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Geoscientific Model Development Discussions

Interactive comment on "StreamFlow 1.0: An extension to the spatially distributed snow model Alpine3D for hydrological modeling and deterministic stream temperature prediction" *by* Aurélien Gallice et al.

Anonymous Referee #1

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The manuscript by Gallice et al. presents a revised and improved version of a hydrology and stream temperature model (StreamFlow). The authors have rewritten the source code to facilitate modularity and future development. A case study using data from a high elevation catchment in Switzerland is used to illustrate an application of the model.

This is a very nice addition to the stream temperature modelling literature and I have no substantive issues with the manuscript. I would like to make a few suggestions to help improve the clarity of the manuscript and clarify some of the model details/limitations.

Section 3.1 and Figure 3:

I struggled with some of the terminology in this section, such as Decorator and abstract vs concrete class. To ensure wide adoption of this model by scientists and practitioners who may not have extensive programming experience, I would recommend the authors try and better guide the reader through this section and limit some of the programming jargon.

Model structure and details:

I think some of the model limitations need to be presented earlier in the manuscript. For example, it's not until page 9 that the reader is made aware that the current model cannot account for riparian vegetation. In addition, it might be useful to expand on other settings where this model may not be appropriate, such as streams influenced by hyporheic exchange and deep groundwater contributions.

It also seems that a stream temperature model built for snow dominated catchments located above the tree line should be able to account for stream channel ice formation and snow inputs. Could the authors comment on why these processes were not included in the model?

Does the two linear reservoir structure from Comola et al. (2015) have a physical basis (i.e., is it meant to represent shallow subsurface flow and a deep groundwater contribution)? I realize this sort of hillslope runoff structure is common in hydrology models, but can it be appropriately extended for heat dynamics? Perhaps these details are outlined in Comola et al. (2015), but it would be nice to include more rationale on this part of the model structure in this manuscript.

Could some computation time measures be given for the model? Obviously it will depend on application and computing resources, but some general benchmarks might be useful.

Case study:

I understand that the aim of this manuscript is not to conduct a rigorous model calibration and test, but the consistent stream temperature underprediction (Figure 9a,b) and the inability of the model to properly simulate rainfall event flows (Figure 6a,c) may dissuade some readers from using this model. Is it simply a matter of more extensive model calibration to achieve better discharge and temperature predictions, or are there some model structure limitations that prevent the model from simulating event flows and/or higher stream temperatures?

From Table 5, it's interesting that most of the modelling resulted in underprediction of stream temperature; however, temperatures at Durrboden were overpredicted and Am Rin didn't show a strong bias. Why were the outlet temperatures underpredicted, but not the other two stations?

It's a nice result showing the importance of subsurface temperature runoff on stream temperature predictions at the outlet (Figure 10). The soil temperature approach seems to do a fair job, but does result in consistent underpredictions (especially during winter). Have there been any evaluations of the Alpine3D soil temperature routine? Does it account for frozen soil processes? Are the simulated zero degree soil temperatures at 2.4 m reasonable for this site? Could you recommend to readers how best to calibrate that model component, as it seems critical for accurate stream temperature predictions.

It appears that mean water depth simulations are highly sensitive to the flow routing approach used (Figure 8). How sensitive are the stream temperature predictions to the flow routing approach?

Additional comments:

Page 2, line 5 (and elsewhere): 'e.g.,' should be used to introduce examples and should not go at the end of the sentence.

Page 3, lines 1-8: Perhaps include reference and discussion of Isaak et al. 2016. Slow climate velocities of mountain streams portend their role as refugia for cold-water biodiversity, PNAS.

Page 3, lines 14-16: This sentence isn't clear (e.g., what's an emergency van) and feels out of place. Consider removing or revising.

Page 4, lines 10-14: I would argue that DHSVM-RBM (Sun et al. 2015) has and can been used in mountainous terrain and employs a more process-based snow modelling approach than a degree-day method.

Page 5, line 5: Replace 'stressed out' with 'stressed'.

Page 6, line 5-6: Please explain why this approach is not compatible with potential future alternatives for subsurface runoff discharge modelling.

Page 16, line 5: '... were installed starting 16 January 2015...' until when?

Page 17, lines 24-27: How sensitive were the discharge and temperatures simulations to this warm-up period approach? Was one year for warm-up sufficient?

Page 18, lines 6-12: Could the benchmark model approach be more clearly described? I'm not sure I understand it correctly - is the benchmark model output a vector of hourly discharge (or stream temperature) that is comprised of the mean hourly values from 2005 to 2014 (i.e., temperature for day of the year 1 and hour 1 is the mean of that day-hour combination for each of 2005 to 2014)?

Figure 5: Is this comparing snow depth or snow water equivalent? Wouldn't snow water equivalent be more appropriate?

Figure 9: The observed stream temperature doesn't suggest this stream freezes in the winter. Is that the case? Does surface ice form? If not, what energy exchange processes are maintaining stream temperature above zero degrees Celsius?

Table 2: It seems that the calibration of k_soil and D_HSPF are pushing the upper limits of the calibration range. Should a wider range have been selected?

Table 3: This table could probably be removed, since these results are already presented in the text.

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