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Hydrology and Earth System Sciences
Discussions

# *Interactive comment on* "Insights from a new methodology to optimize rain gauge weighting for rainfall-runoff models" *by* Ashley J. Wright et al.

**Anonymous Referee #3**

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This manuscript presents a new method to weigh available rainfall gauges in a way that maximizes the performance of hydrological model in terms of streamflow simulation. The method is tested for 3 hydrological models for streamflow simulation of 7 Australian catchments and its performance is compared to a classic inverse distance weighing (IDW) approach.

The proposed method has its merits and was reported to outperform IDW method in the majority of tested cases. However, the proposed method was compared to only one arbitrary selected interpolation method using only one arbitrary selected performance measure of model efficiency. Little discussion was provided on possible model parameter uncertainty and its effect on the performance of the proposed method. Moreover, the Abstract and Conclusion of the manuscript highlight the importance of the proposed



method for flood forecast, whereas in the performed analysis the emphasis is on the simulation of the historical streamflow time series. Finally, the Introduction lacks presentation of existing methods on rainfall optimization and correction techniques for hydrological modeling. Therefore, I think substantial revision of the manuscript is required before it can be accepted for publication. Below I present my detailed comments.

General comments

1. According to the Abstract and the Conclusion of the manuscript the purpose of the proposed method is to obtain superior flood forecast. However, in the technical part of the analysis the proposed method is not tested for such purposes, but rather for the simulation of historical streamflow time series. In the description of the catchments, noticeable flood events are described in great detail, but none of the information regarding these flood events is used to test the proposed method. The proposed method can be indeed useful for flood forecasting, but since this was not the scope of the analysis I would suggest authors to modify abstract and conclusions accordingly by reporting the results that are actually the outcome of this study.

2. The focus of Introduction is largely on issues regarding systematic errors in rainfall measurement and quality control of these measurements. This is an important topic indeed, but in my opinion current Introduction is disconnected from the proposed method and the performed analysis. Additional information about existing methods on optimization/correction of rainfall would be more welcome (e.g., optimization of areal rainfall (e.g., Anctil et al., 2006) or using precipitation correction factor as a calibration parameter of hydrological models that is often used in data scarce conditions (e.g., Schaefli et al., 2007; Duethmann et al., 2013)).

3. In the Introduction (Line 44-50) the authors show that existing studies do not give a univocal answer on the question which interpolation method result in better streamflow simulation, since the performance of different interpolation methods for streamflow simulation might vary largely when different models, different performance metrics and

different catchment are used. Moreover, the authors highlight that none of the existing studies used all interpolation methods making it difficult to identify the most appropriate method for streamflow simulation. I fully agree with these statements of the authors, but I am surprised that for their own study the authors provided little motivation on the choice of a single baseline interpolation method (i.e., IDW) and a single performance measure (i.e., RMSE) for model evaluation. Moreover, the detailed results were presented only for one out of seven catchments. The differences in performance for different catchments were not discussed in the context of different catchment characteristics and climatic settings.

4. Little attention was paid to the uncertainties that might arise due to the parameter equifinality when the different precipitation inputs are evaluated by goodness-of-fit of simulated streamflow. This is an important topic in the field of precipitation benchmarking (e.g., Yilmaz et al., 2005; Heistermann and Kneis, 2011). The proposed method is likely to suffer from similar problems. Since Figure 7 only report the average value of calibrated parameters for 6 different spilt samples, it is not possible to say if the proposed method is affected by parametric uncertainties, but large variability of gauge weights among split sample (Figure 6) hints in that direction.

Specific comments

Abstract Line 6-7: According to Line 210 of the Result Section 15.3% and 7.1% correspond to the improvement when only 5 out of 7 catchments are considered.

Line 38-39: It seems for me that the main focus of the study is developing a new method for rain gauge weighing rather than determining a superior interpolation method since only one existing method (i.e., IDW) was examined.

Line 57-60: What about modeling approaches when precipitation correction factor is used as calibration parameter. I think it will be advantageous to mention these techniques here to highlight the novelty of the proposed method.



Line 64-66: Despite what is claimed here I found no analysis of potential influence of catchment characteristics on performance of the proposed method.

Section 2: No information on used temperature data is provided, which I assume is necessary to run hydrological models used in this study

Line 69: What method was applied to derive monthly PET? Is that a better choice to rely on monthly to hourly disaggregated values than to compute a simple temperature-based PET from hourly temperature data directly?

Line 74-75: Here readers are referred to Table 1 for catchment properties. However, Table 1 only provides catchment size. Consider change the wording in these Lines or provide more catchment properties in Table 1.

Line 79-97: Some details regarding catchments (e.g., when the worst flood has occurred or number of recent floods) are not relevant for the performed analysis. On the other hand, additional information regarding catchment physiography, such as, elevation range, mean annual precipitation etc. would be useful for understanding the difference among the catchments and will give a chance to put the findings of the study in the context of different hydrological conditions. Moreover, instead of describing geographical location of the catchments consider providing a map of study catchments. Please indicate the role of possible anthropogenic influence on streamflow simulation in these catchments.

