lines 21-23, the sentence is unclear; at P2, lines 25-26, the comment is superfluous since in a multiple linear regression, such as the one used by the authors, these components are obviously neglected). P2, lines 7-8: I would rephrase this sentence in more general terms, because the amount of variance in river water temperature explained by air temperature and streamflow are strongly dependent on the case study (hydrological regime, season, etc.).

Comment: Thank you for the comments. We revised the whole introduction. The changes we made can be seen in the track changes version. P2 lines 25-26: We know that our model does exclude ground heat flux and friction. If the parameters are important they would appear most likely and unfortunately in the regression coefficient a_1 . However, a_1 is the basis of our analysis which should display the anthropogenic

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heat input We just want to say that we think these heat fluxes are negligible and do not interfere with our anthropogenic heat input. In this regard, the authors should expand the analysis of parameters a_2 and a_3 of their regression model. The second half of the Introduction (from P2, line 16) should be moved to the methods section and should be improved, as in its current form it does not clearly describe how the authors set up their analysis, especially concerning the definition and use of RBT as an "indicator for industrial heat input" and the time resolution of the data used in the multiple linear regression analysis. Figure 1 This figure should be updated with the location of the monitoring station and of the NPPs. The main course of the Rhine should also be indicated.

Comment: We changed Figure 1. In the introduction we give just a basis overview of our idea which is closely linked to the linear regression model. We moved some parts to the methods section. The detailed calculations are described in the methods section.

Section 2.1. I agree on the comment about accuracy and precision, however I wonder if the measurements are affected by instrumental drift and, in case, if the dataset has been corrected accordingly.

Comment: The data was verified by the data provider (e.g., by recurrent validation measurements, recalibration if needed or cross-validation). The data-set was screened for suspicious features. We stated this in the manuscript.

P3, line 9: this sentence is unclear. In general, I agree that water temperature is rather homogeneous at a river section if it has a compact geometry, while it may be non-uniform if the geometry is complex.

Comment: We know that the measured water temperature, especially in complex river geometries, is an on-spot in-situ temperature and could be different from a cross-section average T_w . However, a method benefit of this analysis is that only the water temperature differences are needed. If the measured T_w changes and the cross-

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section Tw does, accordingly.

Section 2.2. Here the authors used a constant flow speed to evaluate the flow time required to travel from a cell of the catchment to the catchment outlet. The authors should clarify how they selected this flow speed and if it is reasonable to assume a constant value (was this velocity the same for the four outlets?). I wonder about the methodological robustness of the approach proposed by the authors since they applied the same flow velocity to all cells pertaining to the catchment, thus both to hillslope and river network cells. In this regard, I also do not fully agree on the sentence at P5, lines 21-22 since before reaching the channel network, rainfall may follow different paths (infiltration, runoff, etc.), thus exchanging heat with the surrounding environment and decreasing its correlation to T_a .

Comment: In our model, the flow speed does not vary in space and time. Generally, the flow speed in the shipping channel is between 1 m/s and 2 m/s. This is supported by ADCP round robin tests (https://www.bafg.de/DE/05_Wissen/02_Veranst/2007/10-09-07_bericht.pdf?__blob=publicationFile) which showed a average flow speed of 1.2 m/s. Using the Koblenz data as reference we tried several flow speeds to minimize the RMSE of the model. We found a minimum of RMSE at 0.4 m/s. This is in the extended-range flow speeds. We expected a higher correlation at lower flow speeds than actually measured in the Rhine as we do not model standing water bodies. To us a flow speed with a magnitude difference would be questionable, but the one used is within reasonable limits.

P3, line 20:

Comment: We changed the wording.

P4, line 1

Comment: We changed the wording

Section 2.3 The authors state that parameter a_1 (the intercept) summarizes all effects

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that are not directly ascribable to T_a and Q , which "are mostly from anthropogenic sources". Personally, I do not agree that, in general, the value of a_1 can be unequivocally related to anthropogenic factors.

