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Meltwater runoff from Haig Glacier, Canadian Rocky Mountains, 2002–2013

Response to Anonymous Referee #1

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.

Many thanks for this generous summary.

Substantive points:

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 want 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.

I agree with this assessment. Although I have not done the numerical experiments to test or quantify this, I suspect that the reviewer is correct that (i) a simpler methodology, like PDD melt modelling, and (ii) daily mean meteorological variables might give similar values for the main results that are discussed here: bulk monthly melt and runoff. This would be a worthwhile thing to explore, in fact, in some simple sensitivity experiments, to quantitatively assess what level of sophistication and resolution is warranted if one is only interested in e.g. monthly runoff. I do not undertake this additional study here, as the manuscript is already too long, but I have added a short discussion to acknowledge this point and to suggest that this be done, ll.781-787.

As the reviewer will surmise, my research group has other specific interests that motivate the need to resolve the diurnal cycle and some specific energy balance processes. Resolution of the diurnal cycle allows a consideration of some specific energy balance processes that are included in the model, such as overnight refreezing (which delays meltwater production the following day), biases in the time of day that cloud cover impacts the site (with a tendency to clear mornings and cloudy conditions developing through the afternoon in summer months), and the lag between peak insolation and peak temperatures, which affects detailed melt patterns

through the day. While implicit in the model, none of these processes are examined or evaluated within this manuscript, so the reviewer's point is well taken. I appreciate that this and other, more detailed processes are not the focus of this manuscript.

Another example is ongoing research to characterize the storage/delay of meltwater runoff (modelled vs. measured at the stream gauge), which is based on the diurnal hydrographs and their seasonal evolution. The current manuscript will help to serve as a building block for followup studies such as these, where the more detailed process treatment and sub-diurnal time steps are appropriate, so I believe it is helpful to present it here. Now discussed, ll.219-229. I do agree that the methodological approach may be overkill for the current objectives.

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?

Fair point, as the methodological description is certainly slanted to the melt model and the treatment of accumulation was a bit scattered through the manuscript. The information was mostly there, but has now been consolidated and slightly elaborated to better explain how accumulation is treated. See the new section 2.2 (methods). The 'results' (snow climatology from the site) are now presented in a new section 4.1

The reviewer is correct, we have end of winter (May) snow surveys in most years, $b_w(z)$, based on a centreline network of 33 points (4 snowpits and 29 additional probing sites). This is mapped onto a distributed snow accumulation field $b_w(x,y)$, which is specified as an initial condition for the melt modelling. Snow surveys from 2002-2013 were carried out between May 4 and June 1, but most commonly in the second week of May. May 1 is taken as the starting day for the melt model simulations, although the results are not sensitive to this as there is little melt/runoff in May. There is certainly some error associated with May (and June-to August) snowfall that we may have missed, depending on the date of our winter snowpack surveys. I only recall one year, however, where we were 'too late' and missed some of the winter snowpack, i.e. the glacier outlet stream was running and the snowpack on the lower glacier was isothermal, water-saturated, and runoff had presumably begun.

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 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.

This would certainly be a concern over longer simulations. This was neglected as the DEM used to drive the model is from 2005, so is believed to be reasonably representative of conditions over the study period (2002-2013), and our observations indicate that the glacier has thinned more than it has experienced areal loss. The terminus retreated about 40 m over this time. Nonetheless, point taken – one should not assume too much, especially if an assumption can be tested. We do not have good estimates of area change over the study period, but based on the modelled average rate of thinning and assuming that $dA/A = dL/L \approx -2\%$ (linear with the assumption that area changes are only occurring at the terminal margin), I have added a brief sensitivity study to the discussion to estimate the effect of glacier area changes of -2% and, more conservatively, -5% , with the change introduced at the terminus. That is, assuming the glacier is 2 or 5% longer, descending further down-valley. See ll. 738-751 for the discussion. For a glacier area loss of 2%, the modelled runoff declines by 2.6%. The relation is nonlinear because the more extended glacier reaches lower elevations, where it experiences higher specific discharge. For an area loss of 5%, the runoff declines by 6.6%.

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.

