We would like to thank the reviewer for his/her time and effort in reviewing our manuscript, titled 'An improved Grassberger-Procaccia algorithm for analysis of climate system complexity' (ID: hess-2017-445). Your comments and suggestions are much appreciated, which are useful to further improve our manuscript. Please see our responses in the following section.

Comment 1. For readers to quickly catch your contribution, it would be better to highlight major difficulties and challenges, and your original achievements to overcome them, in a clearer way in abstract and introduction.
Response: Thank you for the comment here. In Introduction, we have stated some of the major problems associated with the current methods for computing correlation dimensions (e.g., in lines 11-12 'the use of this method is still not adaptive and relies heavily on subjective criteria', and in lines 53-54, 'However, the G-P method relies on visual inspections for choosing scaling regions, which is subject to human errors (Sprott and Rowlands, 2001). To deal with this specific problem, we tried to find 'more objective and adaptive algorithms for identifying scaling regions to obtain more accurate estimates of correlation dimensions' (lines 59-60). Nonetheless, based on the reviewer's comment here, we will further highlight our contribution for computing correlation dimensions in Abstract and Introduction.

Comment 2. It is shown in the reference list that the authors have several publications in this field. This raises some concerns regarding the potential overlap with their previous works. The authors should explicitly state the novel contribution of this work, the similarities and the differences of this work with their previous publications.
Response: Thank you for the comment. First, the novelty of our current work as compared to previous studies is discussed in details in Abstract and Methodology. Secondly, the studies cited in the reference list (presumably with the last name of Wang) are done by others and not published by the current authors.

Comment 3. It is mentioned in p.1 that an improved Grassberger-Procaccia algorithm is adopted for analysis of climate system complexity. What are the other feasible alternatives? What are the advantages of adopting this particular algorithm over others in this case? How will this affect the results? More details should be furnished.
Response: Thank you for the suggestion. In fact, we have given some alternative methods for studying climate system complexity, such as chaos theory, wavelet analysis, and dynamical analysis (line 35). In particular, for computing correlation dimensions, we also compared our newly proposed algorithm to two other commonly used algorithms, namely the intuitive judgment and the point-based K-means clustering methods, based on two classical chaotic systems. Nevertheless, based on the reviewer's comment here, we will add few sentences to illustrate the differences among existing methods.

Moreover, to address this comment as well as following comments made by the reviewer, we feel that in a single technical paper with limited space, it is not feasible and appropriate to include every aspect existing in the field of complexity analysis, which would deviate from the central theme of this study and make the manuscript unnecessarily excessive. In fact, there are several excellent books that are devoted to entirely discussing relevant problems, which we would like to refer the reviewer to (e.g., Bellie Sivakumar, 2017; Jayawardena, 2014). In addition, we will add those books to the reference list for the convenience of readers.

Sivakumar B. Chaos in Hydrology. Springer Netherlands, 2017.
Jayawardena A. W. Environmental and Hydrological Systems Modelling. Taylor and Francis Group, CRC Press, 2014.

Comment 4. It is mentioned in p.2 that Lorenz and Henon chaotic systems are adopted to test the effectiveness of the proposed algorithm for estimating correlation dimensions. What are the other feasible alternatives? What are the advantages of adopting these particular systems over others in this case? How will this affect the results? More details should be furnished.

Response: Thank you for this comment. Indeed, there are other chaotic systems (e.g., the Chen system (Chen and Ueta, 1999), and the Rössler system (Rössler, 1979)). Among those chaotic systems, the Lorenz and Henon systems have been mostly used to analyze the chaotic behavior in climate systems and to test the effectiveness of algorithms for computing climate system complexity (e.g., Grassberger and Procaccia, 1983; Lai and Lerner, 1998; Ji et al., 2011), moreover, the correlation dimensions of those two systems are mostly studied. In our opinion, for the purpose of brevity and comparison among different studies and methods for computing climate system complexity, it is justified that standard systems, such as the Lorenz and Henon systems, should be adopted. Finally, the discussion on different chaotic systems is beyond the scope of this study. It would be unrealistic for us to compare all chaotic systems in a paper. Certainly, we can add more details and the following references in our revised manuscript.

