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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Predicting discharge capacity of vegetated compound channels: uncertainty and identifiability of 1D process-based models" *by* Adam Kiczko et al.

Anonymous Referee #3

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General comments

The authors present a study of vegetated flow in both a lab and a field experiment. This topic is highly relevant. Field validation of the various proposed vegetation models is needed - especially in the smaller scale systems such as described by the authors. I highly appreciate the authors' contribution to this discussion by presenting these two interesting case studies. In line with the authors' stated desire to present an objective and transparent study, I do hope they publish the data as well, attached to a future version of this manuscript.

The authors' approach to compare three different conveyance models using proba-

bilistic parameter estimation is commendable. I do not share the authors' statement of complete novelty of this approach (p31, l493), and refer to the work of e.g. Reitan & Overleir (10.1029/2010wr009504) and Coz et al. (10.1016/j.jhydrol.2013.11.016), although I'm sure more may be found.

In general, the authors' presentation of the current state-of-the-art in literature is poor, with the notable exception of the introduction on vegetation models in the first half of the introduction. The chosen methodological approach has some well-known weaknesses, well discussed in scientific literature, but which the authors do not sufficiently discuss. For example, the conclusions drawn by the authors on parameter identifiability (conclusions nr. 1 and nr. 2) are contrary to what should be expected. These claims, if upheld, must be supported by better evidence and be placed in light of other, more recent literature.

Overall, I feel the authors focus too much on (sometimes trivial) details of the analysis, and not enough on the practical interpretation of their results. The widths of the confidence bands is just not very interesting, given the use of the informal GLUE approach. If succesfully applied, the confidence bands should describe the variance present in the dataset. I think the check whether the error statistics held up for higher discharges (the verification set) is more interesting, as this shows which model is better suited to present the data, and I'm happy to see the GTLM model performs so well in this regard.

However, this does raise fundamental questions as to the application of the physics-based models. The author's approach treats all models essentially as black-boxes, and calibrated them on a limited number of data points. This seems to defeat the purpose of physics-based models, and one would ask how the authors foresee application of the best tested model?

In conclusion, I believe the manuscript needs revision before publication can be considered. I do hope the authors find my remarks helpful, and I would welcome a revised manuscript for reconsideration.

Specific comments

P1-L10: 'quasi-Bayes'. The only time this term is mentioned is in the abstract. Perhaps a definition could be included in the manuscript.

P3-L58 the authors use 'process-based' models and 'physically-based' models seemingly interchangeably. I recommend choosing either 'process-based' or 'physics-based' (not 'physically-based', which is admittedly used throughout literature)

P3-L58 I do not share the authors' broad assertion that physics-based approaches are unpopular in practical applications.

P3-L62-64 The authors skip over many other possibilities by jumping from the detailed parameters of the Vastila and Jarvela models to the Manning coefficient. A (physics-based) model with fewer parameters would be an option. Numerous studies can be found in literature where two-dimensional models use spatially distributed information on vegetation, often based on remote sensing techniques.

P3-L72 This is a very valid point, and in my view the most important objective of this study

P3-L74 "Any method can ... a parameter calibration". I reject this statement. Do the authors like cake only if all its ingredients can be individually tasted?

P3-L77 'predictive uncertainty' is a technical term used differently by different authors. The authors should define their use of the term.

P3-L79 'As one of the first works, this paper' What paper do the authors refer to? (If they mean their manuscript, see my general remark on novelty).

P3-L82 "most of the previous studies": the literature review by the authors cites dated literature. To give confidence in this statement, the authors might provide a review of more recent literature.

P3-L89-90 I don't think this is a valid contrast. Morphologic and hydraulic modelling

are very different challenges and many existing vegetation models are not suited for morphological modelling.

P4-L95 The authors compare 'explicit' and 'implicit' uncertainty analysis. I'm not sure this terminology is commonplace, and in any case requires explanation.

P4-L116-L120 I'm unsure why the authors use the terms 'minor values' (text), 'minor parameters' (figure), or 'conservative approach'. To me this sounds derogative, as if to discredit this approach in favour of their proposed alternative, although I readily assume the authors do not intend this. For instance, I do not see why surface roughness is in any way 'minor'. In fact, if this parameter is used for calibration it is very likely the most sensitive parameter, so by all accounts should be labelled 'major'. Nor do I see why the first approach should be labelled 'conservative' (what is conserved? Do the authors mean 'traditional'?).
Second (and this point was raised by another referee as well), the 'conservative approach' is surely preferable - if reasonable estimates of the uncalibrated parameters are available - over treating vegetation models as black boxes.

P5 L134-135: For its merits in popularizing uncertainty analysis, the GLUE method is (in)famous for the liberal use of the likelihood measure, which does not agree with Bayes' theorem. The authors choose to use a so-called 'informal' likelihood measure with a scaling factor that controls the uncertainty. The authors then force the model uncertainty to include at least the right amount of data points through equation 5. This approach, inspired by Bayes theorem, has the known disadvantage that predictive and model uncertainty are lumped (the authors approximate the total uncertainty), that parameter uncertainty tends to be overestimated, and that the choice for a likelihood measure is arbitrary (i.e. not following from the error model, as is the case in a proper Bayesian approach). A defense by the authors on their choice for an informal over a formal approach would be appreciated for readers unfamiliar with this distinction. Also, given that the authors use an informal method, I'm interested whether a behavourial threshold was used (the authors mention the need for this, but not whether it was used.

