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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Similarity and dissimilarity in model-results between single and multiple flow direction simulations based on a distributed ecohydrological model" *by* Zhenwu Xu and Guoping Tang

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Dear Referee #2: We greatly appreciate your valuable comments on our manuscript (hess-2018-47). We have carefully addressed all of your comments and our responses are listed below one by one following each of your comments!

General Comments:

(1)The methods and conclusions of this paper would be much clearer if the authors would explicitly define their terminology pertaining to "flow direction", as it relates to

SFD/MFD or SD/MD, in terms of not only possible flow directions but also in terms of the number of possible flow paths. Some of the studies cited within this paper have used "MFD" to describe a single flow path routed between 2 downslope cells, whereas other papers (including this one) use "MFD" and "MD" to describe multiple possible flow paths. Please clarify this terminology early in the paper. I suggest use SD for the single flow direction approach throughout, not SFD. Similarly use MD for multiple flow direction, and not MFD. For specific variations on MD, such as D-Infinity (Tarboton 1997) of MDInfinity (Seibert and McGlynn 2007) just use these terms and mention that they are specific cases of MD.

Response:

Thanks for your good comments! We decide to define the terminology pertaining to "flow direction" in terms of the maximum number of possible flow paths to adjacent downslope cells. Accordingly, we used "SD" instead of "SFD" and "MD" instead of "MFD" to describe these flow routing algorithms. Thus, D-Infinity is defined as a special case of MD algorithms since it allows two flow paths to downslope cells. We have added related definitions in the "Introduction" section of the revised manuscript. These terminologies are kept consistent throughout our manuscript.

In addition, we added one more flow routing algorithm, i.e., MFD-md, to the revised manuscript. As a result, a total of five algorithms (i.e., D8, D-Infinity, RMD-Infinity, MD8, MFD-md) were used in the revised manuscript and results among the five routing algorithms are compared, respectively. For example, we treated D-Infinity as a specific case of MD algorithms. We then compared the ecohydrological variables SSD and LAI between each pair of five algorithms at cell level as we did before in the Section 3.4 and 3.5 for the year 1992 (a relatively dry year) and 2005 (a relatively wet year) as well as for the whole study period 1991-2012 (see Table S4 that will be added to the revised paper), respectively. Specially, four pairs of algorithms (i.e., D8/RMD_inf, D_inf/RMD_inf, D8/MD8, RMD_inf/MFD-md) are selected for comparative analysis in section 3.4 and 3.5 (Fig. S2 on the supplement of this reply). Correspondingly, Fig. 7

and Fig. 8 show comparisons of four pairs of algorithms (Fig. S3). For Fig. 9, we added the relationship between leaf area index (LAI) and “distance to stream (a, b)” as well as relationship between SSD and LAI (c) under five algorithms to offer more details. We believe that these changes in the selection of algorithms and data comparison will help readers to understand our results.

(2)Although this paper presents several descriptive statistics of SSD and LAI, including mean, range, min, max, and standard deviation, it would be more informative to add a figure showing the actual distribution of pixel-level SSD and LAI values as a histogram or density function. For example, probability density functions for SSD and LAI, for each flow routing algorithm and for differences between SD8 and MD8, would provide evidence of similarities or differences that may not be fully expressed by statistics. Boxplots would also be useful for showing the full distribution of SSD and LAI.

Response:

Thanks for your good comments! An additional figure (Figure S1) including both boxplot and density plot are added in supplement and will be eventually added/cited in Section 3.2 in the revised manuscript.

(3)The conclusions state that ecohydrological variables are more autocorrelated under the MFD model, but do not say whether this is good, or why this is important. The paper does not establish why, or for what purpose, the degree of autocorrelation is a quantity of interest or how it relates to model performance. Are these autocorrelation quantities measures of how well the model performs? The contribution of the paper may be stronger if the authors are able to address this concern.

Response:

Thanks! Theoretically, the Moran’s I quantifies the degree of similarities of nearby locations in space. It measures the spatial autocorrelation of a variable and expresses the homogeneity of a variable in its neighborhood extent. In this study, we refer the

major differences between Moran’s I (similarity between nearby cells) to their differentiated degrees in “dispersion” (extent of flow divergence) of model results. We used this index to not only provide statistical evidence and but also quantify the extent of “dispersion” that can not be measured by standard deviation (Fig. S2, difference < 2.8 % in SSD). Overall, according to the values of Moran’s I (Fig. S3, difference ranges from 3% to 26% for SSD), we think differences between model results in terms of “dispersion” do exist for some distinct algorithms (e.g. D8 vs. MD8) but the extent of dispersion is not as significant as some previous studies suggested partially because they often illustrate cases in some drainage zones by theoretical DEMs (e.g. divergent hillslopes). We have added related information in the revised manuscript.

