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

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:
$Q_N = Q{\downarrow}_S - Q{\uparrow}_S + Q{\downarrow}_L - Q{\uparrow}_L + Q_H + Q_E + Q_C$ (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
$Q_H = \rho_a c_{pa} C_H\, v(q_a(z) - q_o)$
$Q_E = \rho_a L_{s/v} C_E\, v(q_v(z) - q_o)$ (3)

where $C_{H/E} = k^2/\{[\ln(z/z_o) + F][\ln(z/z_{oH}) + F]\}$ and F is the stability correction, assuming similarity. While I appreciate the theoretical purity of defining q_a at z_{oH} and q_v at z_{oE}, surface values, q_o and q_o 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 $C_{H/E}$ value directly, without reference to F or z_o. So it is suitable to tune C_H through z_o 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 z_o 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 *S*i= *I*oYa m, where *S*i is direct radiation at normal incidence, *I*o 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, *Q*sf = *S*i 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 *Q*sf plus diffuse (say, *q*d) fits observations. Otherwise, *Q*sf is a spatially variable quantity to use with *q*d, 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. *S*i) 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 *S*i 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 *Q*L=eas*T*a4; ea = *a*ev + *b*ev/*e*s, where *e*s 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 *Q*L when *Q*S is available but *Q*L 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*↓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 '*Q*N' rather than 'QN' in the figure caption.
Figure revised.