References:

Chen, G., Ueta, T.: Yet another chaotic attractor, International Journal of Bifurcation and Chaos, 9, 1465-1466, 1999.

Rössler, O. E.: An equation for hyperchaos. Physics Letters, 71A (2, 3): 155-157, 1979.

Lai Y. C., Lerner D.: Effective scaling regime for computing the correlation dimension from chaotic time series. Physica D, 115: 1-18, 1998.

Ji, C.C., Zhu, H. and Jiang, W.: A novel method to identify the scaling region for chaotic time series correlation dimension calculation, Chinese Sci. Bull., 56, 925-932, doi: 10.1007/s11434-010-4180-6, 2011.

Comment 5. It is mentioned in p.2 that the Haihe River Basin is adopted as the case study. What are other feasible alternatives? What are the advantages of adopting this particular case study over others in this case? How will this affect the results? The authors should provide more details on this.

Response: Thank you for the comment here. The reasons that we took the Haihe River

Basin (HRB) as a case study are as follows: (1) The HRB has been facing serious water shortages due to climate change and increasing water demand. Although previous studies have investigated the climate variability (e.g., rainfall, air temperature, and evaporation) in the HRB from different perspectives, to our best knowledge, there are still no attempts made to quantify nonlinear characteristics of climatic variables, especially regarding their chaotic behaviors in the HRB, which is essential for understanding the nonlinearity of the climate system in the region; and (2) The HRB is a diverse hydroclimatic region with many sub-watersheds of varying geographical and climatic conditions, which make the region ideal for understanding the climate system complexity. Certainly, we can add more details about the advantages of adopting this particular case in our revised manuscript.

Comment 6. It is mentioned in p.3 that the normal-based K-means clustering technique is adopted to partition all normals of the scatter points into K clusters with high similarity. What are other feasible alternatives? What are the advantages of adopting this particular technique over others in this case? How will this affect the results? The authors should provide more details on this.

Response: Thank you for the suggestion. The K-means clustering method has been widely used for cluster analysis, which aims to partition n observations into K clusters. For each cluster, each observation belongs to the cluster with the nearest mean. In this paper, in order to find a precise scaling region, we used the K-means clustering algorithm to remove the points, which were obviously located outside the real scaling region. Different from previous K-means methods (e.g., the point-based K-means clustering method), we measured the similarity of points using the diversity between points' normal, that is, the normal-based K-means clustering technique. This is because the normal directions of different points in Figure 4(a) are greatly different. By comparison, the distance between points is much less, due to the use of the logarithmic scale that makes the points more densely distributed as $\ln r$ goes backward (see Fig 3(a)). Therefore, we proposed to use the normal-based K-means clustering algorithm. As a comparison, taking the classical chaotic models of Lorenz and Henon as two examples, the results obtained by our proposed normal-based k-means method performed better than the point-based k-means method (see Table 1). To illustrate this, we will add some sentences to show the advantages of normal-based K-means method in the methodology section.

Comment 7. It is mentioned in p.4 that the Random Sample Consensus algorithm is adopted to fit a straight line through the log-transformed points. What are other feasible alternatives? What are the advantages of adopting this particular technique over others in this case? How will this affect the results? The authors should provide more details on this.

Response: Thank you for the comment. We have given the reasons for choosing the Random Sample Consensus algorithm (RANSAC) in section 2.2. As shown in section 2.2, the RANSAC algorithm outperformed the commonly used least squares method for linear fitting, based on a hypothetical example (Fig. 1).