(4)Previous papers (Tarboton 1997; Seibert and McGlynn 2007) described and demonstrated examples where over-dispersion of flow among multiple flow paths is unrealistic and thus undesirable. Please comment on how or why this is not a concern is your results, especially given that your results suggest that the spatial autocorrelation of ecohydrological variables is greater under MFD than under SFD due to flow dispersion.

Response:

Thanks! This is a very challenging question. From my point of view, it’s hard to judge whether “dispersion” is good or not without field data and whether some routing algorithms with maximum number of flow paths (e.g. possibly 8 paths in MD8, MFD-md) can results in an “over-dispersion” issue. It might be seen as a theoretical advantages for D-Infinity over MD-Infinity. However, previous studies that were based field observations (e.g., KopeckÃ¡ and Čížková , 2010; Tang et al., 2014; Radula et al., 2018) indicated that MD algorithms with greater possible flow paths (e.g., MD8, MFD-md, TFM) tended to provide better results. We have discussed this in the revised manuscript.

(5)This topic would fit nicely in section 4.2 of your Discussion. Some of the analytical methods used in this paper were not described in sufficient detail to evaluate your choice of methods. Specifically, please address or clarify: 1)exactly how CHESS differs from RHESSys; 2) the extent to which CHESS was calibrated individually with each routing algorithm; 3) differences between the MD-infinity and RMD-infinity algorithms; 4) categories used to classify distance-from-stream, i.e., to convert continuous numeric to categorical variables; 5) method for delineation of patches; 6) identification of "wet" and "dry" years; and 7) the specific tests used to assess statistical significance of differences in Nash-Sutcliffe efficiencies. Items #4 and #5 are particularly critical for evaluating your results because wider numerical categories will include a greater range of values in the same category, and thus have a higher kappa, relative to smaller and thus more precise categories. See below (specific comments) for additional feedback and suggestions pertaining to these specific items.

Response:

#1 A detailed description about how CHESS differ from RHESSys is given in reply on Specific Comments #3.

#2 Additional soil text parameterizations for model's calibration are given in supplement of this response and will be finally put in the revised manuscript (Table S1).

#3 RMD-infinity allows all triangular facets with possible flow paths to participate in flow partitioning i.e. all "routing of flow" while MD-infinity considers all "routing of area" which allows one facet of each area participate in flow partitioning on convex hillslopes. Specifically, in cell's numbers of flow paths (Fig. S5), MD-infinity is more similar to D-infinity and RMD-infinity is more similar to MD8 but they mainly differ in the portion of flow in each flow path. When comparing D-infinity with RMD-infinity, the results from RMD-Inifinty are more similar to those of MD8 and MFD-md than are MD-infinity while both are based on triangular facets in their calculations (Fig. S5, Table S4).

#4 the "Distance-to-stream" is calculated as an integer number for every cell and each integer number stands for a specific type. Since the distances for most cells to their nearest stream-type cells are not at the cardinal direction (E, W, N, S), the distance will be calculated as a specific integer but not numbers like 100, 500. So there are

more and more types as distance rises (e.g., 100, 141($100\sqrt{2}$), 200, 223($100\sqrt{5}$),...) as in Fig.4. We didn't turn it into categorical numbers like 100, 200, 300 because fewer points is usually provided with higher R2 . We want to remain more details in calculating D_R while most distances with specific integer number have more than 10 cells.

#5 Because the CHESS model used in this study is process-based distributed model, the patches (or cells) are delineated by soil, vegetation and topographic features including slopes, aspects and elevations.

#6 The relatively wet and dry year are determined by the amount of annual total rainfall in a water-year (will furtherly given in our new supplement, Table S1). We have added related information in the revised manuscript.

#7 We accepted your advice and use "small" rather than "no significant" difference in NS.

Specific Comments:

(1)Page 1, lines 8-22: It would be helpful for the abstract to name the distributed hydrologic model (CHESS) and ecohydrological variables used in the comparisons (SSD and LAI).

Response:

Thanks for your good comments. We have added these information in the abstract section.

(2)Page 2, lines 7-8: Specifically, flow follows the direction of steepest downwards topographic slope (which is more specific than ": : :follows the topographic relief").

Response:

We accepted your good suggestion and have revised it.

(3)Page 3, lines 4-10: Briefly clarify how CHESS is different from RHESSys. The statement that "specific algorithms for carbon, water, and nutrient dynamics: : : are mostly maintained as in Tague and Band (2004)" is confusing and requires further explanation. What is "mostly"? Tague and Band (2004) indicate that RHESSys relies on either TOPMODEL or DHSVM for routing. How is CHESS different, and which (if any) aspects of RHESSys's routing algorithms are retained in your simulations?

