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Hydrology and Earth System Sciences
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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.

L. Coron et al.

laurent.coron@irstea.fr

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Answer to Referee 1

NB. The referee comments have been repeated here and are written inside < < > > symbols.

< < Page 11339 Line 13: Rephrase. Suggestion is to replace “from different countries” with “focusing on different hydro-climatic gradients”. > >

We rephrased the sentence (but we kept the emphasis on the variety of the research teams rather than the variety of hydrological conditions associated with the studies).

< < Page 11339 Line 27: Rephrase. Suggestion is to replace “these problems that models have simulating” with “problem of the systematic biases on volume during simulation of”. > >

The sentence has been simplified and replaced by “Solving these problems of incorrect water balance simulation requires further investigations and has motivated the study reported herein.”

< < Page 11344 Line 6: You need to explain what Q and Q are. > >

A sentence has been added: “where Q and Q are the time series of observed and simulated flow, respectively, while . . .”

< < Page 11345 Line 11: Shouldn’t the “52” be “53” instead? > >

Yes, this was changed.

< < Page 11346 Line 16: “. . . simulation errors.”. Please define the range of errors in brackets.>

We modified the sentence accordingly: “One can note significant simulation errors: the range of bias variations being 17.7% in this case, with a standard deviation of 4.7%.”

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Interactive Discussion

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< < 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 requires a term "minus mean of observed Q" and hence optimises 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. > >

The referee is correct about mentioning an issue here. This error lays in the formula written but fortunately not in the computations which are reported in the paper.

The computations reported in the paper correspond to the standard deviation, as it was mentioned in the text and shown using notations such as $\sigma[\omega_{\theta_{TP}}]$. However, the formula presented in equations (4) and (5) were wrong in the submitted version, as they do not reflect standard deviation computations (the ratio 1/p was misplaced and more importantly, "minus mean" component was missing as noted by referee 1).

The corrected version of the standard deviation can be written: $\sigma\left[u\right] = \sqrt{\frac{1}{p}\sum_{k=1}^{p}(u_k - \overline{u})^2}$

The "minus mean" component ensures the optimal situation at zero for all $\omega_{\theta_{TP}}$ curves. This is why two curves with exactly identical shapes but at different vertical positioning have the same value, and thus, why we are allowed to compare ρ_i values.

The corrected versions of the equations are the following:

$$\sigma\left[\omega_{\theta_{TP}}\right] = \sqrt{\left(\frac{1}{p}\sum_{k=1}^{p}(u_k)^2\right) - \left(\frac{1}{p}\sum_{k=1}^{p}(u_k)\right)^2} \quad ; \quad u_k = \frac{\left[\overline{\widehat{Q}_{SP[k]}}\right]_{\theta_{TP}}}{\overline{Q_{SP[k]}}} \tag{4}$$

$$\sigma\left[\omega_{\theta_{SP[i]}} - \omega_{\theta_{TP}}\right] = \sqrt{\left(\frac{1}{p}\sum_{k=1}^{p}(v_k)^2\right) - \left(\frac{1}{p}\sum_{k=1}^{p}(v_k)\right)^2} \quad ; \quad v_k = \frac{\left[\overline{\widehat{Q}_{SP[k]}}\right]_{\theta_{SP[i]}} - \left[\overline{\widehat{Q}_{SP[k]}}\right]_{\theta_{TP}}}{\overline{Q_{SP[k]}}} \tag{5}$$

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< < Page 11351 Lines 9-15: 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. > >

This sentence was almost fully removed since it was unclear and did not add much. The previous sentences were adapted to improve clarity. In addition, a table giving an overview of the model performances was added in the paper.

< < Page 11353 Lines 9-10: [. . .] I suggest instead of "or" the use of "and/or". > >

The sentence was modified accordingly.

< < Page 11353 Line 23: Rephrase. Suggestion is to replace ". . . around 0.3, and 75% ..." with ". . .

around 0.3, whereas 75% . . ." > >

The sentence was clarified (we chose not to introduce the word "whereas" since there is no opposition between a median at 0.3 and a 75th percentile at 0.5).

< < 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. > >

A table containing the performances was added and a reference to it was added in the sentence quoted by the referee.

< < Page 11359: [. . .] 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? > >

This sensitivity actually has little influence on the main finding we aim to share with the paper, which is the "parallelism effect" between volume error variations when different calibration periods or model structure are considered.

Yet this sensitivity was investigated as part of our testing scheme. The paper was modified and we now report these tests and associated results. The corresponding new 16 lines are in section 5.1: "Other tests could be made [. . .] model efficiencies during validation."

< < Page 11359: 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? > >

It might indeed be a good direction to go if the reasons for the models robustness issues could be fully understood. Calibration strategies could be used to allow for the selection of more robust (i.e. better transferable parameter sets). During our work on this issue (cf. Coron, 2013, PhD thesis) we did try to look at calibration strategies designed to ensure better parameter transferability in time. As mentioned in the discussion (section 5.1), we even attempted to calibrate the GR4J-CemaNeige model on the total records with the exclusive aim of minimising the standard deviation on the 10-yr-mean flow

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volume errors. Yet these tests remained unsuccessful. Still, we agree with the referee that statistical analyses of the period are useful tools, which can help in analysing pairs of sub-periods where parameters transfers are problematic. The paper was modified to mention these aspects and the suggested references were added (see section 5.1 in the revised version).

< < Page 11359: Lines 5-7: This is a topic broadly investigated. Additional studies would be: Bai et al. (2009), Fenicia et al. (2008) and Pechlivanidis et al. (2010). > >

Indeed. Two out of the three references suggested by the reviewer were added to the paper.

< < Page 11360 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 [. . .] other studies have showed that significant biases can be introduced when different PE methods are used in climate change impact studies. . . > >

We fully agree with the reviewer. Adjustments were made in the section 5.2 and the work from Milly & Dunne (2011) was added in the references. More generally, we tried to put greater emphasis throughout the paper on the role PE estimates errors may have on the test results.

< < Page 11373 Fig. 2: What does the "m" mean? Models? > >

This was a typo which has been corrected: "m" was replaced by "parameters".

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END

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/10/C6385/2013/hessd-10-C6385-2013-supplement.pdf

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