Responses to Reviewer 1

We thank Massimiliano Zappa for his valuable and helpful comments. We agree with most of his comments and are happy to address them. In the following, we summarize our responses to comments raised by Dr. Zappa. The review comments are in italics.

This paper addresses a recently very fashionable aspect of practical application of hydrological modeling experiments, namely the quantification of glacier contribution to total runoff (Huss, 2011). This topic is quite "hot" since administrations and hydropower need to know how much water might not reach the rivers if glaciers would disappear. Some approaches have been presented recently that make use of both glaciological and hydrological information for calibration of hydrological models (e.g. Stahl et al., 2008; Konz and Seibert, 2010 and Schaefli and Huss, 2011).

The proposed "guided GLUE" approach is a very slight variation of the approach presented by Stahl et al. (2008, for which by the way I also served as a reviewer). The current application is focused on a much larger river basin, does not infer climate change scenarios, present verification with respect to snow water equivalent and, foremost, present simple but appropriate considerations of uncertainty in the model parametrization. Another novel aspect is the use of evolving glacier areas during calibration (Page 4982, lines 10-12).

The results section is rather straight-forward. I like the quantification and declaration of possible errors in the estimations of SWE with snow pillows. The discussion is on the same line of the results and presents only one (own) reference to current research in this topic. The paper ends with two sentences rephrasing the first lines of the introduction and with some well known perspectives concerning possible climate implications (Barnett et al., 2005; Bloschl and Montanari, 2010).

We agree that the topic of quantifying the contribution of glacier melt to streamflow is indeed a hot topic as outlined in the citations provided by Reviewer 1 (Stahl et al., 2008; Konz and Seibert, 2010; Schaefli and Huss, 2011). It is pertinent that all three of these studies relied on measured glacier mass balance data to help constrain the glacier contribution to runoff in relatively small basins containing one or a small number of glaciers. A recent study by Huss (2011) focused on macroscale basins with many glaciers, but also relied heavily on glacier mass balance data. In fact, Huss (2011) stated that "[m]ass balance data for 50 glaciers in the Swiss Alps ... [were] central to this study." Unfortunately, glacier mass balance data are rare outside Europe, limiting the geographic transferability of the approaches used by these studies.

As outlined in the objectives of our paper, our work addresses this significant research problem by developing an approach to quantifying glacier contributions to runoff for large basins lacking *in situ* mass balance data. Our approach is thus more general than any other approach published to date. When revising our ms, we will try to better emphasise the gap in research that we identified and the relevance of our methods for water resource and climate change assessments in regions lacking glacier mass balance data.

In response to the comment that we did not examine climate change scenarios, we have conducted future projections for several GCMs and SRES emission scenarios, but believe that there is still a scientific need to address the methodological aspects in assessing the glacier runoff component in large basins with limited amounts of glaciological data. Understanding the timing and magnitude of glacier runoff is important not only for future climate change assessments, but also for current operational forecasting and water resource evaluation. We comment further on future scenarios below, in response to another comment by Reviewer 1.

Our ‘guided GLUE’ approach might appear to be a "very slight variation" of that used by Stahl et al. (2008) because both are based on the GLUE methodology. However, there are some important differences. The most important is that we combined the GLUE methodology with an evolutionary algorithm to assist in selection or rejection of behavioural parameter sets. While the rejection process in our approach is still subjective, just like in the classical GLUE approach, our approach has the advantage that the modeller has some idea of how “good” she or he can potentially get according to one or more goodness-of-fit criteria. In particular, with a low ratio of ‘number of model runs’ to ‘number of parameters’ (i.e., when there is potential that all parameter sets need to be rejected), this approach helps the rejection decision process. In a revised ms, we will underline this argument with references to more literature and also incorporate the recent work by Schaefli and Huss (2011), who also combined GLUE and global optimization methods, though in a different way and for different reasons.