Response:

Thanks for your good comments! Actually, we renamed "R-RHESSys (note: instead of RHESSys)" to CHESS, which is short for "Coupled Hydrology and Ecology Simulation Systems". Tang et al. (2014, 2016) developed R-RHESSys based on RHESSys modelãĂĆAs discussed in Tang et al. (2014), we have removed the hierarchical structure of the original RHESSys model and also excluded the top-model embedded in the original RHESSys. In addition, we have redesigned the model-user interface for R-RHESSys and modified model codes much. We renamed "R-RHESSys" to CHESS for the purpose of its future development and usage. We have revised relevant text in the revised manuscript for clarification The explicit and implicit routing approaches used in this study represent two basic approaches to model lateral soil moisture flux (Tague and Band, 2001). Spatial variability in soil moisture also can be addressed implicitly using statistical distribution methods such as TOPMODEL (Beven and Kirkby, 1979), which distribute saturation deficit across a non-spatial distribution of hydrological parameters within a catchment. Explicit routing approaches have been applied in models such as TOPOG (O'Loughlin, 1990), VSAS (Bernier, 1985), CLAWS (Duan, 1996) and DHSVM (Wigmosta et al., 1994), which explicitly transfer water between connected cells. R-RHESSys (Tang et al., 2014), excluded the TOPMODEL (Beven and Kirkby, 1979) embedded in the model's predecessor and retained the explicit water-routing algorithm. Thus, the major differences between our studies and Wolock and McCabe (1995) used TOPMODEL with implicit routing are that the former performed in a fully distributed mode with explicit routing. Besides, although many studies studied al-

gorithm's performance via estimation of theoretical results such as TWI, we are not certain whether flow routing algorithms can affect model's performance. Comparison between the two routing approaches has been well discussed and illustrates the advantage of the explicit routing approach, though the loss of computational efficiency associated with the explicit routing approach is noted (Tague and Band, 2001). So, we focus on comparisons between model results via explicit flow routing algorithms used in CHESS, which provides not only evidences on the effects of routing algorithms on model's performance but also insights into future efforts to improve model-based research in studying ecohydrological processes in the land surface. Briefly, compared to results of Wolock and McCabe (1995) using TOPMODEL, we found that an ecohydrological model with differing explicit routing algorithms, can be easily calibrated and parameterized to be able to simulate stream hydrograph with similar pattern. We will enroll these discussions in our revised manuscript.

(4)Page 3, line 29: Beginning with this paragraph, for clarity please explicitly state which routing algorithm is being discussed in each paragraph.

(5)Page 4: Given that the methods used (D8, D-infinity, MD8 and MD-infinity) have all been described in detail in the publications cited in this paper, the equations and methods do not need to be presented in as much detail as they are presented here. One exception is MD-infinity, which should be clearly described in terms of its difference from RMD-infinity.

Response to (4) and (5):

We will revised relevant parts for clarification. We retained descriptions of RMD-Infinity and MD-infinity. Results from other algorithms will be summarized with concise words.

(6)Page 4, line 16: Where the citation is provided for MD-infinity, it should also be provided for D-infinity.

Response:

We provided a citation for D-Infinity.

(7)Page 4, line 19: Briefly summarize what you mean by ": : :the advantages of D-infinity and MD8".

(8)Page 4, lines 16-24: The reason for the adoption of a new method (RMD-infinity) is not clear. How does this improve upon MD-infinity, and how can you quantify this? It seems that dividing flow among all triangular facets reintroduces the problem of unrealistic dispersion on convergent slopes, as described in Tarboton (1997) and Seibert and McGlynn (2007). RMD-Infinity is a new terrain flow routing approach. It does not do justice to it as a potential contribution to flow routing methodology to introduce it without presenting a more detailed evaluation and conclusion as to its efficacy.

Response to (7) and (8):

There are two theoretical advantages for D-Infinity: (i) First, D-infinity can more accurately describe water routing than does D8 by assuming infinite possible flow routing in a triangular facet based on DEMs; (ii) Second, it limits the maximum number of flow paths to 2 downslope cells to avoid "dispersion". However, more and more field studies have shown a favor of MD algorithms such as MD8. And since differences between D-Infinity and MD-Infinity is minimal in our studies (6.7% in cells' numbers of flow paths) though Cleve Creek is a mountainous watershed with great topographic relief. To discuss the extent of differences between D-Infinity and MD-Infinity (resolution of DEM) is somewhat beyond the scope of our current studies. Nevertheless, we will explore such issue in the future study.

