

## ***Interactive comment on “Investigation of dominant hydrological processes in a tropical catchment in a monsoonal climate via the downward approach” by L. Montanari et al.***

**Anonymous Referee #1**

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Review of the paper by Montanari, Sivapalan and Montanari

General comments

In their introduction, Montanari and coworkers give an interesting account of the discussion regarding model development at catchment scale. They present a clear argumentation for the course they attempt to follow in this paper, which is a careful, systematic and standard stepwise refinement type of approach, starting from the simplest possible starting point -a single saturation excess bucket type model- and trying to end with a model which offers a good tradeoff between predictive quality and complexity -sufficient to capture the dominant processes and their interactions. They also present a solid motivation for their research - the need to predict streamflows in poorly gauged,

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or ungauged catchments, as reflected in the PUB research initiative.

After the introduction, the authors start with a description of the data used in their study, and a preliminary analysis. The way in which the authors combine of two time series of rainfall data, while taking a qualitative measure of data quality into account may have consequences for their subsequent analyses, and for their model development process. This aspect -which is somewhat worrying- and the preliminary analysis are discussed in the specific comments

Based on the introduction, I expected a paper on a series of models systematically developed, described, compared and selected using a clearly defined performance criterion. Given the context of ungauged basins I would expect models to be developed and parameterized independent from actual hydrological data (rainfall, runoff, evapotranspiration). I would expect hydrological data to be used for validation purposes only. I would furthermore expect the models to be parameterized independently of the data used for model evaluation, taking good care to describe the procedures to define parameter values. I would have looked forward to see how the authors for practical purposes define an increase in complexity (given the range of models), what criterion they use to prefer one model over another, which model they finally arrive at. I could even imagine the authors testing the same range of models in another catchment to see if they would select the same model.

Contrary to my expectation, the authors approach is strongly data-driven. The models are developed on the basis of a goodness of fit criterion, i.e. are developed on the basis of description, rather than prediction, and the presentation of the models is such that it is difficult to verify the claim the authors make: "a systematic and progressive development and testing of rainfall-runoff models of increasing complexity" (page 160 -lines 10.ff ). In this case what I would have expected is a table containing an overview of the model concepts developed and tested, a measure of the complexity, e.g. the numbers of parameters the model requires or which need to be estimated from data, a performance criterion, or - criteria, and a description of the processes added. This would

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allow a reader to reproduce the choices the authors made, and would substantiate the claim made in the abstract. The selection criterion for model concepts is a goodness of fit (Nash and Sutcliffe 1970)- criterion, combined with manual calibration procedures. I would urge the authors to use a calibration method that can be reproduced, or provide arguments why they think in this case manual calibration is justified, and furthermore provide information which would allow fellow scientists to reproduce their results. Another aspect of the approach used by the authors, is that -as the authors state- the region (tropical monsoon Australia, and the PUB-region) is characterized by lack of data, i.e. a model is appropriate which requires little - or as little as possible- data dependent parameter estimation. A model development procedure which relies heavily on the availability of gauging data may be regarded as less appropriate for the region. I would like the authors to address this issue.

The model development paragraph is central to the paper. As such it is paraphrased here to identify some aspects which the authors would need to address.

Starting with a Hortonian infiltration model which was not satisfactory (page 168 -line 5), the authors change to a single bucket saturation excess type of model, and after a manual calibration procedure to identify the optimal number of buckets and its parameters arrive at four buckets in parallel. They estimate four parameters from the data.

At this point the authors would need to provide some more detail - could they provide a quantitative measure for "satisfactory"? In what sense is the four bucket model optimal? If this is based on the analysis of other bucket models with other numbers of buckets, could they present performance criteria? At this point a precise description of a manual calibration procedure to estimate four parameters is also required.

The authors conclude that a four buckets model tends to produce an overestimation of peak flows, and after "attempting various modelling solutions which for the sake of brevity are not described here" the process of model development arrives at a four

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bucket model with a groundwater flow component and a non-linear delayed runoff component model. Two parameters contained in the formulation of the delayed runoff equation are optimised based on the recession curves of the hydrographs.