Another difference is that Stahl et al. (2008) used a stepwise approach, whereby climate gradients were adjusted to fit winter mass balance data (using minimum mean absolute error as a criterion). Monte Carlo simulations were applied to calibrate the remaining parameters. In contrast to Stahl et al. (2008), we never ‘locked in’ any parameters, not only because the absence of winter balance data did not allow us to constrain or ‘lock in’ climate gradients, but also because we believe that the uncertainties in all parameters need to be propagated through to streamflow and icemelt predictions: as mentioned in the manuscript, the temperature lapse rate showed the highest correlation with glacier mass balance and the Nash-Sutcliffe efficiency of streamflow predictions.

Another contrast to Stahl et al. (2008) is that our approach follows the GLUE methodology more closely; i.e., the predictions of the behavioural parameter sets are weighted by the goodness of fit (i.e. likelihood) measure associated with a simulation. Simulation uncertainty limits are not given as minimum and maximum from all ensemble predictions – as in Stahl el al. (2008) – but are estimated from the cumulative likelihood weighted distribution of behavioural parameter set predictions.

Major Issues

1) The novelty of the simulation exercise is rather limited. I acknowledge that using changing glacier areas in the control period is a new aspect, but the authors make no effort to show, that this is really helping the calibration process by making a calibration WITHOUT updating the glacier areas. So, please demonstrate in your reply the added value of glacier areas updates for the calibration of your model. This might be key feature giving your paper visibility in the scientific community.

We believe that our use of sequential digital terrain models to assess the contribution of glaciers to streamflow and the 'guided' GLUE approach are significant and original contributions to science and practice, as we have argued above. In regards to changing glacier areas, we conducted future projections for several GCMs and SRES emission scenarios as part of a broader research project. We accounted for changing glacier cover by updating the glacier representation within HBV-EC every ten years using projections generated by the UBC Regional Glaciation Model, a physically based dynamic glacier model developed by Garry Clarke and his research group in the Earth and Ocean Science Department at the University of British Columbia. The updating of glacier area during the model calibration was done, in part, to be consistent with the updating in future climate impact simulations. While the differences between static and dynamic treatment of glaciers in HBV-EC are small during model calibration, they cannot be neglected in future projections due to the climate-change-related reduction in glacier area. To illustrate this point, we compare the future ensemble model predictions with one GCM (the Canadian CGCM3.1-T47) using a static (observed) glacier cover from 2005 throughout the 100 year simulation period to ensemble predictions using dynamic glacier area change. Because Garry Clarke and his group are currently writing up the glacier modelling work for publication, we cannot present any graphs showing future glacier evolution.

Figure 1 presents future projections of August streamflow using static glacier cover (blue) and a dynamic projected glacier cover (red) for three emissions scenarios (B1, A2 and A1B) based on downscaled output from CGCM3. For each glacier representation, the time series show a range of values representing the effects of parameter uncertainty (based on the guided GLUE approach). The key conclusion from this graph is that the effect of glacier retreat on predicted streamflow *can* be significant over 10-year intervals, depending on the stage and rate of deglaciation. For example, there is a significant change in the difference between the simulations for static and dynamic glacier cover in Figure 1 for the decade centred on 2080 in the A2 scenario and on 2070 in the A1B scenarios.

