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Interactive comment on "The role and value of distributed precipitation data for hydrological models" *by* Ralf Loritz et al.

Daniel Wright (Referee)

danielb.wright@wisc.edu

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The authors present a framework for dynamically adapting the level of spatial detail resolved within a physics-based rainfall-runoff model depending on the spatial variability in precipitation. I found this the be one of the most interesting manuscripts that I've ever reviewed, and commend the authors on this innovative work. Nonetheless, there are some issues that should be addressed before the manuscript is suitable for publication in HESS, and that could help maximize the impact of the work.

Regards, Daniel Wright, University of Wisconsin-Madison

Major comments:

1.I believe the discussion could be strengthened by deeper consideration of how this

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Discussion paper

approach would "scale up" to larger watersheds or regions. Part of my reason for encouraging this is that the land surface modeling (LSM) community is at least as concerned as the rainfall-runoff community about model computational demands of long-term/ensemble simulations, and are seeking ways of representing fine-scale (e.g. hillslope and below) over continental-to-global domains. In fact, land surface modeling was the focus of the well-known Wood et al. (2011) hyperresolution modeling opinion piece. In addition, there has been relevant progress in LSM development that the authors should cite. I will mention these below. But in terms of scaling up, the key aspects seem to be acknowledgement that heterogeneity of model parameters will increase with modeled area, while the rainfall spatial coverage will, on average, decrease.

2. I believe the discussion could also be strengthened by some discussion of how well this approach might fit with specific types of spatial discretizations. It fits quite naturally with hillslope-based models. The fit is less clear with gridded or TIN-based models-or at least with high-resolution gridded models in which individual model grids must "communicate" with each other to transmit water via overland or subsurface flow to channels. It seems that the computational advantages of the approach might be limited in that case. In addition, models such as GSSHA in which overbank river flow can return to the land surface would have some limits here too. These issues are worth discussing because such models constitute important current directions in physics-based model development.

3. While there may be other relevant LSM developments, the one that I am aware of is Hydroblocks (Chaney et al. 2016). While I recommend reading that paper, the basic approach is similar to this manuscript's in that spatial units are grouped into hydrologically similar clusters to reduce the computational demand. The difference is that in Hydroblocks, these clusters are not dynamically reassigned according to time-varying characteristics (unless the developers have recently added that capability). So in fact, your approach appears to be superior in some respects. Specifically, within Hydroblocks, since there is no dynamic reassignment, you can never have a cluster

that extends beyond the spatial extent of a single precipitation grid cell, which means that their approach loses computational efficiency with higher-resolution precipitation datasets. Your approach thus seems to hold more promise in terms of flexibility to advances in precipitation inputs.

4. More clear description of what each model does and does not do is needed in Section 3. Specifically, I found it confusing the way that the models are briefly introduced at the beginning of the section, and then discussed further in various subsections. I also find it strange that you have text that is not assigned to specific subsections. It isn't clear why section 3.2.1 is needed. . . convention is that you don't include subsections unless you have at least 2 or 3 (i.e. 3.2.2, 3.2.3). This section structuring needs rethinking. More important, I really couldn't figure out from the descriptions what the differences between some models were. I also don't understand the motivation for using a different rainfall dataset for the reference model and model a; this seems unnecessary. I think one think that would really help is to not use "model a", "model b", etc. but some brief descriptive names that actually help the reader understand and recall the differences. Also, a table that compares the key features and differences of all the models could be effective.

5. Zhu et al. (2018) and Peleg et al. (2017) both highlight how distributed rainfall structure is really important in determining flood frequency across a range of scales. Though I normally refrain from suggesting that authors cite my own work, in this case it seems appropriate to highlight these studies, since they do show that for extreme events, rainfall space-time structure is extremely important in determining hydrologic response even at very small scales (see Peleg et al. in particular), and that this importance varies with rainfall magnitude and basin size. Along with this, I disagree with the statement on pg. 27: "it seems that catchment size might not be the best indicator to decide if" a distributed model is needed. It probably is the best single indicator, but is still insufficient. I draw a somewhat different conclusion from your work: that a distributed approach is always needed to reap the full benefit of spatially distributed

rainfall (at least in locations in which convective rainfall can occur), and that provides motivation for continued developments such as this into ways of handling this need in computationally-efficient ways. Likewise, I disagree with the statement on pg. 30 line 18-19: compressing rainfall into a single time series isn't so important as the ability to only use as much computational power as is truly needed to solve the problem at hand.

