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

**Anonymous Referee #1**

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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 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.

In addition to these significant limitations of the paper, I have four other major concerns with this paper:



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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). 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 ($70 < D \leq 100$ 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 $70 < D \leq 100$ 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.

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, respec-

tively, outperform the SP and gridded SP approaches in the two dry catchment groups (P< 600 mm/yr and 600 $\leq$ 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° $\times$ 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.

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?





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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.

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