To support their claim for a systematic model development, at this point the authors need to describe the various solutions, the arguments leading to the adoption of various solutions, and their reasons for rejecting them (a goodness of fit criterion?). If the systematic model development is the issue they want to focus their paper on, they may want to drop other parts (sensitivity analysis or preliminary data analysis) to improve this focus, while maintaining overall length.

To achieve further descriptive improvement (inaccuracy of the peak flows), the authors then proceed to re-formulate the model in terms of hourly timesteps, and add refinement to the evapotranspiration routine (partitioning over soil and vegetation). The model is manually calibrated adapting the value of the fraction forest cover.

A common issue in this brief paraphrase is that the authors use qualitative statements to summarize model performance. The authors need to be address this and need to quantify their statements, and summarize this in an overview table.

Furthermore, again throughout the model development paragraph the authors use the notion of prediction. Based on my understanding the authors use it rather loosely. I would strongly argue to reserve the use of the notion prediction for model runs in which the models parameter values were selected/set independently of the data used to determine model quality (or model efficiency). This may seem formal, but there is a more important and related issue connected. The authors claim that the procedure followed may result in models that are sufficient to capture the dominant processes and their interactions appropriate to this region (page 161, 20). I understand the claim of sufficiency and appropriateness to mean that the models suggested are just complex enough for the intended purpose. I feel it would be easier for me to accept this claim if the authors had used the scarce data for prediction only, i.e. had refrained from

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manual calibration. As the final result in this paper the authors suggest a model using an hourly internal timestep as being sufficient. I worry that this model is already over-parameterized, and that the authors may be trying to capture or have been capturing measurement noise. My worst fear is that by using manual calibration, the authors generate their own systematic errors, which they then correct by adding complexity. This point might again be addressed in a table summarizing model development, if the authors provide information to what extent the residuals after each fitting step are systematic, and to what extent random? This may allow to support their step towards a more complex model.

#### Specific comments

P160 - L23-26: please delete. It is less meaningful to list factors in the abstract which influence the waterbalance, but for which no information is available.

P163 L2 In view of the fact that the authors are developing conceptual type models at a daily time scale, the explicit statement that conceptual models is ideal for long time scale behaviour at regional level begs the question why they think building a conceptual type model at daily time scale may be justified.

P163 L9 In what sense is the application of the downward approach standard? If there is a standard, please quote the relevant reference.

P163 L13 please delete whole, be more specific: tropical catchments "in general": what does this include or exclude?

P 163 L16: may contribute to a rational classification of catchments. Is this an objective of the paper? If so the authors might make this contribution a part of their conclusions?

P 165 L2: Whereas I understand the argumentation, it seems a pity to discard data. (Potentially half, but probably less - could the authors indicate how much they had to disregard?)

Alternatively the authors could have introduced a weighing function, conveniently pa-

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parameterized. A possible suggestion might be to classify the data as follows: 1 = good continuous data 2 = interpolated 3 = extrapolated 4 = not recorded; not yet available. The weight function could be calculated as:  $(4/3-x_i/3)^W$  where  $x_i$  is the quality case for observation  $i$ . The case the authors discuss would be the case in which  $W$  approaches infinity. The other extreme would be  $W = 0$ , where quality is not an issue. They could then have executed a sensitivity analysis on the basis of the parameters of the weight function, in this case  $W$ .

P 165 L3-11: I feel there are two approaches to the problem of the exceptional year 1975: 1 - it is exceptional; 2- the authors use a method of generating rainfall data that makes it exceptional. In the first case the question would be: why is 1975 exceptional, and was 1975 also exceptional in other locations? In the second case the question would be: Does 1975 remain an equally exceptional year if the weights of the method used to calculate rainfall series are changed?