Comment: Of course there is no proven, but this the hypothesis. We are able to strongly support this hypothesis by comparing changes in anthropogenic heat input (nuclear power plants) and short term economic changes to a_1 and draw a consistent picture in the manuscript.

The authors should support this statement referring to previous literature on the topic. In this regard, a useful reading is Isaak et al (2011), where also the multiplicative interaction term has been included in the multiple linear regression model.

Comment: We reviewed all citations, thank you for the hints. If applicable we changed the manuscript. Especially, the different methods for modelling T_w are described now more detailed in the introduction.

Variables x_0, y_0 , and in eq 2 are not defined. Table 2 (and corresponding description in the main text): the authors should provide details on why they assumed a linearly decreasing weighting factor instead of other weighting functions.

Comment: We added an explanation of x, y . We revised our model and use now ACC^w as weighting factor. The reason for a linear decrease cannot be answered within this manuscript and more research is needed.

While the weighting factors decreases with Δt , I expect that T_w is no more correlated to T_a after some time. The authors obtain the best results using the "Time lag" model instead of the "Time lag + weight" model, saying that the furthest and oldest T_a influences on T_w are still carried as information in the water mass (P9, lines 4-5). In my opinion, the real reason is that without assuming a decreasing weighting factor the authors increase the dependence of current river water temperature on previous conditions, thus implicitly accounting for the thermal inertia of the river. This is an important

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aspect controlling river water temperature, which is not explicitly included in the model proposed by the authors and that can be accounted for e.g., through autocorrelation terms (e.g., Caissie et al., 2001; Toffolon and Piccolroaz, 2015).

Comment: We think that the reason for using a weighting factor decreasing is a) to put less weight on the large amount of grid-points with less ACC and b) to put less weight on temperatures with a large Δt . Autocorrelation is an option but we decided not use it for this model.

Control scenarios I would use a different word than "scenarios" here, since these are not scenarios but different approaches to calculate T_c .

Comment: Changed.

Section 2.4 The authors should explain how they calculated the heat input by NPP to the Rhine. The section should be expanded, and the sentences harmonized to make the reading more fluid (too short sentences).

Comment: We moved the explanation of the NPP heat input to the methods section and revised it.

Figure 3 and Table 3 Figure 3 would benefit from the inclusion of the air temperature time series with the corresponding linear trends. This would be useful for better understanding the correlation between river water temperature and air temperature fluctuations, which are filtered out when using linear trends. In this regard, it would be useful to add the Pearson correlation coefficient between these two variables in Table 3.

Comment: We added air temperature to the figure. We also added the RAPS index to make trends more visible.

At P8, lines 12-15 it would be useful to compare the trends found by the authors with those of more recent studies.

Comment: We removed this section. The focus of the paper is on providing reasons

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for the heterogeneous T_w trends in the Rhine river, an urgent matter in regulative river heat evaluation in times of climate change.

Tables 4 and 5 Why did the authors use the "Time lag weight" approach for all other results instead of the "Time lag" approach, which performed the best? It should be clearly indicated if the RMSE and NSC refer to daily or annual values.

Comment: As mentioned before (first page of this document), the data was reanalyzed. As a consequence the tables and parts of the results were revised. The scientific conclusion was not changed.

Section 3.3 It is unclear how the authors evaluated RBT over time. Did I correctly understand that they applied the multiple linear regression model for overlapping two-year time windows shifted by one month? What was the rationale of assuming two-year time windows instead of longer periods? Are the results affected by the length of the time window used for this analysis?

Comment: Longer time windows would decrease the temporal resolution of the regression. A shorter time window increases the influence by other linear dependent influences. The two years were chosen to address two full annual cycles. If a year was extraordinary concerning air temperature or discharge, a two year cycle would not be prone to such events.

P10, line 2 these sentences are qualitative, and not sufficiently supported by the results.

Comment: We changed the wording. We add that we cannot meaningfully interpret the absolute value RBT.

P11, line 4: these sentences are qualitative, and not sufficiently supported by the results.

Comment: The similar trends are supported by the analysis comparing calculated ΔRBT with measured ΔRBT .