Line 99-101: Please provide a rationale on selecting these three models. Are any of them used in operational flood forecast?

Line 116: The value of power parameter is chosen arbitrary. Please provide a rationale for this value.

Line 121-135: This portion is difficult to understand. Consider revising it. Please provide more details on optimization procedure for weights definition. Was it a part of calibration procedure, where all the weights were identified simultaneously with model

parameters? Where do coefficients (e.g., 2.03 and -4.74) in equation 4 come from? Please provide an explanation. Finally, in this section you refer to likelihood function that explained only later in Section 3.3.1 and is specific to the selected optimization algorithm. Can the proposed gauge weighing method be used in the context of optimization algorithms that rely on maximizing objective function instead of likelihood function?

Section 3.2: Please provide more details regarding set up of hydrological models (i.e., lumped, semi-distributed, distributed).

Line 145-146: Please explain in more detail what is “catchment exchange term”. Is it one of the calibrating parameters?

Line 175-179: Why RMSE was chosen as a performance measure instead of common metrics such as NSE or KGE?

Table 2, Table 3: consider merging Table 2 and 3 to ease comparison between optimization and evaluation periods described in Lines 181-192. Consider change the name of the column “Opt” to OGW to be consistent with the text.

Line 185-187: I did not understand this sentence.

Line 189: Where can the reader see that IDW rainfall for GR4H model had the lowest RMSE for 5 out of 7 catchments? According to Table 2 in optimization period 6 out of 7 catchments had lower RMSE for IDW than for OGW. According to Table 3 for evaluation period none of 7 catchments had smaller RMSE for OGW.

Line 190-193 and Line 210: Is it feasible to compare the performance of the best IWD to the best OGW case disregarding of the model used? According to Introduction (Line 48-50) the success of interpolation methods is greatly influenced by the choice of hydrological models.

Line 193-203: Is the proposed method is at all feasible for the conditions when gaps are present in the data? Is it possible that the largest weight will be assigned to the gauge



with data gap that covers the evaluation period? In this case it is likely to deteriorate model performance in the evaluation period that could have been compensated by the information from other gauges in the traditional interpolation methods.

Line 203-206: Is it possible to define/suggest a minimum rain gauge density required for the successful application of the proposed method?

Section 4.1: Why Paddys Flat was selected as a case study?

Figure 3: Is this Figure necessary? The Figure simply shows observed streamflow and IDW interpolated rainfall for the whole study period. It would be more informative if apart from observed discharge it will display discharge simulated by different models with corresponding OGW rainfall for a selected period so that discharge would be actually visible on the Figure.

Line 227: OGW leads to increase of cumulative rainfall for HBV and PDM for all split samples, however for some split samples of GR4H the cumulative rainfall is similar to the IDW rainfall.

Line 233-235: These findings are based on Paddys Flat that according to the Figure 2 did not have large change of rainfall amount compared to IDW. Are these results similar for Tully catchment where the difference between OGW and IDW rainfall amounts was larger than 1000 mm?

Line 236, Line 281: The developed method identifies gauges that add value for the streamflow simulation. This manuscript did not investigate the value of identified gauges for the flood forecast.

Figure 7: Instead of average value of parameters for OGW please provide a box plot of parameter values resulting from each split sample to evaluate the stability of parameters. The decimal point on y axis is missing.

Line 244-250, Line 258-260: Since the model parameters were not introduced for each model in the Methods Section, please specify which parameter mentioned here and

are responsible for soil moisture, evapotranspiration, percolation, fast flow, base flow, catchment exchange parameter etc. to make interpretation of Figure 7 easier.

Line 272-273: This study does not investigate which model structure is likely to benefit more from inclusion of soil moisture for calibration or assimilation purposes. Consider adding citation or revising this sentence.

Line 269-270, 275-278 and Figure 9: Figure 9 is not appropriate to make any conclusions regarding IDW and OGW performance for different streamflow events. On this Figure individual events cannot be identified unambiguously and it is hard to say which IGW event correspond to which OGW event. Consider modifying this Figure by selecting several different streamflow events and showing performance of different gauge weighing methods and models for them.

Line 279: Add the results for all study catchments as supplementary material to prove this point.

Line 287-289: Is it because of inability of GR4H to represent the internal dynamics of the catchments or is it because the catchment exchange parameter accounts and corrects for possible bias present in the input data making adjustment of gauge weight (that in essence also corrects for input data bias) redundant?

Technical corrections

Line 23 and 37: Using "QC'd" instead of "quality controlled" is confusing. In general, consider spelling "quality control" instead using QC. It is only used 4 times in the paper.

Line 41: "Thiessen polygons" instead of "Theissen's polygons"

Line 46: Abbreviations OK and PCRR are not used further in the text and therefore can be omitted.

Line 49: "is" instead of "iss"

Line 82: Abbreviation SA is redundant as it is not used further in the text



Line 87 and 93: Abbreviation NSW is redundant as it is not used further in the text

Line 95: "Queensland" instead of "Queenslad"

Line 106: "operator" instead of "oeprator"

Line 107: "an" instead of "on"

Figure 1: Decimal point on y axis is shifted

Figure 4: Label the different split samples according to the period they were calibrated on

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