P 165 L 6-8: The statement “individuated as critical towards area rainfall determination” At present this procedure is irreproducible for other scientists. I would request the authors to define (or refer to) a procedure, and to reformulate (replace “individuated as critical” by an alternative)

P165 L 25-27: Shift the sentence about downscaling evapotranspiration to the part where it is more relevant, in the description of model B4GDETH

P 165 L 23-25: The statement “matching the means and standard deviations of the longer data series ((from the three stations) with the corresponding values of the shorter series (i.e. at Katherine)” At present this procedure is irreproducible for other scientists. I would request the authors to define what they used as the final time series - the average series of the three stations with the mean and standard deviation of Katherine?

P 166 L 7-10: The authors claim that this paragraph results in an identification of the optimal structure. Strictly speaking this claim is too strong, and should be dropped.

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P 167 L 16: Please explain: In what way is 1986 representative?

P 167 L 21-24: How would the authors define the difference between the signature of saturation excess and that of delayed runoff in terms of the variables they analyze in these lines?

4 A hydrological model for Seventeen Mile Creek Suggestion: is it possible to introduce a substructure referring to the different models. i.e. B1, B4 etc.?

P 167 L 24: Please explain in which sense the flow is low, but “significant” Suggestion to delete “low, but significant”

P 168 L 3-5: Suggestion to make the Hortonian infiltration model part of the model development, and include performance criteria in extended version of Table 3

P 168 L 10: physical/conceptual: Given the difference the authors make between physical versus conceptual models a choice should be made

P 168 L 20: Please define the word “predictions”

P 169 L 22: “unacceptable”. The use of the word supposes that the authors have defined acceptable behaviour vs. unacceptable. Please include performance criteria values for the B1 model in an extended version of Table 3.

P 170 L8: Please quantify “overestimation”, and include the values in an extended version of Table 3

P 172 L 2: Perhaps it is an idea to put the statement about the hourly values of evapotranspiration here. How do the authors “downscale” rainfall? Are data on rainfall intensities available? If not, could the authors justify downscaling evapotranspiration, but not rainfall?

P 172 L5: There is another issue in calibration, that of parameter choice: Interestingly enough the authors assume the plant transpiration efficiency to be 1, i.e the reference evapotranspiration provided by the Bureau of Meteorology is assumed to be the best

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estimate for the non-limited evapotranspiration of the soil/vegetation in the catchment area. The authors chose to calibrate the fraction forest cover ( $M$ ) determined from an analysis of the vegetation cover. They might have retained the forest cover, and calibrated the transpiration efficiency. The result would have been the same.

P 172 L20- P173 L 9: This paragraph contains a hypothesis regarding a connection between predictive quality and data quality which is not supported by any analysis. The analysis given is one in which one might present a figure which presents model efficiency as a function of total annual runoff. The sentence “One can easily argue that the model is probably less accurate in the simulation of low flows” could be based on such a figure. However, the authors continue to link data quality, notably the estimate of areal rainfall, to model efficiency. This is not an outcome of the analysis they presented, and should be deleted, as an unwarranted hypothesis. The subsequent part of the paragraph repeats part from the introduction, and may be deleted. Question: Would the authors regard this as over-parameterization?

P 172 L21-24: Please clarify: “excluding the worst and best years” and the worst efficiency was achieved in 1985? Could the authors explain in some more detail?

P 172 L22: “one can see”: could the authors add a table or figure which illustrates the points they wish to make in this paragraph?

P 173 L10-16 Figure 13 also suggests that saturation excess might be responsible for the overprediction of the peak flow, and that a model based on delayed flow and base flow would fit the data equally well. Could the authors comment?

P 173 Section 4.2 Investigation of anomalous years. Given their attention to the rainfall in 1975, I would expect the authors to analyze the year 1975 with some interest.

P 173 L10-16 This last paragraph refers to Figures 12, 13, 14 (as such it could be shifted to the location where 12-14 are mentioned the first time.