All valid points as well. The reviewer is correct that I did not embrace this as a validation because runoff data are limited and are biased to the late summer, when the glacier is mostly exposed ice and the runoff pathways are well developed. Hence it cannot be used as a rigorous test of the model, e.g. concerning some of the main uncertainties associated with snow melt (density, albedo, and location of the transient snowline). There is nonetheless some information here that can be used, in particular daily mean modelled runoff vs. measured discharge (which admittedly has a high +/-). I have followed the reviewer's suggestion and added a discussion of this to the manuscript, with some comparison between modelled runoff vs. measured discharge see ll. 591-607.

Over the period of record of Figure 9, the modelled melt totals agree reasonably well with measured discharge, i.e. within 12%, but the correlation between daily measured vs. modelled runoff is lower than I expected, ca. $r = 0.6$. Part of the problem is a lack of rainfall data, which also feeds the stream and should be extracted from the measured discharge for a true comparison. Measured daily discharge also appears to lag the modelled runoff, with a peak correlation ($r=0.65$) found for a two-day lag. This is an interesting result and relates to storage/delays within the glacier drainage system; as for the diurnal cycle, the stream recession curve is diffuse, with melting able to shut off quickly but a much slower decline in the measured runoff during cold intervals in the summer. The manuscript is already near its limit for content, I believe, so a more detailed hydrological analysis will have to wait for followup studies.

Detailed comments:

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

Added as suggested, thank you.

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

This is true, probably too much detail for the introduction. Revised and shortened.

page 8359, line 20: “Hone” => “Rhone”

Revised.

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

It is a valid but slightly difficult point. This paper takes the winter mass balance as a ‘pre-defined input’ or an initial condition for the model, rather than a result, and the snow data is described in a bit more detail in Adhikari and Marshall (2013). But I understand the confusion here. I have rewritten this section, also to address the point below. There is a new subsection 2.2 that describes the snow survey sites, Figure 1 has been revised to show this, and the snow depth results (Table 1) are now presented and discussed in Table 2 and subsection 4.1. This flows more conventionally now, I think, i.e. methods in section 2 and results in section 4.

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.)

More explanation has been added to the new subsection 2.2 (ii.132-148) and Figure 1 is revised.

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

Revised.

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.

Clarified in the text, ll.292-299. The model is simplistic: there is no densification and I adopt a constant irreducible water content of $\theta_s=0.04$, after Coléou and Lesaffre (1998). Meltwater percolates without delay to underlying grid cells, and occupies available pore space with liquid water fraction θ_w , until $\theta_w = \theta_s$ (saturation). Once saturated, water continues to percolate downwards through the snowpack until it finds available pore space or reaches the snow-ice interface. The glacier ice is assumed to be impermeable, with instantaneous drainage.

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?

Interesting, it did not really occur to me. No reason to do otherwise – I have moved this to 3.1, as suggested, which makes sense as this is now parallel with the flow of the presentation of results in section 4.

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.

Well, a rectangular grid – this is the original ASTER resolution, at 1-arcsec, which is often advertised to be “~30 m”; the ASTER data has been projected from a North American lambert

conformal conic to an x-y (UTM grid), and the direct projection has $dx \neq dy$ because 1-arcsec in latitude is coarser than in longitude (off the equator).

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.

Now Section 4.2. Shortened as suggested, though retained; while not the main motivation, tis true, this data is input to the energy balance model and is of interest for understanding the glacier-climate regime in this region.

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)

Well noted and corrected, thankyou.

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.

There is no firn model (now stated, l. 386-387) – the firn zone is known observationally, and it is just assumed to be deep enough that this part of the glacier has been firn throughout the study period, with constant density. i.e. it is essentially treated the same way as glacier ice, with firn instead of ice above a specified altitude.

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.

This was ambiguous writing, now corrected. I did not mean storage change, but the 'storage reservoir; of firn and ice. Thinking of glacier runoff as a combination of water from storage plus water from the seasonal snowpack.

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?

Yes, it would still be quantifiable, even with $B_a > 0$, as melt at every grid cell is calculated and tracked through the full summer, including the transition from snow to ice (where and when this occurs). Yes, certainly this is true that the 42% is specific to this period of negative mass balance. Over the study period, 2010, ice/firn melt ranged from 19-62% of modelled runoff. This is noted on l.761 and l.820, in the conclusions. With $B_a = 0$, there would certainly be some runoff from the glacier ice in the ablation zone, but I will anxiously await this event to see how it looks.

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 1 has been revised.

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.

I actually started with this, but found it did not present the information as clearly, perhaps because of my limited capability in spatial contour/surface plotting in matlab. Spatial patterns are evident but actual values and vertical gradients are less clear.

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.