Hydrology and Earth System Sciences Discussions

Interactive comment on "Prediction of snowmelt derived streamflow in a wetland dominated prairie basin" *by* X. Fang et al.

Anonymous Referee #2

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GENERAL COMMENTS

This is a technically impressive paper, well-within the scope of HESS, that has the potential to demonstrate what I understand to be one of the key objectives of the CRHM research effort: to create reliable, uncalibrated and physically-based models of cold-regions hydrologic processes with minimal to no calibration. The study therefore may offer an important contribution to the Prediction in Ungauged Basins (PUB) literature for cold regions hydrology researchers. However, I believe some major revisions will be required to clarify or demonstrate some key issues presented (or not presented) within the text and figures. As it stands, these issues significantly detract from the paper, and as a result no clear or significant conclusions emerge for the modelling results or discussion. Specifically, the key issues are (1) lack of a clear description of calibration

process and role of two different DEM sources (2) incomplete or inadequate treatment of hydrologic routing between HRUs ; (3) no assessment of simulated wetland storage dynamics relative to observed; (4) inadequate description of simulated hydrographs relative to observed and; (5) insufficient conclusions regarding the potential role of using LiDAR and other geo-spatial data sources to improve modelling of ungauged prairie basins . Below are my specific comments on these issues, followed by some minor comments.

SPECIFIC COMMENTS

(1) Lack of a clear justification or description of calibration process and role of two different DEM sources

a) Given that the only calibration parameter is depression storage, there should be much greater emphasis on the potential impact of depression storage on the water balance of prairie basins. It seems that this is one of the key objectives of the study but it is not presented as such. If so, the use of LiDAR in this respect should be more thoroughly reviewed and presented as a promising tool to assess the role of depression storage in prairie hydrology.

b) Related to 1a, it seems that there should be some discussion of the calibrated depression storage parameters versus the LiDAR derived depression storage parameters.

c) Section 3.2 requires significant clarification to properly introduce the model parameterization process. Page 1112 line 13 for example, is not clear at all. Also, the referring to "all other parameters" is vague. How many other parameters and how were they chosen?

d) Related to 1b, it seems that not all other parameters were actually held constant, since basin delineations were conducted on different DEMs in both scenarios (section 3.2.1). This could have a large impact on the simulation results. Shouldn't a consistent

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source of basin information be used to assess the role of calibration? Alternatively, if the authors are assessing the role of different DEM sources and DEM-based parameterization methods on model simulations, then the structure of the study needs to be modified as such.

e) The method of parameterizing depression storage using a LiDAR DEM seems over-complicated to me (mainly the method of determining depression depths and areas) . Could it not simply be estimated from a difference grid (with appropriate grid calculation syntax to focus only on filled areas) between the filled and unfilled depression. This method, it seems, could be accomplished in most standard raster GIS software and not require ArcGIS or the cut/fill tool). Also - the generation of a depressionless DEM should be described more clearly (e.g. what algorithm was used?). The choice of algorithm has a major impact on the filled DEM and the resulting depths/areas/volumes of depressions (and I don't believe the standard "fill-pits" algorithm in ArcGIS is best suited for this). This issue needs to be addressed and resulting volumes compared for a more comprehensive treatment of depression storage estimates.

(2) Incomplete or inadequate treatment of hydrologic routing between HRUs

a) The concept of RBs, HRUs and routing between them need further explanation. Figure 6 is unclear to me. How is it possible that the routing sequence of a RB (figure 6a) can be the exact same for all RBs?

b) There should be a map of the entire basin showing HRUs and RBs to clarify this matter and the model structure in general.

c) On page 1124, line 16,the authors mention that "some type of wetland flow sequence be incorporated". To me, the preceding paragraph points to a critical naivety of the semi-distributed model structure, and is a potentially major source of error in the simulated results (particularly the hydrographs). There should be some elaboration on why the topological arrangements of HRUs was fixed for all RBs, the strengths/weaknesses and challenges of the CHRM approach in this respect, and also some discussion of

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how this issue is dealt with in other models (e.g. SWAT). Finally, as I have suggested, the potential impacts of this result on the model simulations should be discussed.

d) Regarding Figure 3 - how is it possible to incorporate this behaviour if individual wetlands are not represented explicitly within the model structure (According to Figure 6a there is only 1 wetland per RB)?

(3) No assessment of simulated wetland storage dynamics relative to observed a) The role of wetland drainage and storage is emphasized in the introduction and objectives, but there is no assessment of the model performance with respect to this aspect of prairie basin hydrology. I presume that because of the model structure, this assessment is not possible. There is also some mention of the fill-and-spill process of runoff generation in this environment (e.g. page 1108). The authors need to reconcile the lack of representation of individual wetlands in the model structure in relation to their potentially critical role in prairie basin hydrology and the lack of ability to assess wetland storage dynamics using the current model structure.

(4) Inadequate description of simulated hydrographs relative to observed

(a) While most of the simulated results are quite impressive given such minimal calibration, I do not find the simulated hydrographs to be particularly so. While hydrographs are not necessarily the most important metric for evaluating model performance, their importance in watershed and water-resource management is often prioritized for good reasons. There should be more discussion on the adequacies or inadequacies of the calibrated and uncalibrated models w.r.t. hydrograph simulation.

(5) Insufficient conclusions regarding the potential role of using LiDAR and other geospatial data sources to improve modelling of ungauged prairie basins.

a) In the conclusions, the authors refer to their "innovative method to derive model parameters using field survey data, LiDAR DEM, SPOT 5 satellite images, stream-network and wetland inventory GIS." I agree that the methods of parameterization used

in this study are somewhat innovative in the way that they were used to derive many key model parameters, thus avoiding or minimizing the need for calibration. However, this was not the stated objective of the paper. Therefore I suggest that the objectives of the study be more clearly stated in such a way that would permit some stronger and more compelling conclusions about the role of high-resolution data and other geospatial data sources in modelling basin hydrology with minimal to no calibration. If this was not part of the objectives, then I foresee the need for additional restructuring of the paper to more clearly define the key objectives and associated conclusions.

b) Related to 5a, I do not think the simulation results present a compelling case for incorporating the LiDAR-based parameterization of depression storage in future models. Whether the authors agree or disagree, they should provide a more thorough discussion of this issue, since it seems to be an important aspect of the paper. Given the price of acquiring LiDAR for example, the model results to do not justify its use for predicting basin hydrology, in my opinion. Also - is it possible that the arrangement of HRUs within the model is more important than accurate parameterization of depression storage?

TECHNICAL CORRECTIONS

P1106 L14 Missing "The" before "Majority"

P1107 L16 to end of paragraph: Objectives are very broad and should be refined

P1111 L12 The description of the two approaches is not clear as worded.

P1118 L12 Missing "the" between "partition" and "amount"

P1117 L27 Typo "Length-are"

P1121 L27 What does this mean "in which errors may magnify" (vague/unclear)

P1122 L2 Missing "the" after "had"

P1123 L26 to end of paragraph: check grammar

Figures 7-10 should be larger (hard to read as is)

Abstract: Too long

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 1103, 2010.

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