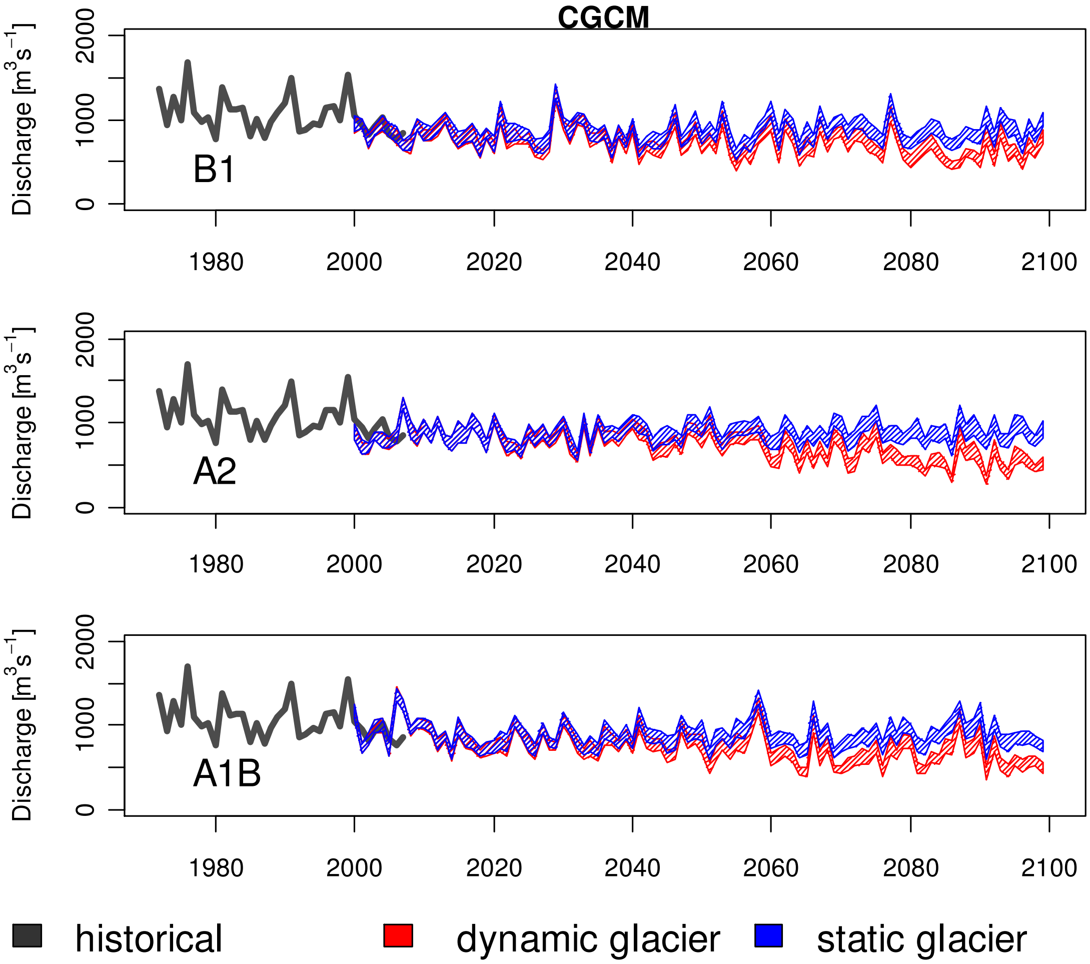

Figure 1. Mean August discharge simulated with static 2005 glacier cover and with dynamic glacier area updates in 10 year intervals.

2) The authors stated in the first lines of the introduction that glaciers might vary they contribution to total runoff according to the current weather situations. I don't find in the results much about proxies indicating the relation between climate and contribution of glacier melt to total discharge (e.g. see Zappa and Kan, 2007). The stakeholders of such studies would surely appreciate to learn under which special conditions they have to expect a smaller or larger portion of glacier melt in the runoff hydrographs.

We consulted the recommended publication and its related work (Schär et al., 2004; Koboltschnig et al., 2008). The statistical analysis in these studies focused on the interesting comparison of the extreme year 2003 with other years. In the case of a single watershed, as in our study, a statistical analysis would not provide much more insight than the comparison of a year with negligible ice melt with the year that had the highest (modelled) ice melt on record, as we have presented. The "special conditions" under which a hydropower company could expect a lower or higher contribution of glacier ice melt are well understood: a year with lower snow accumulation and higher summer temperatures will have more ice melt and *vice versa* (see, e.g., Table III in Moore and Demuth, 2001, and publications by Dr. Zappa that examine runoff during the extreme year 2003). We had conducted a statistical analysis prior to submitting our manuscript but thought that the results would be of marginal value to the scientific community since they are too specific to the Mica basin. For illustration, Figure 2(see below) shows how simulated summer-autumn ice melt varies with cumulative snowfall in the preceding winter and mean June-September air temperature.

