Hydrology and
Earth System
Sciences
Discussions

Interactive comment on "Incorporation of the equilibrium temperature approach in a Soil and Water Assessment Tool hydroclimatological stream temperature model" *by* Xinzhong Du et al.

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The manuscript "Incorporation of the equilibrium temperature approach in a Soil and Water Assessment Tool hydroclimatological stream temperature model" by Du et al. deals with the modification of the stream temperature simulation in the SWAT model. The work presented in this manuscript builds on previous work by Ficklin et al. 2012. I agree with Reviewer 1 that the paper is generally well-written and structured and easy to follow. However, I see major weaknesses that the Authors should address prior to a possible publication in HESS.

General comments

My major concerns are: - why was the approach only tested in one catchments? Ficklin et al 2012 tested multiple catchments, and is also co-author of this study. Why did you not test your approach in the same SWAT models? - It would have been possible to refrain from using the most sensitive parameter by implementing another equation for depicting dew point temperature. It could be possible that this parameter accounts for other weaknesses in the approach and I therefore ask for additional presentation of intermediate temperature calculations. - I think the comparison to a not calibrate-able equation is unfair (e.g. what if a simple lag factor or multiplicative factor would be included in the original equation?). Most SWAT users will not do or will not be able to calibrate stream temperature. Please provide an assessment using default parameter settings of the equations. - at the current status, the section of the 'sensitivity analysis' regarding water quality is of not much use for the reader. I would like to see a comparison to observations or at least a more detailed presentation of the results (e.g. further statistics based on daily data).

Additional specifications to these concerns and further comments to specific text sections are given below:

Abstract

p.1 l.12-15: At this stage, this is confusing for the reader. Make it clearer that you are modifying the hydroclimatological model

p.1 l.25-26: This is not true: The original model needed no calibration parameter at all.

Introduction

p.1 l.38: ...species have...

p.2 l.4: add industry and power plants

p.2 l.16: I think the equifinality problem does not only apply to statistical methods, but can occur whenever a multi-dimensional parameter space exists, e.g. also for a physically-based model

p.2 l.26-27: This is common sense, I agree, but since you are focusing on those later, is there a reference that lists these parameters as the most influential?

p.2 l.29: I would not generalize it to the point that "statistical models" may not be reliable. I suggest to refer to the previous examples.

p.2 l.36: negligible

p.2 l.42-43: suggest: "...approach. Therefore, these algorithms need to be directly linked or implemented in hydrological models to project the effects..."

p.3 l.5-7: classify the model according to your definition (statistical or mechanical)

p.3 l.7-11: this is repeated later in methods. Suggest to move this detailed information to the methods only.

p.3 l.11-13: This raises the question why the current method is not tested in these seven basins as well. Especially since the author of the paper is also co-Author of this paper

p.3 l.27: ...has rarely been used...

p.3 l.33: The primary objective is not "to develop a stream temperature model". To make it clearer for the reader, I suggest something along these lines: "The primary objective of this paper is to improve the simulation of the heat transfer process in the hydroclimatological stream temperature model on the example of SWAT"

p.3. l.41: ....located in Alberta....

Material and Methods

p.4: The catchment area seems very specific and I am not convinced, that other catchments to test the model are not needed. Please comment.

p.5 l.1: datasets

p.5 l.7-8: I don't understand how you obtain only 1370 HRUs with those numbers of

subbasins, land uses, and soils. What are the (spatial?) pecularities of the setup?

p.5 l.31: I would mention also the case if air temperature suddenly drops or rises

p.6 l.29: It is unclear where the approach is linked to the hydrological stream temperature model. What are the previous equations that are used / replaced or is it added on top of equation 4 and 5? Please mention the link to the equations of the previous chapter.

p.7 l.4: I think you should add "qnet = KT(Te-Tw)"

p.7 l.12-25: I do not understand why you do not use the dew point calculation based on temperature and humidity. Both temp and humidity are SWAT input parameters and the simple equation to calculate dew point does not need calibration. Why did you opt for including an additional calibration parameter? It could be possible that the calibration parameter you include may account for other shortcomings in the model. I suggest to printout an example of the stepwise 'improvement' of temperature depiction vs observed to check the validity of the approach.

p.8 l.19: what was the 'higher sampling frequency'?

p.8 l.24-26: I do not understand...what do you mean with 'one set of parameters were used for the calibration process'?

p.8 l.31-32: Mention which equation and which parameter

Results and Discussion

p.9 l.16: I think even more important than a reasonable NSE for your purpose is the correct depiction of streamflow components (surface runoff, lateral, groundwater flow). Can you comment on the model performance in that regard?

p.9 l.19: considering that this parameter is so sensitive I again need to stress my comment for p.7 l.12-25.

p.9 l.24: I think your comparison is not fair. The current stream temperature model for instance does not need calibration and not every user that depicts water quality has temperature data available to calibrate your model. If this approach will eventually be available in the SWAT model by default, it is extremely important that default parameters are defined that make (some) sense and are applicable for the widest range possible. So, please add a comparison of the three uncalibrated methods.

p.9 l. 21,23 and p.10 l.1: Do you briefly discuss the physical basis / validity for these parameters somewhere? Is it possible to deduce default parameter settings from this?

p.10 l.10-14: Can you discuss why the hydroclimatological model is worse than the original model? It performed so significantly better in Ficklin 2012 in multiple catchments

p.10 l.19: What is so special about that station? Connect the physical catchment properties to the simulations. And why was no validation carried out there?

p.12 l.1: labels on the figure are too small. It is unclear where those subbasins are located in the basin and why they were chosen. Maybe it is better to show box- or violin plots of the 12 months incl uding all subbasins.

p.12 l.16: This is repetition from the methods.

p.12 l.22-24: water depth in swat depends largely on river width...how did you make sure that water depth is reasonable and how sensitive is water depth in the approach?

p.13 l.1: It is uncommom to have an equation in the results. Why didn't you include it to the methods? Unclear to the reader where k20 and teta come from - mention e.g. that the values are SWAT default parameters.

p.13 l.4: Table 4: I think the mean values in the table are misleading. The numbers of the equilibrium model are almost the same as the original SWAT code despite the fact that it performs so much better than the original model. While the hydroclimatological model shows siginifically different values, though it performs similarly to the original

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model. Please consider showing three diagrams similar to Figure 4. For BC3: Probably "Organ N hydolosis rate" is "Organic N..."? Figure 4: Text too small, replace "SWAT" through 'Original SWAT"

p.14 l.19: located

p.14 l.21: Figure 5: You did not compare it to measurements (I would have loved to see it), but are these changes significant and plausible and do they go into the right direction, do they improve the water quality simulation?

p.14 l.22: Table 5: These values do not mean much...e.g. changes in the second digit for average water quality parameter at Muskeg are irrelevant. I suggest to add further statistics: e.g. the standard deviation, 2, 20, 80, 98 percentile based on your daily simulations.

Conclusions

p.15 l.24: looking at figure 2, this seems different. The blue dots are not on a daily time step.

p.15 l.28: Please discuss how applicable the model would be in other regions (humid, temperate, arid) regions. Also mention gaps and weaknesses and room for further work.

will the Code be made available?

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