

Interactive comment on “Quantifying the contribution of glacier runoff to streamflow in the upper Columbia River basin, Canada” by G. Jost et al.

Anonymous Referee #3

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This is a well-written paper concerning an important aspect of mountain hydrology. It is a fairly tight description of a modelling exercise and doesn't devote much space to the wider implications of glacier change for streamflow. But as a modelling exercise it makes a useful incremental contribution in outlining a method for incorporating glacier change as a parameter into the well-known HBV model. It succeeds in its own (narrow) terms and I would suggest it could be published with some revisions. Most importantly, the paper needs to be more of a rounded science contribution and less of a specific modelling report. This could be achieved by a fuller discussion of the glacier mass-balance results, of the SWE distribution, and of the wider implications for glacierized

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basins, with greater reference to the literature. As no re-analysis is required, these revisions should be regarded as relatively minor.

Specific comments are as follows: Section 1, lines 16-19: the equifinality issue would benefit from a fuller explanation.

Section 2.1, lines 1-5: need references for the stated glacier changes, and for the land cover/dam facts/statistics lower down.

Section 2.1, lines 6-14: I don't think this is really needed, it comes across somewhat as PR rather than scientific context.

Section 2.2, line 20: insert (FLK) after "Lake"; generally in this section, errors are not stated for the temperature and streamflow data. At least an estimate of these should be included.

Section 2.2, line 12: is the rate of ice loss plausible by reference of other, comparable basins?

Section 2.3, p. 4985, line 10 onwards: would a delta symbol (Δ) be better than D? Need some explanation of AM, and need to insert (s) and (a) after "slope" and "aspect". Again, would a delta symbol be better than d for dKG?. Finally, "post-processing" is a little vague in this context, could this be clarified.

Section 2.4: the approach incorporating glacier retreat is very useful, but it should probably be noted that this is only achieved in a fairly coarse way in 5-year time steps. It may be that this does not have much effect on model outcomes, although as monthly glacier contributions to streamflow can be as high as 35%, it cannot be excluded. Some discussion of this point would be helpful. This also applies to Section 2.6. In the final sentence of that section, reference is made to a modelling study by UBC: observational data would be a more convincing validation of the model, are there none available at all?

Section 3.2, p.4991, line 3 onwards: I'd have thought that model error resulting from

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spatial variation in the distribution of SWE is a more likely explanation of discrepancies than measurement error, which is surely likely to be relatively consistent. The paper would benefit from greater discussion of the likely magnitudes of SWE variation in the basin, from the literature on comparable basins if necessary.

Figure 6: ΔQ needs to be defined, zero should be at the base of the plot and scales should be reduced to show the actual variation better (as in Figs. 7 and 8).

Figure 9: put a key on both panels, define Δ and define uncertainty limits.

Section 3.3, line13: delete "to".

Section 3.3, line 15: indicate an example of a higher-discharge/no-glacier July year.

Discussion: this is reasonable, but brief. Several important implications are raised in the final paragraph without references or further discussion. I'd like to see a fuller discussion of the implications of the modelling work for these issues here.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 4979, 2011.