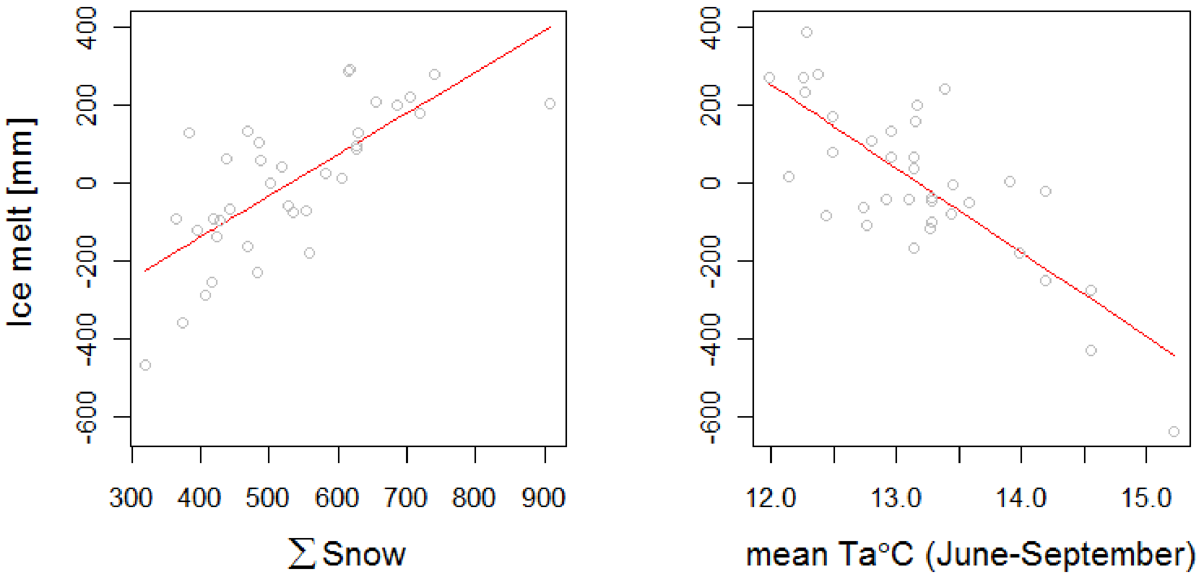

Figure 2. Observed partial residuals for ice melt predicted from total snowfall in a hydrological year (starting 1st October) and mean summer temperature; slopes obtained by multiple linear regression ($R^2 = 0.77$, $p < 0.01$). Note that the residuals are expressed relative to the mean ice melt, thus accounting for the negative values.

3) Soft glaciological information (7 to 9 km^3 volume change) is used to condition the calibration. I wonder if similar information is available also for (parts of) the verification period. This would demonstrate whether the selected parameter sets really suits as predictor for glacier melt contribution.

While it would have been useful to have an estimate of glacier volume change for the test period, only two digital elevation models were available for the study region.

Minor comments

1) The references presented supporting the quality of the HBV-EC (Page 4984, line 19) are difficult to obtain.

We agree that these citations are difficult to obtain. Cunderlik et al. (2010) is a study that was carried out for BC Hydro to assess alternatives to their current forecasting model. Fleming et al. (2010) summarized these findings in a conference contribution. We understand that the authors of that report are planning to write up the work for publication, but a more accessible reference is, unfortunatlely, not currently available.

2) Scale issues in comparing models and SWE observations are a permanent problem in quantitative verification of hydrological model. The authors handle this with two lines (Page 4991, lines 3-4). You might expand on this starting from the work by Bloeschl (1999).

We have published work on this issue and will incorporate this work together with a reference to Bloeschl (1999) and other relevant publications.

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Barnett, T. P., Adam, J. C., and Lettenmaier, D. P.: Potential impacts of a warming climate on water availability in snow-dominated regions, Nature, 438, 303-309, 2005.

Bloschl, G., and Montanari, A.: Climate change impacts- throwing the dice?, Hydrological Processes, 24, 374-381, 2010.

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Schaefli, B., and Huss, M.: Integrating point glacier mass balance observations into hydrologic model identification, Hydrol. Earth Syst. Sci, 15, 1227-1241, 2011.

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Stahl, K., Moore, R. D., Shea, J. M., Hutchinson, D., and Cannon, A. J.: Coupled modelling of glacier and streamflow response to future climate scenarios, Water Resour. Res., 44, 2008.

Responses to Reviewer 2

We thank Reviewer 2 for this constructive review. We placed our comments directly underneath the issues raised by Reviewer 2 (the reviewer's comments are shown in *italics*).

