

Interactive comment on “Meltwater runoff from Haig Glacier, Canadian Rocky Mountains, 2002–2013” by S. J. Marshall

Anonymous Referee #1

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The paper by S. Marshall presents the analysis of an exceptional 10-year data record on the surface energy balance of Haig Glacier, Canadian Rockies. The author describes and applies a detailed energy balance model at the distributed scale that makes use of these high-resolution data and calculates surface mass balance and runoff components of Haig Glacier. The study is motivated by the quantification of the importance of glaciers to regional runoff in a large-scale drainage basin for which only limited data was available so far. The paper is very well written and presents the data, the model and the results comprehensively, and will be a valuable addition to scientific literature in this field. Nevertheless, I have a few concerns that should be addressed, as well as a number of detailed comments.

Substantive points:

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Disagreement between motivation and methods: I noted a certain discrepancy of scale between the main motivation of the study and the methods applied. Estimating the contribution of glaciers to regional runoff is intrinsically uncertain due to various unknowns and generally rather requires data sets with a limited resolution (in both space and time). However, the author focuses the paper on the description of highly detailed measurements of the surface energy balance and applies a sophisticated model at 30 min (!) resolution. For the given motivation, this appears to me like an “overkill”. I.e. if one wants to know contribution of snow melt and ice melt for the annual scale and the summer months, much simpler methods would probably lead to similar results. This comment should not in any way criticize the good presentation of the data and the methods, or the study in general, but might lead the author to partly reconsider the principal motivation that is mainly the large-scale impact in the present paper.

Is there an accumulation model? Whereas a lot of effort is invested into the description of the ablation component, accumulation remains almost unmentioned throughout the paper, although winter snow quantity and its spatial variability importantly determine the depletion pattern, and thus the albedo and surface melting. After some re-reading and searching I believe to understand how the model is set up: Measured distributed accumulation at the end of winter is used as a starting condition to melt model. This is a very good solution in my opinion (as long as winter balance data are available). However, it should be better introduced and presented more clearly to the reader. Also the limitations that this poses to a further application of the model should be discussed. Also some more details should be given: At which date is the model initialized? Is the quality and data-density of the winter surveys always the same or does it vary over time?

Glacier geometry change: As the glacier showed a major thinning over the 10-year period I would expect a retreat of the glacier tongue. Due to the limited size of the glacier, this might have a considerable effect on total area which is directly correlated to the runoff totals of the glacier. For the entire period, however, the author assumes

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glacier geometry to be invariant. The effect of assuming such a constant geometry in the modelling should be investigated. It might be negligible but given the goal of quantifying volumes of melt water contribution, this point should certainly be discussed.

Validation with discharge: The best validation of the distributed energy balance model is clearly the proglacial discharge which yields a temporal resolution that is comparable to that of the model and direct information on the integrated melt water volume. Why is there no validation against this variable? It is clear to me that discharge series are only short, are not perfectly accurate and that a runoff routing model would be required to perform a direct comparison. Nevertheless, correlating daily means of runoff and surface melt (shifted by the time found in Fig. 10) over the periods with data would provide a relatively simple but interesting validation of the model. As this paper invests a lot of effort in the development and the forcing of the model it would be nice to see some more validation of the output to underline its performance.

Detailed comments:

page 8358, line 7: You might consider referencing Radic and Hock (2014) here who provide a comprehensive overview about this topic.

page 8358, line 20: at least the beginning of this paragraph appears to belong to the "study site" section rather than the introduction

page 8359, line 20: "Hone" => "Rhone"

page 8360, line 22: Winter mass balance results are described before the reader knows how it is measured. Might need some restructuring.

page 8361, line 3: 80m spacing only along the centreline or in the spatial domain? It is also not clear what the accuracy in the glacier-wide winter mass balance data is (i.e. extrapolation from point measurements to a area-averaged value). In any case it would be helpful to have an overview figure that provides more details on the measurement program (e.g. location of winter and summer balance measurements etc.)

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page 8364, line 1: Probably "Huss et al., 2008" instead of "Huss et al., 2011"

page 8365, line 13: This model is very interesting but I wonder if it does not require inputs on snow porosity (or a snow / firn densification model) and a prescribed permeability of the ice surface. Without this information it is difficult to understand.

Section 3.2.: I would rather expect this section (data description / homogenization) before the model description. Or is there any specific reason to do otherwise?

page 8368, line 18: So, the calculations are performed on an irregular grid? Or is this just the original resolution of the ASTER GDEM? Please clarify.

Section 4.1.: I would suggest slightly shortening this section. It is well written and interesting but, in my opinion, too distantly related to the main motivation of the paper.

page 8374, line 19: Here and elsewhere. Symbols for Glacier-wide winter (B_w) and annual mass balance (B_a) should be made consistent with the current terminology (see Cogley et al., 2011)

page 8377, line 10: If the model calculates the volume change from the melting of firn, a firn model would need to be included to evaluate the extent and the thickness of the firn layer. If such a model exists it should be mentioned, or, if not, the assumptions be stated.

page 8385, line 29: Earlier in the manuscript as well as in the abstract the final result of 42% always referred to as the contribution from glacier and firn melt. Here, it has suddenly become the contribution from storage change. This is not the same! Even in years with no storage change ($B_a = 0$) there will be a notable contribution from ice melt. This inconsistency in the terminology should be corrected. Furthermore, I asked myself whether the contribution of ice melt in balanced-budget years would be quantifiable with the model. As the mass loss over the observation period was strong, the 42% ice melt contribution should be put into context: Is it only that high because of glacier mass loss?

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Figure 1: Whereas I consider panels b) and c) as not absolutely necessary, panel d) should be improved and enlarged. It would be helpful to see surface contour lines as well as more information on the mass balance measurement set-up. Figure 2: Maybe a legend in each panel would be easier to understand than the description of line colours in the caption

Figure 6 is interesting but it would even be better to see this information on a map. This would allow interpretation of the strong mass balance variability at the same elevation in the context of glacier geometry.

References:

Cogley, J. G., Hock, R., Rasmussen, L. A., Arendt, A. A., Bauder, A., Braithwaite, R. J., ... & Zemp, M. (2011). Glossary of glacier mass balance and related terms. IHP-VII Technical Documents in Hydrology, 86.

Radić, V., & Hock, R. (2014). Glaciers in the Earth's hydrological cycle: Assessments of glacier mass and runoff changes on global and regional scales. In *The Earth's Hydrological Cycle* (pp. 813-837). Springer Netherlands.

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