

Editor Comment on hess-2021-147

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Editor comment on "Technical note: RAT – a robustness assessment test for calibrated and uncalibrated hydrological models" by Pierre Nicolle et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-147-EC1>, 2021

This technical note proposes an additional method to check whether it is reasonable to use a model under climate change conditions, a method that does not rely on numerous re-calibration. I see the value of the idea presented here but in addition to the reviewers' comments, I would like to raise some additional points.

First of all, the choice of the hydrological year might in my view have a considerable impact on the results and I am surprised that it is not tested in this note (easy to do: check the robustness of the results for other closing dates). Why? The hydrological year is most probably chosen such as to have little carry over of water storage from one year to the next (something that might need to be specified in the paper); for soil moisture and snow, August (as chosen in the paper) is certainly a reasonable choice but what about catchments that show the driest month in September? Will the carry-over effect result in spurious model bias correlations to temperature?

This brings me to the next point: the authors use a simple model, which has the main advantage that it should be straight-forward to explain what model "problem" could cause a correlation between bias and air temperature (or precipitation). It would be nice to see how we could attempt to interpret the climate dependency of the model. What model parameterizations can in fact influence the annual bias in this model? What does it tell us about the model if the bias increases with temperature or with precipitation? Obviously, such an interpretation is not possible for complex models but I really see this as a missed opportunity to share the authors' expertise in the use of a conceptual model to give guidance on how to further interpret the results. I.e. I would like to see a further elaboration of Section 4.2 (in addition to the comments raised by reviewer 2 on this section).

This attempt for interpretation might in fact even unravel unexpected reasoning: is it not a good sign if e.g. a simple snow model gives stronger bias in snow rich years as opposed to snow poor years?

Furthermore: I do not understand how the GSST points in Figure 4 are obtained; how is the bias for two simulations over two different periods (validation, calibration) defined.

And there is a formulation error in line 223, which reads like: "We then identified the catchments where the RAT procedure detected a lack of dependency of streamflow bias to climate variables." I guess it should be "identified a dependency"

