

## ***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 #2**

Received and published: 20 June 2011

This discussion paper by Jost et al. seeks to develop an approach for calibrating hydrologic models in large catchments with modest glacier cover (<10%) and no mass balance observations and to use the model to characterize the magnitude and timing of glacier melt contribution to streamflow, along with an assessment of uncertainty. The Mica basin (glacier cover in 2005 - 5.2%) chosen for the study is a tributary of Columbia River. HBV-EC hydrological model based on GRUs has been used for testing and guided GLUE used for mapping the glacier cover changes in the basin. Climate data from five stations within or just outside Mica basin has been used although with a substantial amount of backfilling of climate data for those stations which became op-

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erational towards the second half of the studyperiod (1965-2009). In HBV-EC model, daily snowmelt is calculated from daily meanair temperature, and glacier mass balance computed by post-processing the model output for glacier GRUs. The model calibration (1985-1999) and testing(2000-2009) phases used independent data sets for different time periods. The discussion appears to be tightly wound around the results with the main conclusions following through.It is a crisp well thought about paper with carefully edited and collated document . but the validity of model seems to be not enough with observed field data that is where they have to concentrate in detail in future for making a a very authentic model.

As far as specific editorial comments are concerned, the following needs attention:

Page 5,line 20: the abbreviation (FLK) may be added

Page 12,line 5-10: the understanding that more the glacier loss, the more the Nash-Sutcliffe E will be, seems to suggest a bias towards glacier loss.

Page 13,line 18: Was any attempt made to quantify the possible factors like gauge catch efficiency and effect of using fixed vertical precipitation gradients in the underestimation of SWE?

The paper is well written and logically communicates the work carried out, with sufficient insight into the methodology used. However, a significant portion of the climate data, which is the basis for the hydrological modelling, has been backfilled could be a limitation.

However, I must say that hydro logical modelling/testing is not exactly my forte, and I recommend this paper for publication with necessary modifications as suggested earlier and also may be sent to an expert in the hydro logical modelling for further identification of its limitations/strengths.

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