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Discussions

Interactive comment on "Regionalising rainfall–runoff modelling for predicting daily runoff in continental Australia" *by* H. Li and Y. Zhang

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First, we would like to thank the critical review from this anonymous referee, and thank the HESS editorial office to provide us an opportunity to clarify the concerns and address the queries. We have copied the comments one by one and each of them is followed by response (separated by 'End of this response' statement).

In this study four regionalisation methods are used to predict streamflow in continental Australia. The authors claim that regionalisation studies at the continental scale are almost non-existent in the literature. However, I do not believe that applying well known regionalisation methods to a different dataset represents a sufficiently novel contribution to the already extensive literature on this topic. Furthermore, it should not come as a surprise to the authors that spatial proximity methods do not provide the best

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results, when, for many ungauged catchments, the nearest gauges are located thousands of km away. Spatial proximity methods are not supposed to be used in such circumstances, and, therefore, I do not see the value in comparing this method with others in the context of the authors chosen case study.

Re: It is indeed that there are few regionalisation studies carried out at the continental scale. We DID NOT claim that the regionalisation studies across a continental scale will be a sufficient novel contribution. Instead, we comprehensively compare the four regionalisation methods across the continental Australia. In addition, we feel a bit surprise that the merit of this study using 600+ catchments has not been pointed at all.

We DO NOT agree that there are no merits to apply spatial proximity method for far regional distance. There are no reports in literature that how the spatial proximity performs with the increase in regionalisation distance for Australian catchments. This is particularly important since most Australian catchments locate along coastal regions, but the inland and central Australia has very sparse gauges. It is not clear how the very uneven distribution of the catchments influence performance of different regionalisation approaches. Our study indeed demonstrates that use of the gridded integrated similarity approach outperforms the spatial proximity in data sparse inland and central Australia. (End of this response)

In addition to these significant limitations of the paper, I have four other major concerns with this paper: 1) The results do not support the conclusions. I will provide some examples, but more can be found in the text. On page 11, lines 10-17, the authors report the median NSEsqrt and the median NSE for each regionalisation method for both SIMHYD and Xinanjiang model, which come from the boxplots shown in Figures 2 and 3. The authors state afterwards that 'This result suggests that the gridded SP approach is better than the SP approach : : :' It is unclear to me whether the authors make such a statement based on the median value given in the previous sentences of that paragraph. If that is the case, this statement is incorrect as the median NSEsqrt for the SIMHYD model is higher for the SP approach (0.67) than for the gridded SP approach (0.66).The authors also state that 'the gridded IS approach slightly outperforms the IS approach' (page 10, lines 16-17), but the median NSEsqrt for the SIMHYD model for the IS approach (0.67) is the same as for the gridded IS approach (0.67).

Re: Thanks for picking up some not accurate statements. But we do think that the results do not support the overall conclusions. It is indeed that that the four regionalisation methods show marginal difference in predicting the daily runoff in terms of NSEsqrt. We should clarify what you state is for NSE results. The median NSE for the SIMHYD model for the four methods (SP, gridded SP, IS, and gridded IS) is 0.55, 0.56, 0.54, and 0.58, respectively; the median NSE for the Xinanjiang model for the four methods is 0.54, 0.57, 0.54, and 0.57. This result suggests that in terms of NSE of daily runoff the gridded SP approach is better than the SP approach, and the gridded IS approach slightly outperforms the IS approach. (End of this response)

Other examples can be found on page 15, lines 12-15 and lines 16-18. On lines 12-16 the text reads 'For both the SIMHYD and Xinanjiang models, the IS and gridded IS approaches, respectively, outperform the SP and gridded SP approaches in the two groups with a large regionalisation distance (70 < D <= 100 km and D > 100 km) but are marginally different from the SP and gridded approaches in the other three groups.' Looking at Figure 9, for 70 < D <= 100 km, I cannot conclude that IS (Xinanjiang) outperforms SP (Xinanjiang) or, for D > 100 km, that IS (SMYHD) outperforms SP (SMYHD). Furthermore, on what grounds do the authors assess whether the results of one method outperform another? This is not explained in the text, and significantly weakens the authors' statements about the validity and importance of their results. Statistical tests should be performed, to guarantee that such assertions are statistically significant.

Re: We should clarify that one approach outperforms another based on the NSE difference more than 0.02 and the two approaches perform similarly when the difference is smaller than 0.02 (Zhang and Chiew, 2009). We should state the case of Xinanjiang with 70 < D <= 100 km as follows " these two approaches are similarly for Xinanjiang model with 70 < D <= 100 km. (End of this response)