Comment 8. It is mentioned in p.6 that the intuitive judgment method and the point-based K-means clustering method are adopted to compare the results obtained by the proposed method. What are the other feasible alternatives? What are the advantages of adopting these particular methods over others in this case? How will this affect the results? More details should be furnished.

Response: Thank you for the comment. The intuitive judgment method and the point-based K-means clustering method are two commonly used methods for identifying scaling region (e.g., Sprott and Rowlands, 2001; Ji et al., 2011). Although more comparisons can be done, additional comparisons may seem redundant. In addition, it is unrealistic to list all the comparisons in one paper.

Comment 9. It is mentioned in p.6 that the normal-based K-means clustering technique is adopted to determine the scaling regions of the curves in Fig. 3a. What are other feasible alternatives? What are the advantages of adopting this particular technique over others in this case? How will this affect the results? The authors should provide more details on this.

Response: This comment is the same as the comment 8.

Comment 10-11. 10. Some key parameters are not mentioned. The rationale on the choice of the particular set of parameters should be explained with more details. Have the authors experimented with other sets of values? What are the sensitivities of these parameters on the results? 11. Some assumptions are stated in various sections. Justifications should be provided on these assumptions. Evaluation on how they will affect the results should be made.

Response: Thank you for this comment. We rechecked the paper and found that the ranges of r were missing. We will add more details in line 92 (i.e. 'Set r_{min} and $r_{\max}$ as the minimum and maximum distances between points, respectively (Ji et al, 2011; Lai and Lerner, 1998). If $r \leqslant r_{min}$, none of the vector points will fall within the volume element and $C(r, m)=0$. Otherwise, if $r \geqslant r_{\max}$, all vector points will fall within the volume element and $C(r, m)=1$.'). Other parameters have been given in the paper. We must point out that some of the parameters in this study were determined by routinely used methods. For example, the time delay (see line 86) was determined by the autocorrelation function. Some other parameters (for example, T=5°) were determined by testing the data. In terms of the assumption about the value r, we will add it in our paper.

Comment 12. The discussion section in the present form is relatively weak and should be strengthened with more details and justifications.

Response: Considering that this is a technical paper, we limited our discussions for the purpose of brevity. However, if needed, we can give more details and justifications in our revised paper.

Comment 13. The manuscript could be substantially improved by relying and citing more on recent literatures about real-life case studies of contemporary soft computing techniques in hydrological engineering such as the followings: ïA¸nˇ Gholami, V., et al., “Modeling of groundwater level fluctuations using dendrochronology in alluvial aquifers”, Journal of Hydrology 529 (3): 1060-1069 2015. ïA¸nˇ Taormina, R., et al., ““Neural network river forecasting through baseflow separation and binary-coded swarm optimization”, Journal of Hydrology 529 (3): 1788-1797 2015. ïA¸nˇ Wu, C.L., et al., “Prediction of rainfall time series using modular artificial neural networks coupled with data-preprocessing techniques”, Journal of Hydrology 389 (1-2): 146-167 2010. ïA¸nˇ Wang, W.C., et al., “Improving forecasting accuracy of annual runoff time series using ARIMA based on EEMD decomposition,” Water Resources Management 29 (8): 2655-2675 2015. ïA¸nˇ Chen, X.Y., et al., “A comparative study of population-based optimization algorithms for downstream river flow forecasting by a hybrid neural network model,” Engineering Applications of Artificial Intelligence 46 (A): 258-268 2015. ïA¸nˇ Chau, K.W., et al., “A Hybrid Model Coupled with Singular Spectrum Analysis for Daily Rainfall Prediction,” Journal of Hydroinformatics 12 (4): 458-473 2010.

Response: Thank you for providing the relevant references for further modification of our paper, and we have read them and we will also cite some of them in the revised paper.

Comment 14. In the conclusion section, the limitations of this study, suggested improvements of this work and future directions should be highlighted.

Response: Thank you for the comment. We will discuss the limitations of this study and suggest possible future improvements for computing correlation dimensions.