Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-47-RC2, 2018

Hydrology and Earth System Sciences Discussions

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Interactive comment

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

Anonymous Referee #2

Received and published: 4 April 2018

SUMMARY:

This study compared single flow direction (SFD) vs. multiple flow direction (MFD) approaches in the derivation of DEM inputs for the distributed hydrologic model CHESS (coupled hydrology and ecology simulation system). The analysis was based on pixel-level and patch-level comparisons and spatial autocorrelation of ecohydrological variables, soil saturation deficit (SSD) and leaf area index (LAI) produced by the model with inputs from different flow routing schemes. Simulations covered a 12-year period, including one wet year and one dry year, in the semi-arid Cleve Creek water-

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shed, Nevada, USA. To examine spatial autocorrelation of simulated variables, the study used Moran's I to relate mean SSD and LAI at the patch scale to Tesfa's (2011) distance-to-stream metric. SFD vs. MFD simulations produced similar streamflow values and similar watershed-scale mean values of SSD and LAI. SFD vs. MFD simulations produced different cell-level values of SSD and LAI, where differences were greatest in areas farthest from channels. In contrast, hydrologic variables were most different near channels. Spatial autocorrelation of SSD and LAI based on MFD was greater than that based on SFD, likely due to a higher degree of flow dispersion under MFD.

GENERAL COMMENTS:

The contribution of this paper lies in evaluation of model behavior rather than understanding of physical processes. In its current form, the value of the contribution of this paper is hard to discern, beyond the finding that differences between hydrograph simulations using the different routing methods evaluated were very small. The main differences reported among routing algorithms were in terms of autocorrelation of the ecohydrological variables (SSD and LAI) evaluated across grid cells. 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.

The paper is also unclear on how these findings might apply to future distributed hydrologic modeling research. Is it likely that the model behavior observed in this study will apply to other ecohydrologic models and other geographic regions? I hope that the authors can address this comment in the Discussion to increase the value of the paper's findings.

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 MD-Infinity (Seibert and McGlynn 2007) just use these terms and mention that they are specific cases of MD.

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.

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 eco-hydrological variables is greater under MFD than under SFD due to flow dispersion. 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.

SPECIFIC COMMENTS:

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).

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

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?

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

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.

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

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

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.

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

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.

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.

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.

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.

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”.

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

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.

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

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.

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).

CITATIONS:

Raduła, M. W., Szymura, T. H., & Szymura, M. (2018). Topographic wetness index explains soil moisture better than bioindication with Ellenberg's indicator values. Eco-

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logical Indicators, 85, 172-179.

Seibert, J., & McGlynn, B. L. (2007). A new triangular multiple flow direction algorithm for computing upslope areas from gridded digital elevation models. Water resources research, 43(4).

Tarboton, D. G. (1997). A new method for the determination of flow directions and upslope areas in grid digital elevation models. Water resources research, 33(2), 309-319.

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