This discussion paper by Jost et al. seeks to develop an approach for calibrating hydrologicmodels 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 Micabasin (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 hasbeen used although with a substantial amount of backfilling of climate data for those stations which became operational 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

This will be added in a revised version of the manuscript.

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 12, lines 5-10 describe Figure 3 in the ms, which shows that the Nash-Sutcliffe efficiency E exhibits an optimum value associated with negative glacier volume change. This indicates that the quality of streamflow prediction does require that at least the direction of glacier volume change must be simulated correctly, i.e., that parameter sets predicting positive mass balance do a worse job of predicting streamflow. In contrast to other studies (e.g., Stahl et al., 2008), the optimal E can be achieved by a wider range of glacier volume changes (-5 to -40 km^3), which reflects the modest glacier cover in the large Mica basin and the associated lower sensitivity of streamflow to glacier melt, relative to studies of more heavily glacierized catchments. However, even if there had been a narrow peak of E over glacier volume loss (like in Stahl et al., 2008), Schaefli and Huss (2011) showed that one has to be careful not to infer glacier volume loss from such a relation since the 'real' glacier volume loss does not need to coincide with the glacier volume loss that gives the best model performance (due to model structural errors).

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?

We did not quantify the gauge catch efficiency but the precipitation lapse rate (PLAPSE in Table 1) was one of the parameters that we calibrated. In a revised version we will provide the uncertainty in SWE by plotting predictions from the entire ensemble parameter set instead of just the model with the highest Nash-Sutcliffe efficiency. In this context, we want to clarify that despite the fact that HBV-EC only allows static precipitation and temperature lapse rates, by delineating Mica basin into six climate zones our model setup can account for seasonal changes of vertical climate gradients (see Figure 1 below; zones 5 and 6 are forced with the same climate station, all others are forced with a separate station). With this setup, differences between lower elevation zones (zones 1 and 2) and higher elevation zones (zones 3-6) can vary seasonally and also spatially (e.g. different vertical gradients between zone 1 compared to zone 6 and zone 1 compared to zone 3).

