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 ≈ −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, thankyou.

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 ofθs=0.04, after Coléou and Lesaffre (1998). Meltwater percolates without delay to underlying grid cells, and occupies available pore space with liquid water fraction θw, until θw = θs (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 ≠ 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, Iasked 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.Â˘aIHP-VII Technical Documents in Hydrology,Â˘a86.
Radi´c, V., & Hock, R. (2014). Glaciers in the Earth's hydrological cycle: Assessments of glacier mass and runoff changes on global and regional scales. InÂ˘aThe Earth's Hydrological CycleÂ˘a(pp. 813-837). Springer Netherlands.

Response to Anonymous Referee #2

General Comments:

As the author points out, little has been reported on glacier runoff from the Canadian Rocky Mountains, so this paper addresses an important need. It has been thought that the long standing mass balance program at Peyto Glacier would one day address this need, but detailed runoff records there ended well before AWS records began at Peyto, where only recently have efforts been made to acquire runoff data that coincide with AWS records. Runoff data for Haig Glacier do not cover lengthy periods but those that were obtained coincide with some of the twelve years of AWS records obtained at the glacier, such as to give credibility to the 2002-2013 runoff simulations. Notwithstanding the long list of specific comments stated below, I did find this to be an interesting and stimulating paper to read. Most of my comments have to do with presentation and curiosity about the results.

There is need for editorial corrections to text, tables and figures, and to tighten and clarify the presentation in some parts of Sections 2 (such as instrumentation list) and 3 (notably, topographical corrections to radiation inputs). Little modification is required beyond that other than responding where appropriate to points that I raise out of curiosity, certainly none that requires reanalysis because the paper appears to be technically sound.

Many thanks for these comments and for the suggestions here and below to improve the clarity of the presentation. I have incorporated all of these suggestions.

Specific Comments:

p.8356, l.19: 'mountain' rather than 'mountains'
p.8357, l.25: Delete 'all of' and 'of' at beginning of l.27.
p.8358, l.7: Here and wherever else it occurs, state 'time scales' rather than 'timescales'.
p.8359, l.19: I suggest replacing 'other' with 'some European' to expand the scope of the narrative.

All of the above revised as suggested.

p.8360, l.1: 'Sections' rather than 'Sects.'

In my copy of the submitted text this was 'Sections' so I left as is – Sect. is perhaps a copy-editing preference with HESS?

p.8361, l.18-27: 'Each AWS measures...' The author should state station instrumentation in tabular form rather than as part of the text, where it can be difficult to keep track. I found reference to the SR50 near the end of Section 3.1, where its role in calibrating the energy balance model is stated. Perhaps data from this sensor was also useful in calibrating the stochastic summer precipitation model that is introduced later in the paper though I see no mention of this.

Good suggestion; the setup is fairly standard so I was not diligent here, but I have added a new table to clarify this (Table 2), and rewritten this section to address concerns of both reviewers and hopefully increase the clarity. See Table 2 and section 2.3, ll. 150-159.

p.8361, l.29: Delete 'a total of'.
Revised as suggested.

p.8362, l.9-10: The GAWS is not listed as such in Table 1, but is listed there as AWS?
Amended to GAWS.

p.8362, l.17-18,29: Despite the statement of 'data' in the plural being lost to writing, this reads better if you state 'Data are recorded', 'represent' rather than 'represents', remove 'a' before 'snapshot' and 'These data' in l.29.
Revised as suggested (first occurrence); the second part has been rewritten and is now n/a.

p.8363, l.16: 'approximately' rather than '~'
Revised as suggested.

p.8364, l.6-14: It may be better to state 'Net surface energy, Q_N, is determined by:
$$Q_N = Q{\downarrow}_S - Q{\uparrow}_S + Q{\downarrow}_L - Q{\uparrow}_L + Q_H + Q_E + Q_C \quad (1)$$
in which $Q{\downarrow}_S$ and $Q{\uparrow}_S$ are the incoming and reflected short-wave radiation, $Q{\downarrow}_L$ and $Q{\uparrow}_L$ the incoming and outgoing long-wave radiation, Q_H and Q_E the turbulent fluxes of sensible and latent heat, Q_C the subsurface conductive heat flux, and heat transport by precipitation and runoff are taken to be negligible.' The sentence in l.6-8 of p.8365 can then be deleted. Units are stated in Table 5, so there is no need to state them here, and the definition of albedo can be left until p.8366, l.9, where reference is made to it as an indicator of seasonal transition from snow to ice surface.
Revised as suggested, this does save a couple of lines. I retain the units as these have been specifically noted here at the request of the Editor.

p.8365, l.8: 'one-dimensional' rather than '1d'
Revised as suggested.

p.8365, l.14-p.8366, l.14: In fact, this is a standard bulk transfer method, best stated simply as
$$Q_H = r_a c_{pa} C_H\, v(q_a(z) - q_o)$$
$$Q_E = r_a L_s/v C_E\, v(q_v(z) - q_o) \quad (3)$$

where $CH/E = k2/\{[\ln(z/zo) + F][\ln(z/zoH) + F]\}$ and F is the stability correction, assuming similarity. While I appreciate the theoretical purity of defining qa at zoH and qv at zoE, surface values, qo and qo are used in practice because they are assumable for melting snow and ice, and so should be stated here. I would also recommend the use of T notation rather than potential temperature notation.