6. Some discussion of implications for calibration would be interesting. Is it necessary to calibrate using a fully distributed model? This would limit the usefulness of this approach in some respects such as automated calibration procedures.

7. There are a number of minor grammatical issues that nonetheless cause some distraction from the overall high quality of the manuscript. I will point out some of these below, but it could be worthwhile to have a native English speaker perform a careful proofreading.

Minor comments:

1. It may be worth defining more carefully what you and others mean by hyperresolution. There seem to be important differences-Wood et al. (2011) mention 1 sq. km or smaller, while you seem to refer to much finer scales than that. This isn't trivial since a 1 sq km gridded Richard's equation based model can, in my experience, run quickly (of course, whether such models make physical sense is another issue. . .).

2. Figure 2a: what is the small conceptual diagram to the lower left? No explanation is given. Consider deleting.

3. It may be worth more carefully explaining in the introduction what is meant by "dissipate spatial gradients efficiently"

4. Pg. 16 line 30-32: change to ". . . maximum number of precipitation grid cells (42 in this study)."

5. Pg. 1 line 9: delete "at different periods"

6. Pg. 1 line 10: change “yields. . . performances” to “only improved performance”

7. Pg. 1 line 12: delete “the”

8. Pg. 1 line 17: change “They furthermore” to “We also”

9. Pg. 2 line 7: Usage of “constrained” seems strange here. Consider rewording this sentence.

10. Pg. 2 line 18-19: change to “they analyzed the dependence of average runoff rates on the spatial and temporal variability. . .”

11. Pg. 3 line 3: change “this study” to “that study” unless you mean the specific manuscript that I am reviewing right now.

12. Pg. 3 line 12: “higher” than what? Consider changing to “. . . are only important during. . .”

13. Pg. 5 line 2: delete “large”

14. Pg. 7 line 2: delete “an”

15. Pg. 8 line 3: change “pol” to “polarimetric” and “as well as” to “and”

16. Pg. 8 line 5: add a comma after “removed”

17. Pg. 9 line 13: sentence starting with “Moreover” has some grammatical problem related the usage of “are”. I’m not sure how to fix it.

18. Pg 10 line 27: delete “still”

19. Pg. 11 line 7: change to “. . . deficits in simulated runoff response. . .”

20. Various places, including Pg. 12 line 27: replace “capable to” with “able to”

21. Various places, including Pg. 13 line 2: replace “structural similar” to “structurally similar”

22. Pg. 13 line 10: replace “inside” with “insight into”

23. Various places, including Pg. 13 line 16: replace “time-variant” with “time-varying” or “temporally varied”

24. Pg. 18 line 4: delete comma after “We”

25. Pg. 18 line 5: “. . . the spatial distribution of the forcing. . .” I’m not quite sure what you’re saying, perhaps because I’m still confused on the differences in the different models.

26. Pg. 21 line 6-7: The sentence beginning with “Here” is awkward. At least need to change “have” to “has”, but probably needs other changes too.

27. Pg. 21 line 11: Delete “the” before “visual”

28. Pg. 21 line 12: delete “a” before “lower”

29. Pg. 21 line 25: change to “the shape of the simulated hydrograph is acceptable” and delete “the” at the end of the line

30. Pg. 23 line 10: replace “as” with “than”

31. Pg. 23 line 19: replace “at” with “for”

32. Pg. 23 line 20: delete “More”

33. Pg. 26 line 11-12: change “has still several” to “still has several”

34. Pg. 27 line 13: delete “or if not”-not needed in English.

35. Pg. 27 line 28: replace “at first glance contradicting” with “In contrast”

36. Pg. 27 line 31: replace “amount” with “number”

37. Pg. 28 line 6: replace “although it used” with “using”

38. Pg. 28 line 8: replace “less” with “fewer”

39. Pg. 29 line 12: strange usage of "up-to-date"

40. Pg. 30 line 4: at least change "constraints" to "constrains", but I'm not clear on what exactly the authors mean... probably related to subcatchment heterogeneity.

41. Pg. 30 line 28: change to "... only yields improved performance..."

42. Pg. 31 line 20: I suggest adding to this sentence "... and the specific model discretization used." This goes back to my earlier point about high-resolution gridded models, etc.

References:

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