Hydrol. Earth Syst. Sci. Discuss., 10, C5326–C5330, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C5326/2013/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "On the lack of robustness of hydrologic models regarding water balance simulation – a diagnostic approach on 20 mountainous catchments using three models of increasing complexity" *by* L. Coron et al.

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Received and published: 1 October 2013

This is my review of the paper "On the lack of robustness of hydrologic models regarding water balance simulation – a diagnostic approach on 20 mountainous catchments using three models of increasing complexity" (Manuscript number: HESSD-10-11337-2013).

The paper investigates the adequacy of three conceptual rainfall-runoff models to represent the temporal variation of the water balance in 20 unregulated mountainous

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basins in the southern France. To generalise the conclusions, the hydrological models used (Mouelhi, GR4J-CemaNeige, and Cequeau) vary in complexity and spatial-temporal resolution, whereas the testing procedure was applied to a set of 365 basins with different hydro-climatic gradients. A 10-year sliding window was used to identify and further evaluate the models.

I have reviewed the manuscript, and I believe that overall it can sit well within the journal's objectives. The manuscript is very well written and the figures are appropriate. Most of my comments below are just expansions on good points made in the manuscript and the specific suggested comments are minor.

Page 11339

Line 13: Rephrase. Suggestion is to replace "from different countries" with "focusing on different hydro-climatic gradients".

Line 27: Rephrase. Suggestion is to replace "these problems that models have simulating" with "problem of the systematic biases on volume during simulation of".

Page 11344

Line 6: You need to explain what Q and Q ÌĆ are.

Page 11345

Line 11: Shouldn't the "52" be "53" instead?

Page 11346

Line 16: ". . . simulation errors.". Please define the range of errors in brackets.

Page 11348

Line 16: Equation 4 gives the standard deviation operator (σ), which according to the text (lines 4-5, page 11349) has an optimum of 0 (for a perfect model). However, according to the mathematics, is Equation 4, this is not correct. To do so, the numerator

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requires a term “minus mean of observed Q” and hence optimise the equation to 0. Also the 1/p term should be within the square root. I should note that I checked the results and they seem alright, so I believe that there was only a typographic mistake. I need the authors’ confirmation! Please check Equation 5 (Page 11349) also.

Page 11351

Lines 9-15: I believe that the conclusion stated in this paragraph could be subject to/an artefact of the models’ performance. The results presented here are based on the model’s bias which is only one of the three terms in the KGE measure. In order the reader to have a better understanding of the models’ adequacy, a table is necessary that summarises the KGE values for each model. A table indicating the 25, 50, 75th percentile of KGE (from the 20 basins) for each model (static optimum model) would be adequate.

Page 11353

Lines 9-10: Possible explanations for the Cequeau’s model’s performance are indicated. However, combination of all these reasons is also possible, so I suggest instead of “or” the use of “and/or”.

Line 23: Rephrase. Suggestion is to replace “. . . around 0.3, and 75% ...” with “. . . around 0.3, whereas 75% . . .”

Page 11354

Line 26: The authors mention that “. . . likely because Cequeau is slightly more robust . . .”. To justify this statement, it would be helpful to the reader to know the KGE performance values. This point is linked to comment 6 also.

Page 11359

General comments on Section 5 Maybe this specific comment would be more appropriate to be discussed in subsection 5.1. Although the paper aims to assess the ro-

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bustness of hydrological models in the time domain, its potential could be extended to multi-period identification and evaluation of hydrological models. This paper's analysis is subject to the selection of the window width (in this case, a 10-year sliding window was used), which needs to be discussed further. For instance, are the results/conclusions sensitive to the width of the window?

In addition, I believe that the authors should briefly discuss the potential usefulness of multi-period model evaluation in which periods are identified based on statistical analysis (de Vos et al., 2010, Zhang et al., 2011) or even identification of inflection points in the climatic signal. Would it be more adequate to identify the information content in each sub-period and force the model to represent highly informative periods?

de Vos NJ, Rientjes THM, Gupta HV., 2010. Diagnostic evaluation of conceptual rainfall-runoff models using temporal clustering. Hydrol Processes. 24(20): 2840–2850.

Zhang H., G.H. Huang, D. Wang, and X. Zhang (2011), Multi-period calibration of a semi-distributed hydrological model based on hydroclimatic clustering. Advances in Water Resources, 34, 1292-1303.

Lines 5-7: This is a topic broadly investigated. Additional studies would be: Bai Y., Wagener T., Reed P., 2009. A top-down framework for watershed model evaluation and selection under uncertainty. Environmental Modelling and Software, 24: 901-916.

Fenicia F., McDonnell J.J., and Savenije H. H. G. 2008. Learning from model improvement: On the contribution of complementary data to process understanding. Water Resources Research, 44(W06419):1–13.

Pechlivanidis I.G., McIntyre N., Wheater H.S., 2010. Calibration of the semi-distributed PDM rainfall-runoff model in the Upper Lee catchment, UK. Journal of Hydrology, 386(1-4): 198-209.

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Lines 3-6: The authors assessed the sensitivity of their results to the PE estimation formula and concluded that PE estimates do not affect the performance of the hydrological models. However, I believe that this can be misleading and it is important to note that PE estimates do not affect the models' performance in the present climate (particularly because parameters can compensate for those types of biases/errors/uncertainties). For instance, other studies have showed that significant biases can be introduced when different PE methods are used in climate change impact studies (Milly and Dunne, 2011).

Milly P.C.D., Dunne K.A., 2011. On the hydrologic adjustment of climate-model projections: The potential pitfall of potential evapotranspiration. Earth Interactions, 15(1): 1-14.

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Fig. 2: What does the "m" mean? Models?

Best regards,

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 11337, 2013.