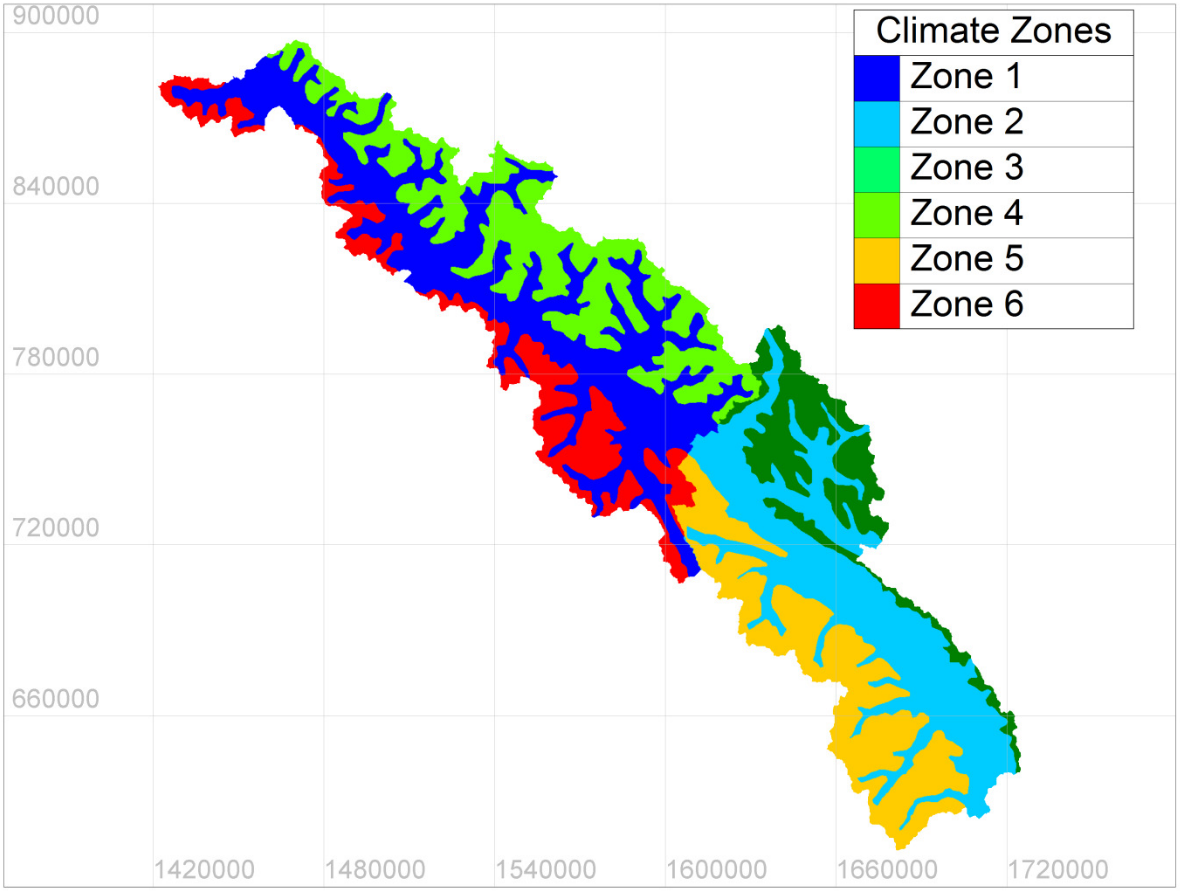

Figure 1. Delineation of climate zones used for hydrological modelling.

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.

As stated in our manuscript, we used only measured climate data for model calibration and testing, except for eight years of backfilled data from FLK (1985–1993) and 1985 for MOL. We used backfilled data for model spin up and to simulate the period shown in Figures 6-8. In a revised version of the manuscript we will remind the reader of this data limitation in the results section regarding these figures. We did some testing of different input configurations – higher number of climate zones but with backfilled data versus lower number of climate zones with observed data – and found that accounting for more spatial variation in climate forcing by using a higher number of climate zones led to better model performance.

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.

References

Schaefli, B., and Huss, M.: Integrating point glacier mass balance observations into hydrologic model identification, Hydrol. Earth Syst. Sci, 15, 1227-1241, 2011.

Stahl, K., Moore, R. D., Shea, J. M., Hutchinson, D., and Cannon, A. J.: Coupled modelling of glacier and streamflow response to future climate scenarios, Water Resour. Res., 44, 2008.

Responses to Reviewer 3

We thank Reviewer 3 for this constructive review. Our comments can be found directly following the issues raised by Reviewer 3 (*the reviewer's comments are in italics*).

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 massbalance results, of the SWE distribution, and of the wider implications for glacierized basins, with greater reference to the literature. As no re-analysis is required, these revisions should be regarded as relatively minor.

When revising our manuscript, we will put more focus on the wider implications of our work and discuss our mass balance results in the context of other published work and include the most recent literature (e.g., Huss, 2011; Kaser et al., 2010). We will better emphasize the research gap that we identified and our solution – using sequential digital elevation models to estimate the contribution of glacier melt to streamflow as an alternative to *in situ* mass balance measurements – and highlight the differences from other methods that are currently used in glacio-hydrology (see also response to Reviewer 1).

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

We agree and will add a better explanation in a revised version of our manuscript.

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

The 1985 glacier extents were generated from provincial photogrammetric mapping; the 2000 and 2005 extents were created using digital image processing of 43 landsat satellite images (see Bolch et al., 2010). Land cover information is based on the VRI (Vegetation Resources Inventory) data of British Columbia (http://www.for.gov.bc.ca/hts/vridata/). The dam facts are from personal communication with the lead of the hydrological forecast group at BChydro. All references will be added in a revised version of the manuscript.

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

We will delete this section.

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.

FLK will be inserted.

The error associated with air temperature measurements will include instrumental error (likely to be small relative to spatial and temporal variability) and also the possibility of bias associated with site characteristics, which is difficult to estimate on the basis of available information.

