Hydrol. Earth Syst. Sci. Discuss., 3, S66–S72, 2006
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European Geosciences Union

Hydrology and Earth System Sciences Discussions

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.

L. Montanari et al.

Received and published: 21 March 2006

First of all we wish to thank the anonymous Referee for the careful examination of our paper and the very detailed review he provided, which is to the point, critical and stimulating at the same time. We think the prompt and precise work done by the Referee is a significant demonstration of the potential value of this type of on-line review process.

In our reply we are focusing on the main remarks of the Referee, that is, on the most relevant issues related to the essence of our analysis. Of course we will keep into full account all the major and minor Referee's comments if we are encouraged to prepare a revised version of the paper.

We believe that the Referee is questioning two major points about the paper: first

of all, (1) he repeatedly asks for a more detailed description, maybe in a synthetic form, of all the modelling solutions we have considered while trying to identify the best approach for the considered watershed. Secondly, (2) the Referee questions an essential behaviour of our modelling study. That is, he would have expected that the models were parameterised without using any observed data, therefore emulating a completely ungauged situation and using the data only for model validation.

About point (1) listed above, we recognise the full validity of the Referee's concern. Therefore, in the revised paper we will provide a more detailed description of all the different modelling attempts we tried. We fear that a detailed description might be difficult to follow and somehow bewildering for the reader. Therefore, we would prefer to follow the Referee's suggestion thus summarising the results by using a table.

About point (2) listed above, we believe that we did not explain clearly enough the main focus of our analysis, which is primarily aimed to understand the hydrological behaviours of the investigated basin. We believe that it is helpful to remember here some basics concepts of the PUB initiative. First, Sivapalan et al. (2003) provide a definition for ungauged basin which reads: "An ungauged basin is one with inadequate records (in terms of both data quantity and quality) of hydrological observations to enable computation of hydrological variables of interest (both water quantity or quality) at the appropriate spatial and temporal scales, and to the accuracy acceptable for practical applications". In this view, we believe that our Australian basin can be considered ungauged, as we have available only rainfall, temperature and runoff records. In particular, we know nothing about basin morphology, soil type, soil use, vegetation cover, dominant runoff generation processes. In this perspective, one of the PUB scientific objectives is (Sivapalan et al., 2003) "Increase the awareness of the value of data. Quantify the links between data and predictive uncertainty". Moreover, one of the PUB science questions is "How can we maximize the scientific value of available data in generating improved predictions?".

In fact, when dealing with an ungauged/poorly gauged basin we believe we have three

HESSD

3, S66–S72, 2006

avenues of research we can follow if we need to set up a hydrological model: (a) if we do not have records of meteorological and hydrometric data, but we have extended knowledge about the basin, including topography, detailed soil and vegetation information, we can introduce hypotheses on the dominant hydrological processes, on the basis of the available knowledge, and apply a physically-based model, whose parameters can in principle be estimated on the basis of in-situ measurements and physical reasoning. (b) If we have no knowledge about the basin but we have (even scarce) meteorological and hydrometric records, we can build up a conceptual and simple model by introducing hypotheses on the dominant hydrological processes. Testing the model against the observed data allows us to check the correctness of the introduced assumptions. (c) If information is only available at regional scale, we can try to use regional methods for introducing hypotheses on the dominant hydrological processes, select a model and estimate its parameters. Ultimately, we can conclude that in any case we need a basic information in order to formulate and verify assumptions about the dominant hydrological processes.

We believe that our analysis falls within category (b) described above. As no knowledge is available about the catchment, we believe we need (as it is scientifically correct) to use the observed data in order to verify our preliminary hypotheses. Verification is necessarily done by performing model calibration, as we need to verify if the model is indeed capable to catch the main behaviours of the river flow pattern. We decided to use manual calibration because we felt that in the presence of uncertainty a mixed quantitative and perceptual evaluation of the different modelling solutions is a suitable choice. In fact, automatic calibration discriminates the different models and parameters sets on the basis of an objective function that may fail to comprehensively evaluate the goodness of the fit. Therefore, we believe the Referee's suggestion to use the observed data for model validation only is not appropriate in our case, as we are urged to use the full data record to perform model set up.

We believe that a model selection and parameterisation cannot be carried out in full

HESSD

3, S66–S72, 2006

absence of information. In this case, the modelling procedure would be a completely blind attempt that can be successful only by chance. Such procedure would not allow us to gain any significant additional knowledge about the basin. As a matter of fact, this is not the objective of the PUB initiative, which is instead aimed to set up and verify techniques for the full exploitation of the available information and knowledge. In this respect, we feel that our study is an instructive example of how to profit from scarce data for improving our basin knowledge.

