

Interactive comment on “Large-scale hydrological modelling by using modified PUB recommendations: the India-HYPE case” by I. G. Pechlivanidis and B. Arheimer

Anonymous Referee #2

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1 General comments

This manuscript reports on the process of implementing a hydrological model for the Indian subcontinent and reflects upon this process in the context of the recent recommendations of PUB on hydrological modelling. It is well written and well presented.

But to be honest, I find this paper of a lightness that verges on the unbearable. It surely touches upon some of the fundamental issues of modelling in a complex and challenging area such as India. But it feels strongly as if the authors really wanted to convey their entire thought process in the smallest detail, and to interweave that with

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each relevant concept of current hydrological thinking that springs to mind. This makes the manuscript rather tedious and long, especially because it regularly gets trapped in generalities and even clichés. In the specific comments below, I have tried to highlight some sections that I think need particular attention but it is in no means exhaustive.

In addition, I understand that authors use the model implementation as an illustration to make their point, rather than as a scientific experiment that generates insights in the local hydrology (or the model behaviour). Nevertheless, in my opinion it is not a very powerful illustration, mainly because of:

- the lack of purpose of the model. Without a particular purpose, it is very hard to make a point about the quality of a model implementation. It is a bit of a cliché in itself, but Box's aphorism that all models are wrong but some may be useful, is relevant here: there is a convincing case here that the model is wrong, but because of lack of purpose the argument that the model is adequate is far less convincing. A more concrete hypothesis to test, or use case, would help a lot.
- The discussion on uncertainties and data errors, while exhaustive, is almost entirely qualitative. With the very many methods for uncertainty analysis available, a more comprehensive uncertainty analysis would surely make a strong point.
- Lastly, the monsoon climate characteristics of the study region do not help. With such an extreme seasonality, and only monthly data available, the modelling challenge essentially boils down to a yearly water balance prediction, which is almost entirely dominated by uncertainties in the precipitation data and the evapotranspiration parameterisation. Under such conditions it is hard to do proper model diagnostics.

Based on this, I strongly recommend the authors to rethink the message they want to convey (and its novelty) and how they'd convey it.

2 Specific comments

2886/23: adequate is about as vague as it gets. Of course the model performance is discussed in more detail further on in the manuscript, but I think it would still be very useful to give some form of purpose and fitness for purpose evaluation to the model (see general comments).

2886/27: consistent: that is of course nice, but again very vague. One the one hand, of course many model structures are applied over a large variety of hydrologies (global models being the extreme case) so it is not really that unusual to expect models to be consistent over the variety of hydrological processes considered in this paper. On the other extreme, it would be easy to rebuke the claim of consistency, given that there are so many known and unknown processes such as water abstraction, irrigation, etc. that are not or only very limitedly represented by the model. So again, I feel that I am missing the point. Did you expect the model not to be consistent? This goes very much against hypothesis driven research setup.

2887/20: "increasing the information content": of what? Of your model? Not really. Perhaps the best formulation is "Constraints are generated by independent information..."

2888/6: "putting science into practice": I find it hard to find any evidence of this in this manuscript.

2889/1: "Influenced by human activities": This is the point, isn't it? But the presented study does not do this. I think it is one of the many missed opportunities.

2889/11: "test the recommendations": can you really test recommendations? I can't help but being a bit sceptical.

2889/18: "frequent quality checks": arguably with all models. The case for large-scale models is not made very convincingly here.

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2890/3-5: again, these are hardly unique to the large-scale. Of course data collection will be harder, but could even turn the argument around and claim that large-scale modelling is easier compared to a data-scarce small catchment, because of the availability of global datasets and the fact that many small scale errors are smoothed out.

2891/9-14: apart from being quite a cliché, I would disagree with "familiar to the modeller" and "can be easily set up and run"

2892/4-20: again I would challenge the authors to go beyond the commonplace. Moreover, further on the manuscript the authors mention that they did 3 extensive field visits to the basin and go on to state that they have been very useful for the modelling exercise but. But again there is no discussion as to how it has been useful beyond some general statements on source of irrigation water! I think that such a discussion could be highly informative, but it would have to be much more in-depth. Here is your chance to show really the value of (expensive and time consuming!) field visits.

2892/24-26: This is a rather platitude statement, isn't it? Not only don't you have thousands of stations in your case study, in the (luxury) case where such information is available, it should not be too hard to filter unrealistic values plot spatial and temporal patterns etc. After all, from a statistical perspective it would not be such an unusual large dataset!

2893/11: "practically not feasible": A rather strong statement in this era of cloud computing and big data.

2894/10 - 26: I would think that these recommendations belong to basic quality control practice, and surely are not specific or particularly important to large scale modelling.

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2895/13: "lake parameters": this comes a bit out of the blue. Why lakes? Surely they influence the hydrological response, as do probably a plethora of other processes (e.g., water intakes, swamps, unusual groundwater systems, ...). Again it is a pretty generic (though not always the most useful) strategy in analysing large datasets to start with a subset...

2896/9-10: Again a rather unbearable generalization. Who could argue against this at any scale?

2896/section 2.6: despite featuring here rather prominently, the absence of a rigorous uncertainty analysis (apart from a lonely mention of the "behavioural range" of parameters in the caption of figure 7) is in my opinion one of the major shortcomings of the paper.

2908/23: "of in" -> in

2942: "outlet of the subbasin" -> outlet of the basin? If not, what subbasin?

2942: "behavioural": this is the only mention of behavioural in the entire manuscript. How where they obtained?

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