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The comment on the effect of alpine lakes is not well connected to the rest of the paragraph and should be expanded with some more detailed discussion.

Comment: We just hypothesize why Basel has such an alternating RBT. However, the RBT does not show a long term trend over the whole dataset. Finding the reason is not in the scope of this paper.

Eq 10 is dimensionally not consistent.

Comment: Thank you, we missed the density. Changed.

How did the authors select the periods in Table 6?

Comment: The start of the period is the time of the maximum heat input by NPPs at the respective station. We added this information to the text.

The authors could do the same calculation in continuous, for the entire period when the data are available (e.g., using the same two-year time windows as before).

Comment: This would be a good idea. However, the aim was to use a time windows with the largest signal to noise ratio. Therefore we picked the largest ΔHI to avoid influences by short term trends.

P11, line 16: what is the BASF company? This should be explained.

Comment: We added two sentences to explain the BASF.

Why RBT in Figures 4 and 5 are different? How sensitive are the results of the correlation analysis to the filtering of the data?

Comment: Figure 5 has filtered RBT. We used a Butterworth band-pass filter instead of a running mean filter because the filter function of a butterworth is much easier to understand and it simply cuts all variations that are outside of the pass area. In this manuscript everything with a periodicity of 20 years (0.05 y⁻¹) or longer is cut off. The reason is to eliminate long term trends, because the aim is to compare RBT to the GDP

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change. The lower limit is 0.9 years (1.1 y⁻¹). Fast variations (faster than a year) of the RBT could influence the correlation vs a data-set (here the GDP) which is provided on a yearly basis. Therefore we smoothing is needed.

How the filtering parameters have been chosen and why 480 days has been used to shift the GDP-change time series? This number seems quite arbitrary.

Comment: It was shifted to ensure a visual match between the two data-sets (GDP and RBT). The shift can be explained by lagging and leading economic factors. This is explained in the manuscript. Mathematically the 480 days shift does not yield the largest positive correlation.

Appendices could be moved to the main text. In particular, the sentences in Appendix B should be revised because they have some syntax errors and typos. Figures A1 and A2 are inverted and the caption is the same. The analysis of parameters a2 and a3 should be deepened and moved to the main text.

Comment: We move the biggest part of the appendix into the main text, as advised.

Technical corrections

P1, line 13: "but an" → "but is an". Is "means of production" an appropriate term in this context?

Comment: Thank you for the hint. We think means of production is appropriate.

P2, line 3 and following lines: the use of "Ta → Tw" is informal and should be modified.

Comment: Thank you for your comment but we would like to keep it that way.

P2, line 8: "hydro-logical" → "hydrological"

Comment: We changed it.

P2, lines 8-9: a reference is needed here.

Comment: This part has been moved and we added a reference in this sentence.

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P2, line 16: is "revise" the most appropriate term here?

Comment: You are right. We use "test" now.

P2, line 20: "almost ideal" → "ideal", "interesting", "meaningful"

Comment: Thank you, we changed it.

P4, line 13: "followed, by" → "followed by". Please, thoroughly revise the punctuation-throughout the article (use of commas, missing close-brackets, etc).

Comment: We completely revised this part. The sentence is now rewritten.

P5, line 17: "ptovided" → "provided"

Comment: We changed it.

P6, line 1: I would say that authors present fourTccalculations, not two.

Comment: We revised this part completely.

P6, line 18: "heat input by NPPsto the Rhine" → "heat input by NPP to the Rhine"

Comment: We changed it.

P8, line 5: "(0.0350âCy-1)" → "(0.0489âCy-1)"

Comment: We completely revised this table.

P10, line 15: "over the a time period" → "over a time period"

Comment: Thank you, we changed it.

P11, line 1: "shorter timer scale but do not seem,to our" → "shorter time scale but doC6 not seem, to our"

Comment: Thank you, we changed it.

P11, line 14: "A a discontinuity" → "A discontinuity"

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Comment: Changed. P11, line 19: "by a by a" → "by a"

Comment: Changed.

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