Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-152-RC3, 2019

Hydrology and Earth System Sciences
Discussions
Open Access
EGU

Interactive comment on "High-resolution hydrometeorological modelling of the June 2013 flood in southern Alberta, Canada" *by* V. Vionnet et al.

Anonymous Referee #3

Received and published: 14 August 2019

This paper examined the capability of a hydrological model (GEM-Hydro) in simulating the June 2013 flood event in Alberta, Canada. In particular, three sub-basins in the Bow River basin were selected to assess the impacts of spatial resolution, precipitation gauge density, and initial snow conditions on model ability in reproducing the flow volumes. Also, the model sensitivity to Manning coefficients in capturing the peak flow was investigated.

General Comments:

The objective of this paper is straightforward and this paper is well-written, easy to follow and well-structured. However, the creditability of the study has been reduced

Printer-friendly version

Discussion paper

because the study is highly localized and reads more like a report of an application of a hydrological model to a specific flood event. There are little knowledge gain for the community. In general, two major concerns are needed to be addressed in this paper:

1) Novelty of the study

While the study would be a great contribution to the development of a Canadian hydrological forecasting system, it might not be novel enough as a scientific contribution for the international community. The effects of different spatial resolutions on model simulations have been previously and heavily studied in different hydrological models and it is well expected that finer resolution could provide better simulations because of its ability in capturing the fine-scale hydro-meteorological processes. It is also expected that the inclusion of additional information (e.g. increasing network density, inclusion of SWE information) would improve model performance because of the data-driven nature of the sophisticated models nowadays. Lastly, the Manning coefficients (both channel and floodplain) are well known to be one of the most sensitive routing parameters in any hydrological/hydraulic models. Adjusting such parameter will definitely improve the model ability in matching the peak flow timing and magnitude.

Furthermore, the Discussion section of the study did not provide innovative insights on modelling of extreme flood events. Although the Discussion section was well-written and fully supported by references, the major findings of this study were merely a confirmation of what had been shown in many previous studies (as repeatedly mentioned by the authors). Therefore, there is a lack of true novelty and scientific contribution in this study. The authors should vigorously address this critical issue by providing a better discussion on what new knowledge and information the international community could learn from this study.

2) Lack of connection with other worldwide extreme flood events

The Introduction section was again well-written and the rationale of the study was well presented, however, it was highly focused on the description of the June 2013 flood

Printer-friendly version

Discussion paper

event and the previous works that were related to the 2013 flooding. The literature review did not discuss any research related to other worldwide flood events and any modelling works that address the current challenges of the modelling community in dealing with extreme flood induced by rain-on-snow events. This makes the study highly localized and event specific. The authors should provide a boarder discussion on similar research conducted in other regions, modelling strategy used in simulating such kind of flood events, and the research gaps this study could fill in for advancing the knowledge of the community.

Specific Comments:

P1L28-29: How could the results of this study guide the development of the hydrological forecasting system worldwide?

P4L10: Could the authors comment on the consistency of these four different networks?

P5L3-4: What are the drainage areas of these river basins?

P5L6-7: Could the authors provide the basic information of these 10 stations (e.g. station name, drainage area, name of tributary)?

P10L10-12: It would be better to provide the performance measures (Bias, RMSE, R2) of each basin in a table.

P10L13-14: Could the authors explain how the negative bias was removed by increasing the spatial resolution to 2.5km and 1km? Figures 7c) and 7d) do not show an obvious removal of the negative bias, instead, the points are still scattered above and below the 1:1 line.

P10L15-16: Could the authors explain why there were overestimations of cumulative precipitation using CaPA 1.0km?

P11L1-2: While the analysis on the effects of spatial resolution, gauge network den-

Printer-friendly version

Discussion paper

sity, and initial snow condition were conducted across the Oldman, Bow, and Red Deer River basins, the hydrological simulations were evaluated on Jumpingpound Creek, Elbow River and Highwood River, which are all located within the Bow River basin. I think such selection of hydrological simulations could not fully reveal the impacts of those factors controlling the flood dynamics. A better experimental design could be selecting one or two headwater sub-basins from each river basin (Oldman, Bow, and Red Deer). The results could potentially provide more information than the current setting (10 stations all within the Bow River basin) especially when different responses were witnessed after the inclusion of additional information (precipitation and/or SWE) across the three basins (e.g. consistently underestimation of cumulative precipitation plus overestimation of SWE in the Bow River, a mixture of over-and under-estimation of cumulative precipitation in the Red Deer, and underestimation of cumulative precipitation plus fairly accurate SWE estimates in the Oldman River). I wonder why the authors only focused on hydrological simulations within the Bow River basin.

P15L30-P16L15: This study did not examine the model structure and the process representation in the model at all. Providing a list of potential reasons that might affect the model performance here becomes irrelevant unless concrete proof and result analysis are given to show the underlying causal relationship. This sub-section should be better re-written.

P17L19-20: What about the ground station data from the ABE, CHRO, and COOP networks? Are they publicly available to the international readers?

Remarks:

P2L4-5, 10, and 12: could the authors check this reference please? Is this reference the correct one that the authors intended to use? I guess it should be Pomeroy et al. 2016a. Please correct me if I am wrong.

P8L18: should be "similar to" not "similarly to"

Printer-friendly version

Discussion paper

P8L24 and 27: please spell out the full name first before using the abbreviation

P8L33: missing full stop after "(18 June 2013)"

P12L5: should it be "Fig. 10c, f and Fig. 11c, f" instead of " Fig. 10d, f and Fig. 11d,f"?

P12L23: better use "hypothesis" instead of "assumption"

P17L15: delete "are"

Figures 3 and 4: better use "boundaries" instead of "limits"

Figures 10-12: it is a bit misleading to use filled area to represent the results because essentially there are only two simulations using initial conditions from OPL and SND. They might not necessarily represent the upper and lower limits of the model performance, especially when more different initial conditions are used.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-152, 2019.

Printer-friendly version

Discussion paper