Besides, we tested if RMD-Infinity is different from other MD algorithms (MD8, MFD-md) in model's results as we have done throughout the studies (Fig.S2 & Table. S4). The results indicate that RMD-infinity differs to some degree from other algorithms. This might offer additional insights into future ecohydrological modeling studies.

(9)Page 5, line 6: Explain how the land cover data "are pre-specified". What is the

source?

Response:

The land cover data used in this study were provided by the courtesy of Dr. Lutz when Dr. Tang conduct the US DOE research project, for which Dr. Lutz is the PI. We have added related information in the proper place.

(10)Page 5, lines 15-18: This section states that calibration was done for each of the four routing algorithms, while the following statement indicates that model parameterizations were almost identical among the four simulations. These statements appear contradictory. Please clarify how the calibration methods accounted for any streamflow differences among the four simulations. Also consider that Wolock and McCabe (1995) found that separate calibration for models using alternative routing methods affected accuracy of simulated streamflow, and discuss how your findings compare with their findings in your Discussion.

Response:

Actually, we only slightly modify the values of soil hydraulic conductivity decay parameter through which we can have model to simulate well the observed streamflow under all different algorithms. The differentiated parameters for calibration hydraulic conductivity with depth (m) is given in Table S1 along with other undifferentiated parameters of soil texture. Thanks for offering another point to explain our results and we will revise the discussion Section 4.1 as well as referring to studies of Wolock and McCabe (1995) .

(11)Page 5, lines 24-25: Means and standard deviations are only two metrics that can describe a distribution, and they are often inadequate at detecting important differences among multiple distributions. Please also consider showing the entire distribution in the form of a probability density function, histogram or boxplot.

Response:

Thanks, we have used other plots such as boxplot and density plots in the revised manuscript. Also, see our response to the General Comment (3).

(12)Page 6, lines 12-14: Neither citations for these metrics nor the methods used to delineate patches are presented here. Please specify the numerical categories that were used to classify distance-from-stream. Also describe how patches were delineated, as well as their number and range of sizes.

Response:

Thanks, please see our response to the General Comment (4)#4.

(13)Page 7, line 2: This is the first mention (other than in the Abstract) of "wet year" or "dry year". In Methods, describe how "wet" and "dry" years were identified, with at least minimal data to support the identification of these years.

Response:

It is determined by the amount of annual total rainfall. We will put related information in the revised manuscript. Table 1.).

(14)Page 7, line 28: Was an actual significance test applied to the NS values? If yes, what test (describe in Methods)? If no, this sentence should describe differences as small rather than "no significant difference".

Response:

We accept your advice and use "small" rather than "no significant" to describe the differences in NS.

(15)Page 8, line 15: What were the pre-specified ranges of values? These should be stated in Methods.

Response:

As in our response to the General Comments(4)#5, the six ranges are the same as in

Fig. 3 and Fig.5 to calculated the Kappa statistics (For SSD and LAI).

(16)Page 10, lines 6-11: Radula et al. (2018) also compared differences in simulated soil moisture among several flow routing algorithms. They evaluated regressions between soil moisture and topographic wetness index, and also between ecological indicators of soil moisture and wetness index, where wetness index was estimated under different flow routing algorithms. I suggest comparing your findings to theirs in the Discussion.

Response:

In our model, the baseflow generated form a cell is determined by the transmissivity curve and via explicit routing algorithm. Thus, we considered to discuss the differences in model results between TWI and explicit routing algorithms in relevant discussion section in our revised manuscript.

(17)Figure 1: Please specify the source of the land cover information shown in the map.

Response:

See our response to the Specific Comment (9) above.

(18)Figure 4: As described in the Methods, this analysis uses means within patches. This detail should be specified in the caption; otherwise it implies that distance-from-channel for individual pixels were used.

Response:

Thanks and we have added these information in our new figure caption (Fig. S2).

(19)Figure 7: This figure does not appear to present any new information beyond what Seibert and McGlynn (2007) showed. I suggest eliminating this figure (or alternatively, clarifying how it expands on previous work).

Response:

Thanks and we removed it and now kept it in Fig. S2 of the revised supplement.

Again, we greatly appreciate your valuable comments that have helped greatly improve our manuscript.

Yours Sincerely,

Zhenwu Xu

2018/5/3

Major References

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Wolock, D. M., and McCabe, G. J.: Comparison of single and multiple flow direction algorithms for computing topographic parameters in TOPMODEL, Water Resources Research, 31, 1315–1324, 10.1029/95WR00471, 1995.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2018-47/hess-2018-47-AC2-supplement.pdf

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