Hydrol. Earth Syst. Sci. Discuss., 12, C4332–C4338, 2015
www.hydrol-earth-syst-sci-discuss.net/12/C4332/2015/

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Evaluation of global fine-resolution precipitation products and their uncertainty quantification in ensemble discharge simulations" *by* W. Qi et al.

Anonymous Referee #4

Received and published: 20 October 2015

General Comments

This manuscript evaluates the application of fine-resolution rainfall products in hydrologic simulation using two models with uncertainty quantification done using an ANOVA based approach. However, before the hydrologic simulations were done, the quality of the fine-resolution precipitation products was evaluated using statistical methods. The manuscript is written in readable English. The thrust of the work presented has good value and it is worth considering for publication in HESS. I however have some concerns with the submitted manuscript as outline below:

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

Major Comments

1 Compensation of errors

Consistently in the manuscript the authors make an argument summarized by this statement in the abstract: “It is also found that a good discharge simulation depends on a good coalition of a hydrological model and a precipitation product, suggesting that, although the satellite-based precipitation products are not as accurate as the gauge-based product, they could have better performance in discharge simulations when appropriately combined with hydrological models”. The authors call this a “comprehensive result” on P9357, L26-28. See also: the 5th point of the Conclusion (P9362).

I have challenges with this point, which is presented as a buzz point of the manuscript. It would appear to me that the models could be compensating for the errors/uncertainty in the input precipitation products. Generally, this compensation manifests in some instances in the form of questionable/unrealistic model parameter values (see Nikolopoulos et al.,(2013)). Looking at Table 1 and 2 one cannot help but notice some potentially odd values that might not compare with general range of values found in most publications. Whilst the authors do not explain the geology and soil type of the study region, except that the study area is a mountainous region, parameter values like: lnTo(why is negative?), RV, saturated hydraulic conductivity and the hydraulic conductivity anisotropy ratio need to be evaluated and justified.

A discussion on where or how the precipitation error/uncertainty was compensated should have been presented!

Unfortunately, this makes the key conclusion or “comprehensive result” of this work stand on shaky ground, if the input error is being compensated in parameter value estimation.

2 Precipitation performance argumentation

The authors in presenting and discussing their evaluation of the performance of pre-

HESSD

12, C4332–C4338, 2015

cipitation product take an algorithmic inclined line. Basically, the authors were arguing that, say, if product X outperforms Y, the authors then say to improve Y the algorithm behind X should be taken-up/considered. This line of discussion or argumentation runs through their whole presentation in Section 3. As example on Page 9353, L20, the authors say "Thus, if TRMM3B42 wants to improve heavy precipitation estimation, the artificial neural network function [of PERSIANN] and APHRODITE products could be helpful".

Although in the particular reference cited above, the ANN is recommended when it had overestimated heavy precipitation (why?), my challenge is with overall approach of argumentation. It is not only the main statistical or mathematical equation behind a product that determines its success. It could be influenced by other factors such as the inputs used in the merged products or any internal calibration or merging procedures etc. Without addressing such confounding factors how do the authors draw up their discussions and conclusions along these lines? I did not see sufficient evidence in the manuscript to warrant such a discussion and conclusion to the extent of singling out the statistical or mathematical equation. The cause-effect framework is too simplistic and limited!

I feel the authors could have gotten more purchase by discussing their results with the perspective of use/application of the products! Section 3 was generally not well presented!

3 Other argumentation issues:

> Arguments without clear process-level backing: There are instances in the manuscript that the authors make arguments without adequate process-level backing. On P9353, L1-5, that the authors argue that the performance/accuracy of PERSIANN is related to latitude! From a process-level perspective this is difficult to comprehend. Is this process related or it's related to the sensors used? The authors should ensure that there is process-level sufficiency in their statements in the manuscript.

HESSD

12, C4332–C4338, 2015

> Arguments without backing: On P9352, L1-5: Without seasonal analysis, how do the authors pinpoint summer convective rainfall with the authority that they do in the text? P9357, L3-5: How is the non-linear amplification derived and supported?

> P9356, L20-23 “Big differences. . .using PERSIANN”, where can the reader see this?

4 Flows – P9360

> L3-5: You should highlight to the reader what “small”, “middle” and “large” discharge means using a value or a quantile and its corresponding discharge value?

> L7-9: “This may. . .discharge magnitude”. What is the meaning of this statement? Explain to the reader what “interaction effects” are – and maybe give an example?

> L 13-15: Whilst it is vague what the authors mean by middle flows numerically, I am not sure that the statement holds physically. I would think contributions from groundwater flows in such a simulation would be baseflows and these would be low flows – in your terminology, what you call “small” discharges?

> L22-24: How is this practically feasible that precipitation products provide such information?