2) The paper is poorly written, in particular section 2 onwards. Besides problems with grammar, there are many sentences that are challenging to understand and interpret. I will provide two examples, but more can be found in the text: 1) '(: : :) where NSEsqrt is Nash-Sutcliffe Efficiency of the daily square-root-transformed runoff data, which compromises weight when simulating high and low flow (: : :)' (page 8, lines 3-4); 2) 'For both the SIMHYD and Xinanjiang models, the IS and gridded IS approaches, respectively, outperform the SP and gridded SP approaches in the two dry catchment groups (P< 600 mm/yr and 600 _ P < 800 mm/yr), but are marginally different from the SP and gridded approaches in the wet catchment groups with a P > 800 mm/yr.' (page 15, lines 4-7). Some ideas are also expressed in a non-scientific manner, with little precise meaning or detail provided to the reader. As an example, ': : : the median NSE obtained from this study is similar to or marginally different from : : :.' (page 19, lines 20-21). A result cannot be both similar to, and marginally different. The paper does not appear to have been proof read, as the same information is repeated unnecessarily throughout the text. For example, at the beginning of section 2 (page 5, lines 5-6) it reads 'daily meteorological time series : : : from 1975 to 2012'. In the paragraph immediately after (page 5, line 21) it reads 'Data from 1975 to 2012 are used in this study.' Moreover, in section 2, page 5, lines 14-15, it reads 'The 0.05_ _ 0.05_ SILO spatial data were averaged across all of the grid cells within a catchment to produce a catchment average time series for use in this study.' and on page 6, lines 9-10, it reads 'The gridded data in each catchment were then extracted and averaged to obtain an aggregate daily data series for use in the modelling.'. Another curious example of the lack of attention to detail is that, in the acknowledgments, the authors thank two anonymous reviewers and the associate editor for their thoughtful comments and suggestions before the review process has even taken place. Some sections could also be better structured. For example, section 3.6 is comprised of only a single sentence.

Re: Thanks for the reviewer for picking up grammar issues. Sorry for the careless mistakes to confuse this reviewer. Having said that, this manuscript has been proof-read by a professor editorial company – the American Journal Expert. I feel very disappointed that the reviewer still pick up the grammar issue. We will do a thorough grammar correction.

In terms of the acknowledgement, this reviewer's thoughtful thinking will make us to present our manuscript more accurately. We will follow his/her suggestions for rephrase text and manuscript structure.

(End of this response)

3) Some of the choices made in the study are not justified in an adequate way. An example relates to the five properties used to define catchment similarity. Why were these five chosen and not others? Did the authors select these five properties based on statistical tests, literature, etc.? Similarly, the explanation for the use of five donor catchments (page 8, lines 18-22) is difficult to understand and needs to be more clearly explained so that the reader can judge the methods employed. Furthermore, why did the authors chose NSE, NSEsqrt and bias (section 3.5)? Finally, on page 10, line 2: the authors state 'This study used a value of 2 for p (Zhang et al., 2014b).' Why was a value of 2 used?

Re: in terms of choice of the five properties, we have had a section to discuss this. In section 5.5, the text states that "5.5 Practical ways for selecting predictors to build the gridded IS approach. It is necessary to select the IS predictors that are easily available and representative for macro-scale runoff prediction studies. This study chooses five predictors to build the similarity indices. Among them, the aridity index reflects climate wetness or dryness; the fraction of forest ratio reflects the vegetation condition; the mean annual air temperature represents both climate and elevation; and the two rainfall indices represent rainfall seasonality and the standard deviation of daily rainfall. These predictors are relatively easily obtained and representative and are believed to be sufficient for continental Australia or other warm regions. It is possible that the current selected predictors are not enough for the high latitude northern hemisphere or high elevation regions where snow melt is often a major contributor to runoff, and therefore, extra predictors, such as permanent snow cover, snowfall percentage, and days with a mean daily temperature less than 0°C, should be included as well.". We should put more argument for the choice, such as we did correlation analysis first and picked up the five with good correlations and they are representative.

In terms of use of five donor catchments, we should more clearly explain that. We did this based on numerous donor catchment number sensitivity analysis, as indicated by Zhang and Chiew, (2009) and Oudin et al. (2008)

In terms of chose of NSE, NSEsqrt and bias, these metrics are standard ones used for evaluating runoff estimates from rainfall-runoff modelling. The NSE focus on high daily flow evaluation; the NSEsqrt focuses on not only high daily flow but low daily flow as well; the bias is the evaluation of accuracy for mean annual runoff.

In terms of 2 for p, this is the default parameter used in the inverse distance weight approach. Ideally, we can optimise this parameter and do the sensitivity analysis on the weight parameter by varying it within a certain range. However, it is out of scope of this study.

(End of this response)

4) Some of the equations do not seem rigorous. For example, in Eq. (3) the authors use daily values, but in the explanation (page 8, lines 12-14) they mention mean annual runoff. Another example, refers to Eq. (7). The authors say that 'wk is the proportion of the grid cell within the "ungauged" catchment' (page 10, lines 9-10). If that is the case, why does the sum of wk from the L grid cells need to be 1, as the authors state on page 10, line 10? Lastly, the authors refer to Root Mean Square Error in the results section (page 15, line 21), but they have not defined it anywhere.

Given the many limitations in the paper, I cannot recommend publication of the manuscript in its current form and suggest that the paper should therefore be rejected. I hope that the authors will find these comments, while critical, to be useful in revising their manuscript for a future submission. Please note, however, that the list of examples given in this review is not exhaustive.

Re: Eq. (3). Yes, we should mention mean daily runoff.

'wk' issue. This is bad English. When we talk about the proportion, the sum of wk is 100; when we talk about the ratio, the sum of wk is 1.

'rmse'. Yes, we will define rmse before we use it.

In summary, we thank dearly to this reviewer for his/her thoughtful thinking and detailed points which can easily help us to improve our MS during next round of revision. Overall the critical points do not influence the key conclusions drawn from this study. The merit of this study should be more clearly communicated and articulated. We are preparing to do so. (End of this response)

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