Question (Page 175 - line 5) Could the authors comment on the following: The fact

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that saturation excess seldom occurs might also point to the possibility that including saturation excess flow is irrelevant for this catchment, and effectively a model based on delayed flow and groundwater flow only might be sufficiently complex?

P 176 L 12-14: “therefore capable of depicting the dominant hydrological processes of catchments in this region” I feel the authors could provide more substance to this claim by either including arguments about the representativity of this specific catchment for the region they refer to (geology, land use, slopes, rainfall, evapotranspiration), or by presenting validation cases. At present I would urge the authors to delete this claim.

P 177 line 22-23: replace “permitted a deeper understanding of tropical catchments in the Northern territory of Australia.”by “permitted to gain some understanding of the rainfall-runoff behaviour of a tropical catchment in the Northern territory of Australia.”

P 177 L 24-25: “also helps to develop appropriate monitoring schemes.” Whereas I think this is an interesting idea, the authors have not indicated what kind of help a model could offer in developing appropriate monitoring schemes, and for what purpose this monitoring scheme would be appropriate.

## Technical corrections

Page 160 Line11. please replace "at the end" by "This procedure results in a"

P161 L 1ff. Please reformulate. Accurate streamflow predictions can provide basic information to projects that aim to guarantee sustainable environmental management, and to those engineering projects that aim to prevent and control floods (Predictions can not prevent floods, projects can)

P161 L 25 please delete vexed.

P162 L1 please reformulate The debate has seen a deep examination. e.g. There has been an intensive debate about

P 163 L27; please delete: the presence of

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P 164 L4: please reformulate: No open water is present, but swamps containing seep-ages occur in the tableland (I would regard springs as open water).

P 164 L6: please replace "grading" by "changing";

P 164 L7: please replace "at depths greater than" by "deeper than"

P 164 L9: please clarify: no abundance of mid stratum tree species. Are trees sparse?

P 164 L11: please reformulate: The catchment is monitored by the... Alternatively The ... (DIPE) monitors the catchment using two raingauges and a gauging station.

P 164 L15: replace "has been" by "was"

P 164 L18-21: reformulate: The analysis is based on the period common to both the rainfall and the river flow timeseries.

P 164 L25: shorten: In order to improve the quality of the meteorological input,

P 166 L 17-20: Please reformulate: “under the assumption that there are no unaccounted losses of infiltrated water that does not reach the basin outlet”. (double negative)

P 167 L 3: Please replace “worthful” e.g. take into account

P 167 L7: Please reformulate “opposite in phase” e.g out of phase; differ in phase by xx months

P 167 L 10: Please consider: The need to address month by number is caused by the way Figure 4 is plotted. There is no need to do this, e.g. a calculation algorithm, or a formula. Please relabel the axes using the name of the month.

P 167 L 19: Please replace “can be seen as”by “is”

P 167 L 28: Replace “realise” by conclude

P 169 L22: Please delete “Indeed”.

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P 170 L2: Note the point made earlier about the optimal number of buckets: in what sense is a 4 bucket model optimal?

P170 L4: Please delete “upon carrying out” manual calibration; replace by “after”. Replace : “model parameters assume the values” by “estimated values are presented in”

P 170 L11: Please replace the sentence beginning “The water balance equation... “with The difference equation for model B4GD is:

P 171 L 9: Table 2 does not present values for all model parameters; please add values for a and b.

P 171 L 19: Add symbol for bare soil evaporation (eb) in this line.

P 172 L 20: please replace “refined” by e.g. detailed.

P 174 L13 please reformulate “almost the entire annual rainfall total” e.g. x% of the annual rainfall fell in (delete “over”) 140 days (delete “a mere”).

P 175 L 24: please replace “exited” by “left”

P 175 L 24: “catchments” in plural?

P 177 L 8: please delete “a couple of”

Figure 4: The Figure legend either needs to indicate which number corresponds to which month, or the axes labels need to be given as month names (cf legend Figure 5)

Figure 9, 10, a.o. define Q, P on the axes.

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