The streamflow data were calculated from a water balance for the reservoir, and were provided by BC Hydro following quality control. They have not conducted a formal error analysis of these values, but they do routinely use them in their operation of the reservoir (Frank Weber, BC Hydro, pers. comm).

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

Schiefer et al. (Schiefer et al., 2007) estimated a mean thinning rate of -0.78 ± 0.19 m yr^{-1} for all of British Columbia between 1985 and 1999. Table 1 in Schiefer et al. (2007) summarizes thinning rates (and volume changes) for different mountain regions. Compared to other mountain regions in British Columbia, the estimated thinning rate for Mica basin, -0.43 m yr^{-1}, is on the lower end. The geodetic estimate of thinning for the entire Columbia region is -0.53 m yr^{-1}. Northern, Central, and Southern Rocky mountain region experienced thinning rates between -0.57 and -0.86 m yr^{-1}. The Coast had the highest ice losses with thinning rates between -0.61 and -0.89 m yr^{-1}.

The geodetic rate of mass loss for Mica basin is slightly lower than the *in situ* measurements of mass balance at Peyto Glacier, Alberta, which averaged approximately -0.6 m yr^{-1} between 1966 and 2005. These data are collected by the Glaciology Section of Natural Resources Canada, and can be viewed via the following link: http://www.statcan.gc.ca/pub/16-002-x/2010003/ct006-eng.htm. Peyto Glacier is located to the east of Mica basin in the Rocky Mountains, which receives less snowfall than Mica basin (http://www.statcan.gc.ca/pub/16-002-x/2010003/m004-eng.htm). Therefore, the difference between the rates of mass loss for Mica basin and Peyto Glacier is consistent with the differences in the climatic settings.

Section 2.3, p. 4985, line 10 onwards: would a delta symbol (_) 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.

We agree that delta would be better in this manuscript and will change this, as well the requested changes to slope and aspect.

As mentioned in our manuscript, “The coefficient *AM* controls the sensitivity of melt rates to slope and aspect.” For AM = 0, there is no difference between slopes and aspects (i.e, AM = 0). Positive values for AM produce a difference between, e.g., north- and south-facing slopes, and also adjusts for slope gradient. For example, on south-facing slopes, greater melt would occur on a steeper slope. This parameterization allows the effects of spatial contrasts in solar radiation to be mimicked.

The net mass balance for each GRU is calculated from SWE and glacier ice melt time series for each glacier GRU. The total mass balance for the Mica basin is calculated from area-weighted net mass balances from each elevation band. For more details see Stahl et al. (2008).

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?

See response to Reviewer 1. As outlined in that response, the difference between static and dynamic glaciers is smaller than the parameter uncertainties for the 1985-1999 period. However, projections of glacier change and streamflow to the year 2100 indicate that there can be periods when glacier area changes within a decade are large enough that the glacier area needs to be updated.

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

We addressed this issue in a response to Reviewer 1. In a revised version we will address the parameter uncertainties by simulating SWE for all three snow pillows using all ensemble members instead of just the best performing parameter set.

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 _ and define uncertainty limits. Section 3.3, line13: delete "to".

We agree and will address these issues as recommended.

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

The issue of a delayed streamflow response due to routing in the glacier storage compared to a non-glacierized basin can be best seen in Fig. 9. We will add a line at Q = 0 in Fig. 9 to help highlight this effect and discuss more explicitly the routing issue with reference to this figure.

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.

The discussion will be strengthened with more reference to recent literature. As pointed out in the beginning of this reply, we will better emphasize the research gap that we identified to highlight the importance of this work for assessing the contribution of glacier runoff to streamflow for large basins lacking mass balance data.

References

Bolch, T., Menounos, B., and Wheate, R.: Landsat-based inventory of glaciers in western canada, 1985-2005, Remote Sensing of Environment, 114, 127-137, 2010.

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Kaser, G., Großhauser, M., and Marzeion, B.: Contribution potential of glaciers to water availability in different climate regimes, Proceedings of the National Academy of Sciences, 107, 20223-20227, 10.1073/pnas.1008162107, 2010.

Schiefer, E., Menounos, B., and Wheate, R.: Recent volume loss of british columbian glaciers, canada, Geophysical Research Letters, 34, 1-6, 2007.