This is true and I was aware of this, but I guess that I also appreciate the theoretical purity of Eq. (3), and it leaves open the opportunity to directly explore stability corrections or true profile-based approaches, e.g. with T and q measured at multiple levels. I retain this, but have added a discussion and a presentation of the equivalent bulk transport equations to point out that this is equivalent, along the lines of the reviewer suggestion. See ll. 309-323. I also revert to temperature rather than potential temperature.

Monin-Obukov theory works well for stability corrections to the bulk transfer approach over melting snow and ice (e.g., Munro (2004)), but Oerlemans (2000) achieved closure simply by tuning the CH/E value directly, without reference to F or zo. So it is suitable to tune CH through zo selection alone as the author does here for Table 2 because the effect of stability correction over a melting surface is to reduce the turbulent fluxes to a fairly consistent 80 percent of their neutral values. This implies underestimation of zo due to the fact that F is not included, but probably not to any significant degree. Alternatively, Klok and Oerlemans (2002) used combined geostrophic and katabatic transfer coefficients but I don't think that this would work for a small glacier like the Haig, so the 'tuned' bulk transfer procedure used here is as well as one can do.

This is a nice discussion and I would greatly enjoy further dialogue here. Again, the reviewer is correct and nicely describes the different approaches to parameterization here, and their equivalence. I don't add this full discussion to the manuscript, but note clearly (I hope) the way that my tuning of roughness values absorbs these effects. I think there could be some fruitful debate on MO theory, and mechanical vs. thermal stability of the glacier boundary layer, but this is probably not the place. Rather than wade into this, I removed the discussion of whether stability corrections are appropriate or not – this is a distraction, as I don't analyze this one way or another and stability corrections are probably implicit in my tuned roughness values, as the reviewer points out.

p.8368, l.17: '...derived from 2005 Aster imagery...' My experience of working with Aster imagery is that it provides more local spatial variability than is obtainable from digital national topographic map data, but that absolute elevation across the DEM can be off by more than 50 m, so there was the need in my case to tie it in to a local benchmark.
This is interesting but I have not found this problem. When I compare ASTER grid cells to several tie points on Haig Glacier (from differential GPS surveys of our mass balance and meteorological stations), I have found these to be within 10 m (point vs. grid cell comparison), with no consistent bias.

p.8368, l.18-19: 'Potential direct solar radiation....' This needs some expansion so that the reader can better identify it with Oke (1987). The best expression for this appears on p. 345 of Oke, which states $Si = IoYa\ m$, where Si is direct radiation at normal incidence, Io the solar constant and Ya is transmissivity adjusted for air mass number, m, where m can be omitted if 0.78 is a bulk daily value. Then, turning to the notation used in this paper, $Qsf = Si$ cosq, where q is the angle between the normal to the slope (at angle f ?) and the solar beam, as stated in Eq. (A1.6) of Oke. Presumably the sensors at the FFAWS and GAWS are horizontal, so sensor q is the solar zenith angle and Qsf plus diffuse (say, qd) fits observations. Otherwise, Qsf is a spatially variable quantity to use with qd, which may itself vary spatially according to Eq.(A1.14) of Oke (not stated if this is the case here). Also, topographic shading is noted among the items in parentheses in line 16 above, but not mentioned further, thus leaving the reader unsure as to what was done in this regard. A few additional sentences on the incorporation of topography, with suitable references (such as Hock and Holmgren, 2005; Klok and Oerlemans, 2002?), would clarify matters for the reader having to restate generally used equations.

Apologies that this was unclear. The treatment of the solar radiation model has been revised and slightly expanded, ll. Citations have been added as appropriate, as this treatment is following past studies for the treatment of direct and diffuse radiation, with local (GAWS) optimization for the transmissivity.

p.8368, l.19-20:'...set to a constant 20%.' Twenty percent of what? Taking it to be 20% of 0.78 (i.e. Si) would imply a downward scattering coefficient of ~0.16 which seems reasonable for this environment.
In fact it is assumed to be 20% of the direct solar radiation (after Arnold et al., 1996), but I guess this is equivalent to 20% of Si cosZ, for zenith angle Z, so a downward scattering coefficient closer to 0.12 in the summer months. This point is clarified in the rewritten and expanded explanation of the solar radiation model, ll.333-361. This is relatively conventional treatment, but it is true that the expanded text is more self-contained and clear with respect to the solar radiation model.

p.8369, l.7-15: Another way to state Eq. (4) is $QL=easTa4$; $ea = aev + bev/es$, where es is saturation vapour pressure, thus making it consistent with the style of Table A2.2 of Oke (1987). Fair suggestion, these are equivalent and I am not attached to either formulation. I have shifted to this notation.