I assume none was used given the scaling factore.)

P7-L194 Please elaborate which resistance (all, only the bed, only the imaginary wall?)

P11-L297 Is the Ritobacken Brook free flowing? Can uniform flow be reasonably assumed?

P13 In general, I suggest adding the first section of chapter 3 to the method section, as new methodology is introduced here.

P13-L315 By 'trial and error' choosing the sampling size of Monte Carlo, do the authors mean increasing the sample until convergence is observed? What convergence criteria is used? Which sampling method is used?

P13-L315 "In a similar way"; are a priori distributions chosen by trial and error? In principle a priori distributions are either informative, based on prior knowledge, or uninformative. Here an uninformative uniform prior is chosen, but I have to learn this from the captions in table 1. It would be helpful if this is explicitly added to the text as well.
I would also appreciate a brief exposition of the choice for an uninformative prior. Given the models are physics-based, and the authors have a pretty good estimation of their likely values, it seems more logical to use informed priors.

P17-L365 Here the authors define "model identifiability" as (I paraphrase): "it is identifiable if it is fittable". The authors then admit that their approach would allow even poor models to fit well, while "the only limitation could be the physical meaning of the parameters". It is unclear whether the authors did indeed let themselves be restrained by the physical meaning of those parameters. One may remark here that minor changes to their chosen approach (i.e. a formal Bayesian approach and informed priors) would be expected to alleviate some of these problems.

At this point it is also good to remark on a different, perhaps more fundamental point. The authors go into depth into 'model identifiability' but the reader was led to believe

that 'parameter identifiability' was the objective of the study. Yet most results and almost all figures focus on the question 'will the model fit' - which is not a very interesting point to stress given the objective of the study. The only figures that support 'parameter identifiability' are 14 and 15, but those are currenty insufficient to support the claims made in the conclusions; it would be helpful to plot the a priori cdfs as well, so as to see how they were constrained a posteriori.

P31-L495 - The 'trial and error' a priori distribution estimations bothers me a bit when claiming objectivity. Given the limited number of observations, the a priori distribution is expected to affect the output.

P31-L500 'It was possible to identify...one (DCM)'. This should not be surprising. It is in general easier to fit a model with more parameters than one with fewer. However, the more impressive claim would be that the parameters are identifiable as well. I refer to the work of Werner et al. (2005, doi:10.1016/j.jhydrol.2005.03.012), to illustrate that challenge.

P32-L528 'Thus, our result... resistance dominated'. I do not see how this follows from your results nor from the previous sentence.

P33 Conclusions. The authors conclude the article with 8 claims.

Claim 1. The authors claim it is possible to identify the parameters of physics-based models, even if those models have many parameters. This is an unlikely claim (given the number of data points and the number of model parameters), but may follow from a confused definition of 'parameter identifiability' versus 'model identifiability'. If the parameters are 'identifiable', I would expect narrow a-posteriori distributions compared to the a-priori distributions. Figures 14-15 do not show a-priori distributions. Although it is difficult to judge whether the parameter distributions are meaningfully narrow, it seems only C is well defined. If so, it would be interesting to reflect on figure 1.
The second claim, like the first, seems to only apply to model identifiability, not pa-

rameter identifiability. The authors might spend some words on how they perceive the model to be used - does it matter if the models are physics-based? Or any (data-based) model that fits the rating curve applicable?
Claim 3: Perhaps the authors could explain how uncertainty relating to parameter equifinality can be distinguished from other uncertainty.
Claim 4, first sentence: Would not a better explanation be that the model is insufficient in some way, and that model parameters differ to account for this? Second sentence: I don't understand this in relation to the third claim.

Technical corrections

P2-L22 use plural for solutions
P2-L53 Use of the indefinite article would improve sentence
p3-L57 discussed by Yen p3-L58 For instance P3-L77 model's P3-L77-78 'The better model...uncertainty' please revise P3 - L86-87 'as it was assumed...their distributions' I don't understand this sentence P4-L92 check references

P4-L109 'It is out...the available methods'. Methods for what?

P5 L133 'fit measures' what are fit measures?

P8-L211 I count 8; which are the other two?

P14-L316 'It was was done'. In general, it is advised to explicitly state what you are referring to. In this case, I'm not sure what the authors mean by 'it'.

P20-L101: 'the probabilistic term'. Which term is this?

P31-L506: 'horological' (I assume 'hydrological', although I can accept the term if the model was setup around October 31st :))

Table 1: I suggest to merge Tables 1-2 into one table.

Table 1 caption: It is not clear from the caption whether this is a priori or a posteriori distributiosn

Figure 4: The vertical axes of the upper figures are not equal

Figure 4 caption I don't understand what the authors intend by presenting 'exemplary' rating curves. Aren't these results?

Figure 8 Please explain 'n' here as well. Each figure should be independently understood

figure 15 caption: 'confidence intervals and median of a probabilistic solution'. I'm a bit thrown of by the indefinite article.. which solution are the authors referring to?

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