Moreover, it is worth noting that in our opinion even a split sample application (i.e., to calibrate the model by using only a part of the data, therefore using the other part for model validation) is not indicated in the context of our analysis. Calibrating the models on a reduced amount of data would increase the uncertainty in the evaluation of the dominant hydrological processes. The scope of our analysis is not to set up a prediction model. Our modelling exercise is finalised to processes understanding and therefore we believe it is advisable to reduce the calibration uncertainty as much as possible by using the full data set.

We also would like to provide a brief comment about some less relevant issues raised by the Referee.

The Referee mentioned that we used the term "prediction" quite loosely. We completely agree, in fact we are not applying a set of validated parameters over a different series of data from that used in the analysis. Our purpose is in fact to investigate the main hydrological processes of the catchment and to acquire as much information as the data availability allows. For this reason, we make use of the whole data set to implement the model. According to these considerations we concur with the Referee's opinion that we have to change the wordings in the revised manuscript.

The Referee is concerned about the possibility that the analysis might be biased for the effect of measurement noise. In particular, his worst fear is that by using manual calibration we may generate systematic errors that we may try to compensate by

HESSD

3, S66–S72, 2006

increasing model complexity.

Of course, the presence of uncertainty might affect our results. This is the reason why: (1) we used simple models, precisely with the aim to adopt robust approaches which allow us to minimise the risk of over-parameterisation. (2) We used the full data-base for model calibration. (3) We used observed data for model calibration. We believe there is no way to further reduce the effects of uncertainty in our analysis.

We well know that uncertainty is always present in any hydrological study; all what we can do is to reduce it to the best of our possibility. We believe that the perplexity of the Referee would be justified if we did not use the available data for model calibration, or used only a part of them.

In consideration of the simplicity of our models and the type of calibration procedure we used, we feel that the conclusions of our analysis could not be heavily affected by the presence of uncertainty. In fact, the use of manual calibration allows us to check the results provided by the modelling solutions. We carefully inspected the reliability of each approach and the potential effects of uncertainty. In particular, we checked that the simpler models were rejected because of their effective inability and not because of unreliable parameterisation. We are reasonably convinced that the increase in model complexity was always justified and not induced by uncertainty or lack of model fit.

The Referee argues that we discarded a good amount of rainfall data. Unfortunately we did not understand the formulation he proposed to compute a weighted average of the rainfall observations collected in each one of the two raingauges. However, we fully understand that he suggests the use of a weighted average instead of the use of the best quality hourly record. We believe that this point might be a matter of opinion. However, we would like to note that there is a substantial dissimilarity between the quality classes of the observations. We wish to point out here that the data are classified as "good continuous data", "interpolated", "estimated", "not yet available" or "not recorded". Therefore, we are firmly convinced it is advisable to use the best

HESSD

3, S66–S72, 2006

quality data instead of a weighted average of two quality classes, that in our opinion would lead to a loss of information. The use of a weighted average is instead advisable when the quality of the data is the same. This is the procedure we used in our analysis by applying the Thiessen Polygons method.

As far as the exceptionality of year 1975 is concerned, the Referee suggests to further address to the causes of its anomalies. In particular he asks whether it might depend on the procedure we adopt to obtain the new series, from the two available series of rainfall observations. We can certainly affirm that this is not the case: year 1975 is in fact exceptional in both the rainfall series, of course when the data are available in both the raingauges therefore allowing a comparison. This assumption is strengthened by the analysis of runoff annual series: year 1975 is exceptional with regards to runoff too. This is a confirmation of the fact that the high rainfall recorded in 1975 is not a local phenomenon and that, moreover, it is not due to the rainfall spatial interpolation method.

The Referee is concerned about the possibility that saturation excess might be responsible for the over-prediction of the peak flow, and that a model based on delayed flow and base flow would fit the data equally well. We think this interpretation is not physically feasible. First of all, as the daily flow duration curve and the daily plots show, the peaks are not systematically overestimated. Moreover, if the saturation excess runoff was not considered, the peaks would be underestimated and the recession phase of the curve would become less steep than the observed one, as water from the catchment would be released over a longer period of time.

For the same reasons expressed before, we do not agree with the Referee that the rare occurrence of saturation excess flow might be the proof of its irrelevance for the catchment. As the results of our modelling attempts show, saturation excess flow is necessary to simulate the peak flows. We feel it is necessary to better comment these results in the revised version of the paper, as they are not discussed with enough detail in the present version.

HESSD

3, S66–S72, 2006

We fully agree with the essence of all the Referee comments we did not explicitly discuss above. We will address all the minor and major remarks if we are allowed to revise the paper.

Interactive comment on Hydrology and Earth System Sciences Discussions, 3, 159, 2006.