I am curious to know whether 'locally calibrated' a and b are one set of values throughout and what those values are because that would allow comparison with other schemes that employ vapour pressure, such as the first two that are listed in Table A2.2. Also, does the sky clearness index play a role in estimating QL when QS is available but QL is not?

Unfortunately I cannot expand too much on this empirical formulation and its development, as this is the focus of a separately-submitted manuscript (Ebrahimi and Marshall, submitted to JGR, July 2014). This manuscript is still in review, so I am not sure it is appropriate to cite this work here. I miswrote the form of the expression: it is epsa = a + bev/es + cev. The empirical

parameters from this work, *a,b* and *c*, are now included in Table 2 and these are fixed in time and space. This is now stated in the text and this has been rewritten for clarity, ll.366-373. In Ebrahimi and Marshall (submitted) we test whether the sky clearness index is a useful predictor of QL, and it certainly is (as found by others). However, it is highly correlated with RH and RH proves to be a stronger independent variable than clearness index in multivariate regressions. Hence, the bivariate relation with QL = f(ev, RH) is our recommended model. I regret that I cannot provide more details here, but this is discussed at length in our submitted manuscript.

p.8373, l.2-4: 'The larger differences...' Because warming applies to all snow free areas around the glacier, another interpretation is to see this as the effect of glacier cooling on the overlying air mass and the fact that the cooling effect doesn't extend much beyond the glacier boundary. JAS is the stand out period in this regard due to snow persistence through June, as stated in 4.2, so perhaps this period should be the centre of attention rather than JJA, especially as JAS also seems to be the primary ice melt period.
Quite true – text revised and the value for JAS is now reported.

p.8373, l.12: 'snow years' rather than 'snows years'
revised

p.8374, l.5-8: One other thing to note here is that the net short-wave part of $Q*$ for JJA, which I make out to be 95 Wm-2, is mostly comparable to Q_N, while the net long-wave part, -32 Wm-2 is substantially off-set by Q_H less Q_E, so it is crucial to have a good model of the glacier shortwave radiation regime. In fact the comparisons are closer in Table 5, where a $Q{\downarrow}S(1-a)$ value of ~84 Wm-2 slightly exceeds a Q_N value of 81 Wm-2 and Q_{Lnet}, -26 Wm-2, is mostly offset by $Q_H + Q_E$ = 22 Wm-2.
This is true for the means, though not necessarily so on shorter time intervals. Certainly though, shortwave radiation and albedo are the critical components to get right for energy balance modelling.

p.8374, l.17:'...May snowpack intializations...' Are these done by interpolating across the GIS between field sampling points, or perhaps by using an altitude relationship based on field measurements? Table 1 values indicates altitude dependency, while Fig. 1d suggests variation across the widths of altitude zones such as occurs at South Cascade Glacier.
This is now discussed in Sections 2.1 and 4.1, as the other reviewer also wished to know more details here. An altitude relation, bw(z), is adopted, based on field measurements, with mapping of bw(z) onto bw(x,y).

p.8376, l.14: '...albedo-influenced impact on summer melt.' Consider replacing this with '... melt reduction due to albedo rise.' to specify the impact.
Revised as suggested.

p.8377, l.19: Delete 'but'.
Revised

p.8377, l.25-27: 'Periods of high overnight flows reflect...' They could also reflect runoff delay from storage, as noted by the author in relation to Fig. 10. Further to Fig. 10, I am surprised that the author did not include a linear reservoir in the runoff model, such as described in Hannah and Gurnell (2001) because it is not difficult to do.

Indeed we are interested in this and are developing such models, but they are the subject of other studies and we have kept them out of this, in an attempt to keep the focus on monthly and seasonal discharge. The daily and diurnal patterns are very interesting and we will certainly investigate this further elsewhere.

p.8382, l.17: '... 7:1 upstream of Calgary and 15:1 over the Bow Basin.' Should these be stated the other way around? I find them difficult to reconcile with annual flow percentages stated in l. 24-25 below.

These are correct, although perhaps the reviewer's confusion suggests that this is not an intuitive way to discuss this. This is an attempt to think of specific runoff from the landscape, something that is often thought of as P-E in nonglacial environments. Haig Glacier is contributing 2350 mm of runoff per year, vs. an average of 320 mm from the Bow River basin upstream of Calgary (7:1) and 160 mm for the entire Bow River basin (15:1) (rounding off).

p.8383, l.12: 'channelized, draining' rather than 'channelized and draining'

Revised as suggested.

Table 1: Replace 'AWS' with 'GAWS'.

Revised.

Table 2: Use left justification in the units column.

Done in submitted text.

Figure 1: Delete a and b panels.

Figure revised.

Figure 3: I suggest using line plots to avoid the visual impression of stacked bar graphs.

Figure revised.

Figure 5: A line plot may be better for Fig. 5a as well. Also 'net energy' rather than 'net radiation' in Yaxis caption of Fig. 5b and 'QN' rather than 'QN' in the figure caption.

Figure revised.