Interactive
Comment

Interactive comment on "Multiobjective calibration of the MESH hydrological model on the Reynolds Creek Experimental Watershed" *by* A. J. MacLean et al.

A. J. MacLean et al.

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We would like to thank Dr. Parajka for the constructive comments and in general, we agree that our manuscript should undergo some revisions to address the concerns. For the comments that are most significant, we provide a response below.

1) Our goal was primarily to contribute a modelling case study (on a very well-instrumented basin) for a relatively new hydrologic model coupled with a land surface scheme (MESH) that was calibrated simultaneously to both runoff and snow water equivalent (SWE). One novel component of the study is that we evaluate the tradeoff

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between modelling the snowpack versus modelling basin runoff accurately while previous MESH modelling studies do not explicitly evaluate this tradeoff (e.g. see line 117). Unfortunately, as Dr. Parajka has pointed out, we did not properly position our work relative to extremely relevant work with other models in studies such as Udnaes et al. (2007), Parajka et al. (2007) and Parajka et al. (2007). We will definitely include these references and discuss the similarities and differences relative to our work in the revised manuscript. In addition, depending on feedback from the editor, we could add some scope to the manuscript if necessary that compares constraint handling approaches associated with calibrating spatially distributed models. The added scope would be another scientific contribution of our work.

2) As suggested we will add more detailed information to the manuscript. In particular, we will add detailed information on the model parameters calibrated (ranges, initial values, final values etc.) and as requested, avoid or minimize the references to an MASc Thesis.

3) We are now re-running our calibration experiments for a longer period (more than 2 years as in previous manuscript) and will also utilize a longer validation period. Furthermore, we can extract sensitivity information from our calibration experiments and plan to report this information in the revised manuscript.

Specific Comment 5). In regards to the multiple objective calibration question: There is no 'optimal' non-dominated solution. When presented with a tradeoff (i.e. non-dominated solution set) between calibration objectives, the hydrologist/modeller interested in identifying a single deterministic calibration solution must select their *preferred* solution. By definition, there is no single non-dominated solution that is clearly better than the rest in terms of all calibration objectives. In terms of how the tradeoff is changed with different weightings, varying the weights of the aggregated objective enables the identification of solutions in different regions of the tradeoff. In this particular case, the shape of the approximate tradeoff in Figure 11 implies that varying the weights and resolving the problem would yield solutions that would be very close to

a line connecting the non-dominated solutions. In other words, evaluating weight sets (0,1), (1,0) and (0.5, 0.5) provide the modeller with a very good depiction of the shape of the tradeoff that would not change appreciably when evaluating different weight sets. This is an important point that we will try to make more clear in our revised manuscript.

In general, we agree with and believe it is fairly straightforward to address the remaining 'Specific Comments' made by Dr. Parajka in the revised manuscript and final response.

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