

## ***Interactive comment on “Assessing the hydrological effect of the check dams in the Loess Plateau, China by model simulations” by Y. D. Xu et al.***

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Received and published: 13 March 2013

Comment: Only one hydrologic station was used in the evaluation. Can't more flow data at other hydrologic stations in this watershed be used in judging model's performance? Any information on evapotranspiration available which can justify the accuracy of simulations?

Answer: Totally, there were 5 hydrologic stations in the watershed. In Y D Xu's PhD thesis, hydrological data of these 5 hydrologic stations were used for comparing with the simulation results. The conclusions were all the same, that is, the simulation results

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were acceptable in 1956–1966, but unreasonable in 1984–1987 and 2006–2008. The effect of check dams deduced by compare of simulated and observed data were also the same. In consideration of the article's length and results' conciseness, the Ganguyi hydrologic station was selected in this article, for it's the the most downstream station and could represent the most part of the watershed. This explanation was added by the end of introduction, and Y D Xu's PhD thesis was added into the references.

Observed evapotranspiration data were unable to get in the studied watershed. The simulation could only be validated by runoff and sediment data. The simulated evapotranspiration was only used as reference to judge if the results were reasonable.

Comment: What about the well-known equi-finality problems in hydrologic modeling? How to prove or convince that the model prediction presented here is unique, and not depending on the single parameter set chosen?

Answer: Equi-finality (non-uniqueness) is a common problem in hydrological modeling research. In the article, we tried our best to avoid this problem. Firstly, we strictly followed the warmup-calibration-validation procedures, which test the parameter set in several periods. Secondly, an automatic calibration software, SWAT-CUP (SWAT Calibration and Uncertainty Programs) was applied in our research. It was designed partly to allow for the possible equi-finality of parameter sets during the estimation of model parameters. The calibration results were not specific values but ranges of values. Thirdly, the parameters chose in the calibration mostly had physical meanings. The calibration results of the parameters had been verified by the field investigated values.

Moreover, if the equi-finality problem did exist in the simulation, it could be inferred that there were sets of parameters with which the simulations in 1980s and 2000s would be approximate with the observation. However, after altering the adjustment ranges of parameters and running SWAT-CUP repeatedly, the “correct” parameter sets were unfound. It's also an evidence for the model prediction.

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The above was simplified and added as the first paragraph in the "Discussion" section.

Comment: The presentation of major findings in the Abstract and Conclusion was all in terms of the relative percentage of changes. Please compare by using the actual numbers and put the percentage change together within the parenthesis.

Answer: The results were adjusted as this suggestion. The absolute numbers were added in the revised manuscript and the percentage changes were put in the parenthesis.

Comment: The authors has to present the overall water balances (between precipitation, evaporation and river flow) for different periods considered as one key Table.

Answer: For the lack of observed evaporation data, it's not attainable to analyse the actual water balances. Evaporation could be simulated in the model. However, the main conclusion of the article was based on the fact that the model could not correctly simulate hydrological processes affected by check dams.

Therefore, the observed precipitation and river flow were averaged and constituted a table. The runoff coefficients (runoff depth / precipitation) in different periods were calculated. It lead to an encouraging conclusion, that was, the runoff coefficients declined from 1950s and 1960s to 1980s and 2000s (see Table 1). Especially in 2006-2008, the runoff coefficient was less than half of the ones in the two earliest periods. This result further buttressed the conclusion of the article. The table was added into the "Discussion" section.

Comment: The arrangement of the figures is too rough and leaves much room for improvement. For example, Figs 1 and 2 can be together (in fact both photos are not absolutely necessary to be presented in the paper); Figs 4 and 5 can be merged into one figure, and Figs 6 and 7 too.

Answer: The figures were merged according to this suggestion.

Comment: The "95% prediction uncertainty" was plotted in almost every figure, but no

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explanation was offered. What is the definition? How to derive that uncertainty? What's the implication?

Answer: The 95% prediction uncertainty was a variable used in the SWAT-CUP, for the parameter calibration results were ranges and the simulation results were also ranges. The variable was explained in the text where it first appeared.

Comment: Page 13493, line 14: change "Overtime" into "over the time". page 13494, line 10: change "Xu Xiangzhou and his colleagues (Xu et al., 2002, 2004)" into "Xu et al. (2002,2004)". page 13496, line 5: can the unit be more clear? For example, change "t" into"ton"? page 13496, line 8: change "much cropland were" into "many croplands were". page 13496, line 9: change "for quite long period" into "for a quite long period".

Answer: Thanks for these specific comments. All of these were corrected.

Comment: Page 13496, line 28: only one reference was cited here. Can some more relevant references be added in the end of this line (since the authors said "from some literatures and field works").

Answer: Two more references were added.

Comment: Page 13497, line 1: "Precipitation and hydrological data. . .", please be more specific what kinds of hydrologic data you are referring to? Also the same comments can be applied to page 13496, line 14.

Answer: Hydrologic data here meant monthly flow and monthly sediment yield. It's amended in the revised manuscript.

Comment: page 13497, line 15: sometimes "long-term" was used, sometimes "long term", please be consistent throughout the manuscript. Also, change "watershed scale model" into "watershed-scale model". page 13498, line 13: Here, the authors have to explain what does the overbar mean in equations (1)-(3). page 13499, line 1: please mention which variable (river flow?) will be used for Eq(3). Page 13500, line 13: Avoid using the word "so" in scientific papers. "Therefore" "Thus" can be much better. page

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13513, Fig. 3: The watershed shape looks not consistent at all with that in the inset of this figure. Please improve the quality and accuracy of this figure.

Answer: All above were amended according to the comment.

Comment: Page 13499, line 6: The reason is? Land use map only available for two years?

Answer: Land use maps were limited. Among the land use maps we had, the nearest one for 1984-1987 was the land use map of 1990, and for 2006-2008 was the land use map of 2008.

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/9/C7063/2013/hessd-9-C7063-2013-supplement.pdf>

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 13491, 2012.