5 Use appropriate terminology and scientific English. For example the issue of “small”, “medium” and “large” discharges highlighted above. In section 3, avoid using the word “worst” in your comparisons.

6 The scheme in Figure 2 – Did you use this scheme eventually in the paper? Or it’s something you forgot to edit out? In case you used it, on P9344, you will need to justify the experimental set ups you came up with e.g. why not GLDAS and APHRODITE?

7 Why didn’t you apply the same calibration method for both models?

8 CDFs - (P9354) L14-25: How are you defining “larger rainfall intensity”? Could it be due that there is usually limited “larger intensity” events/observations in those regions of any distribution such that it appears as good performance, but in actual fact it’s just

HESSD

12, C4332–C4338, 2015

spurious good performance?

9 P9358, L11-14: Is this really counter-intuitive? It is accepted that good modelling work depends on having good data and a good or appropriate model structure

10 Without advocating for long and winding papers, generally it appears that the writing leaves a lot of the issues hanging or incompletely presented. As an example in section 1.0, as a reader one does not get the continuity of the text. Please revise the manuscript.

Technical Corrections

P9338, L1-7: Sentence too long. Please revise!

P9338, L5: "usually-neglected area" has no hydrologic value. Remove!

P9339, L5-7: "However. . .rural areas". Revise!

P9340, L21-23: Add reviews of uncertainty quantification/analysis work by Kuczera et al., (2006), Vrugt et al (2009b), Vrugt et al (2009a), Tolson and Shoemaker (2007).

P9341, L3-6: Revise! The repeated use of "and" makes the sentence difficult to read.

P9341, L11: Remove "usually-neglected area"

P9341, L14: Replace "include" with "are"

P9344, L17: Use the common-abbreviation NSE instead of NSCE throughout the manuscript.

P9345, L4-6: So?

P9346: If you don't have hourly data, why force it?

P9346, L22-24, "Surface air. . . gauges" Not clear. Explain!

P9347, L14: Secondly instead of second.

HESSD

12, C4332–C4338, 2015

P9347, L26: add ‘the’- “. . . of the above mentioned. . .”

P9347 – Fig 3 contains results and these should be presented and discussed in the results section and not here! Discuss your parameter values also.

P9349, L14-15: this line is too similar in phrasing to what is in the paper by Bosshard et al.(2013). Revise! P9351, L21-23: Revise!

P9353, L3: This may be attributed to the different. . .

P9353, L10: Is the “trend” visually assessed?

P9357, L14-23, “In the case. . . and models”: Highlight where the peak discharge over/under estimation assessment is coming from? Clarify the reason in simple terms for the reader – what is the hydrological model influence and interactive influence?

P9358, L3: Is no better than?

P9358, L4: This could be due to. . .

P9358, L19-25: Review and revise! The conclusion on TOPMODEL vs. WEB-DHM appears rushed. It’s obvious that using good data and an inappropriate model structure results in poor performance, if parameter physical implications are kept in check! How do we achieve the coalition?

P9359, L7-9: “This shows. . . accuracy” - What do you mean?

P9361, L1-2: Sentence is incomplete!

References

Bosshard, T., Carambia, M., Goergen, K., Kotlarski, S., Krahe, P., Zappa, M., Schär, C., 2013. Quantifying uncertainty sources in an ensemble of hydrological climate-impact projections. Water Resources Research, 49(3): 1523-1536.

Kuczera, G., Kavetski, D., Franks, S., Thyer, M., 2006. Towards a Bayesian total error analysis of conceptual rainfall-runoff models: Characterising model error using storm-

HESSD

12, C4332–C4338, 2015

dependent parameters. Journal of Hydrology, 331(1-2): 161-177.

Nikolopoulos, E.I., Anagnostou, E.N., Borga, M., 2013. Using High-Resolution Satellite Rainfall Products to Simulate a Major Flash Flood Event in Northern Italy. Journal of Hydrometeorology, 14(1): 171-185.

Tolson, B.A., Shoemaker, C.A., 2007. Dynamically dimensioned search algorithm for computationally efficient watershed model calibration. Water Resources Research, 43(1): W01413.

Vrugt, J., ter Braak, C., Gupta, H., Robinson, B., 2009a. Equifinality of formal (DREAM) and informal (GLUE) Bayesian approaches in hydrologic modeling? Stochastic Environmental Research and Risk Assessment, 23(7): 1011-1026.

Vrugt, J.A., ter Braak, C.J.F., Diks, C.G.H., Higdon, D., Robinson, B.A., Hyman, J.M., 2009b. Accelerating Markov chain Monte Carlo simulation by differential evolution with self-adaptive randomized subspace sampling. International Journal of Nonlinear Sciences and Numerical Simulation, 10(3): 271-288.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 9337, 2015.