

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

Anonymous Referee #1

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Abstract: The abstract is far too long and could be shortened easily to provide relevant results with only a brief introduction of methodology. Removing the first four sentences is an easy start. Strangely absent are some numerical results of the model/method performances which should be placed in the last few sentences.

Introduction and Objectives: The introduction leading up to the objectives (given on page 4) is well written and provides good information and context regarding the dependency of wetland-prairie hydrological processes/modeling on land use, etc. But I take issue with the way the objectives have been worded given the introduction. The first objective arises naturally from the literature review but if “sensitivity” to land use, wetland drainage, etc. . . was truly an objective, then I would argue the methodology to achieve this objective was not chosen as well as it could have been and thus, was

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not really achieved in this paper. Because words like “sensitivity” have important and specific meaning in hydrology in terms of the analysis required to show sensitivity, one could perhaps simply reword the objective to state that they are introducing a model designed for the uniqueness of wetland-prairie hydrology. The authors should also clarify what they mean by a “hydrological state.” Do they mean a state variable? The second objective is a problem because the authors actually provide no justification in their introduction as to why they are setting up calibrated and uncalibrated approaches. Perhaps making reference to PUB initiatives might help. When reviewing the rest of the paper, in my opinion, the authors actually haven’t achieved their objectives. What they have done (at least what I could determine they did given the lack of clarity in section 3) is tested their model using two approaches – the “calibrated” approach in which depression storage is “calibrated” from a coarse resolution DTM, and an “uncalibrated” approach in which depth-area-volume relationships leading to depression storage were obtained from a DTM that is aggregated to 10 metres. Yet all other parameters remain the same. Interestingly enough, in their “calibration” the authors at no point present their parameter values after calibration which they should. This investigation is really a test of their model on two different DTM sizes with two different depression storage parameterizations. It is not an investigation of the sensitivity to land-use for example as that would require a much greater amount of analysis, nor other processes as described in the first objective. The authors need to reword their objectives and consider their choice of words in general a little more carefully. They should really combine their objectives into one to demonstrate what they are really trying to show – how the model performs under different depression storage approaches that use two different DTMs – only. The justification for using a different DTM for each approach is lacking and raises a lot of questions as to why the authors chose to do this and what information it really provides on the two methods for determining depression storage.

Study Site and Model Description: Figure 1 is okay but it should be followed by a MAP to show the RB’s, their locations, landuse, and thus how they drain into each other, etc. This will eliminate Figures 4 and 6. I would not accept this paper without it. It is essen-

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tial to any hydrological investigation involving land coverage influences on hydrology and I'm surprised it's not there. Please throw out Figure 5. It's well described in the text already. Perhaps the authors can call the wetland module shown in Figure 2 as simply "Wetland module" instead of "soil moisture balance calculation. . . ." Since they refer to it as the wetland module in Figure 3 and in the text on page 6. I'm wondering about two arrows in Figure 2 showing the relationship between (1) evaporation module and Muskingum routing Module; and (2) wetland module and routing module. Should both these arrows be two-way? Can you put the arrow leading off to routing from this module in Figure 3?

How is an RB different then just a sub-basin with different HRUs? Figure 5 doesn't show "similar internal structure" – it just shows that each sub-basin has each type of HRU. Is this the only criteria that determines an RB? The text on page 7 describes a "specific arrangement" of HRUs. The authors need to be careful here. Arrangement speaks to spatial position. If the spatial position of the HRUs is not an issue, don't claim a "specific arrangement". Be clear in what exactly an RB is and remove all this notion of "similar internal structure" and "specific arrangement." Perhaps there is not enough space for it, by why did the authors decide to insist that each RB have all seven HRU's? The map requested above should explicitly show all the RBs.

The model is well described in terms of context but I also think that a paper that is introducing a relatively new model and the results of modeling, the values of the parameters used (including results for depression storage from the different methods and DTMs) should be cited somewhere (they have in some cases like the value of attenuation in the Muskingum routing, but not all), if only for replicating the results by other CRHM users. A side note – as far as I am aware, the model can handle partially frozen soil and the formation of ice lenses, but on page 15, it says that earlier observations could not be used because of the partially frozen soil. Can the authors explain why this could not be simulated?

Results / Discussion: In general, the discussion does not really explain the values

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noted in the results section. The authors should take the section at the bottom of page 17 starting with "Other model.." and continuing to "frozen soil infiltration)" which is out of place, and move it to the introduction where it provides better justification for why CRHM is developed in the first place. Then the last paragraph (with the exclusion of the above sentence starting on page 17) which is not a discussion, should be moved to the conclusions. All of this would then reduce the discussion to essentially 3 paragraphs which really don't say very much.

The discussion begins with talk about the effectiveness of the PBSM model and how soil moisture was modeled quite adequately. If both approaches use the same concepts for soil moisture and PBSM, why use them as a means to compare the methods? The results show that the model actually tends to overpredict the SWE on a fairly consistent basis even though the trend generally matches the observations. Why isn't this overprediction explained? Why aren't the differences in the "calibrated" and uncalibrated approach shown in figure 8d explained? The methods are poorly compared and thus, they have not achieved their second objective. For the SWE for 2008 figure 7d:river channel, why are the predicted trends (close to zero it seems) and the observed trend (upward) so different? What is causing the opposite trend in Figure 9a? Why have the authors chosen to show only sub-basin 1 (or should they call it RB1?) for Figures 7 and 8? Table 1 shows that sub-basin 1 seems to have the lowest RMSD for wetland HRU – really, the whole point of this paper is focusing on wetlands - but if the authors are really trying to test their model, they should also show the worst RMSD values and their trends for subbasin 4 and the model's performance on the wetland HRU (and explain them please). What is that second simulated peak about (but not observed) in Figure 10a around May 6th? The authors argue on page 17 that perhaps their representation of wetland is oversimplified – if so, then why wouldn't they have incorporated this in their model in the first place which is intended for wetland-prairie hydrology? This last question speaks to a perceived duality in this paper – the authors seem uncertain as to whether they should emphasize the model's representation of wetland-prairie hydrology in general, or the influence of using two different DTMs and

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two different methods for depression storage on simulations (and their discussions and text seem to waffle between emphasizing one or the other). Because these really are two different things, and neither is presented terribly well or with great depth, I would suggest the authors pick one as their primary focus in re-writing the objectives (and part of the introduction), methodology (would suggest some reduction in text regarding model concepts), results, and discussion.

In general, I believe the authors need to explain the results and in particular differences (such as in simulated volume) between the calibrated and uncalibrated cases before this work can be published. They also need to justify why two different DTMs were used and how they impact the results (in terms of depression storage estimations and then RMSD values, etc.)

Finally, the conclusions could be strengthened by having more emphasis on the results shown and how the model performed against the observed data.

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