Interactive comment on "Surface Energy Balance Sensitivity to Meteorological Variability on Haig Glacier, Canadian Rocky Mountains" by S. Ebrahimi and S. J. Marshall

Anonymous Referee #1

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General Comments

The manuscript explores the sensitivity of surface energy balance components to summer climate perturbations for site over a small mountain glacier in the Canadian Rocky Mountains. Theoretical sensitivity is calculated using mean summer conditions, while empirical sensitivity is established using daily variability from 11 years of in-situ data. The paper also presents a reconstruction of summer melt from reanalysis data for the later part of the 20th Century. The paper is generally well written with well-presented figures and a logical progression through the results.

However, there are significant shortcomings in the methods that limit the usefulness of the results in address the key questions posed. In particular, it is well established that feedbacks are important mechanisms in determining glacier sensitivity to climate, in particular those between air temperature, precipitation and albedo (Oerlemans and Fortuin, 1992). For this reason, models assessing the sensitivity of melt (or mass balance) to climate perturbations require, 1. Driving data that covers the full range of meteorological conditions through multiple seasons, 2. A model that includes formulations for surface energy balance components that allow for important feedbacks (i.e. dynamic albedo, variation of incoming longwave with air temperature and humidity, dynamic surface temperature to include variations in refreezing/sub-surface conduction, 3. Study periods that include the full season to include air temperature/ precipitation /albedo feedbacks.

The authors reflect on most of these points throughout the results/discussion, but fail to adequately address them in the methods chosen. This undermines the results and ultimately reduces the interpretations that can be made from the data. The use of theoretical sensitivity based on mean summer conditions does not meet the criteria above and is subject to many assumptions implicit in the formulae used. It may provide an efficient way to assess the sensitivity over a large number of glaciers (and elevations on each glacier), but it would have to be carefully compared to the sensitivity assessed using realistic meteorological forcing across the full season over a large number of glaciers. Similarly, the use of reanalysis data perturbations could provide a useful method to derive sensitivity, again if the method could be shown to work for a number of glaciers in a variety of geographic settings. Unfortunately, the results of the NARR reanalysis driven surface energy balance conflict with the in-situ data here, so little can be interpreted from the seemingly accidental good model performance.

If the authors can work to robustly test their methods at a number of sites, and carefully redefine the focus of the work as presenting a new method for efficiently assessing sensitivity then it may be acceptable. If the authors wish to remain focused on the climate sensitivity of this particular glacier, then they need to employ methods appropriate to the task and put their results more carefully in the context of previous efforts to understand climate sensitivity.

Thanks to the reviewer for this insightful summary, and for pointing out here and below some of the limitations in our approach and analysis. We acknowledge that most of the reviewer's concerns are valid and we have done considerable extra work to address some of the main limitations in our study. Specific points are discussed in detail below.

Concerning the main points here: we agree that it would be valuable to examine several different sites and glacio-climatic environments, and that was indeed our original idea within the PhD research of S. Ebrahimi – to examine glacier sensitivity to meteorological variability in different regions. This still needs to be done, but as always happens, we found that it was already relatively rich and involved to perform this analysis thoroughly at one site. Our particular glacier is small and is not of global interest, but the glacier and the climatic regime are typical of mid-latitude mountain glaciers in e.g. the Rockies or the Alps, and the general findings are relevant to these environments.

As the reviewer points out, sensitivity analysis is not new. However, most prior studies focus on temperature and precipitation (appropriately so, these are the two most important variables for mountain glacier mass balance). We attempt to present a detailed sensitivity analysis for the full array of meteorological conditions that affect surface energy balance, both with and without feedbacks. One of our objectives is to systematically explore and document the magnitude of different feedbacks – we certainly recognize that these are essential in understanding glacier response to climate variability. The study takes advantage of an 11-year in-situ dataset that permits some exploration of interannual variability (requirement 1 above). We recognize that comparable datasets are available from a few other sites that would allow us to extend this work and its value, but this would be a different contribution involving a broader network of collaborators.

To keep this manuscript focused, we propose instead to remain with our original goal of an in-depth analysis at this one site, but we take seriously the reviewers' concerns about the limitations of our methodology and analysis. Specifically, we have: (i) added a subsurface temperature and drainage/refreezing model to allow free determination of subsurface heat flux and surface temperature, which feeds into the sensible heat flux and outgoing longwave radiation; (ii) extended to year-round simulations, though our focus remains on the summer melt season (MJJAS and JJA); (iii) better described aspects of the precipitation, albedo and humidity feedbacks and effects, which were there already but perhaps not properly described and explored; and (iv) better couched our methods and results in the context of previous studies. We believe that the modelling approach addresses requirements (2) and (3) above and is appropriate to the objectives of our study, and we thank the reviewer for pushing us on this. Our response has taken some months because of the model development and testing that were needed to improve this study.

Based on the two reviewers' comments, the reanalysis-based melt reconstructions have now been discussed differently in the manuscript, with this section reduced by about 50%. In the original manuscript, this section represented an application of the energy balance/melt model more than an extension of the sensitivity analysis, wandering into questions of mass balance reconstructions and trends. While this topic is certainly of interest, it is not relevant to the rest of the manuscript and so it was distracting from the focus. We now restrict the NARR discussion to an exploration of energy balance (summer mass balance) sensitivities, in line with the rest of the paper.

Specific Comments

Ln 27 – The abstract needs to be clearly state what the results of NARR analysis indicated.

Abstract revised.

Ln 40 – Interesting choice of the words 'banal' and 'trivial'. Perhaps these apply to the general public but likely not the readers of the current journal. Please revise.

Rewritten.

Ln 45 – The introduction needs to present a more thorough review of the atmospheric controls on glacier mass balance, in particular the link between air temperature and mass balance (melt) on extratropical glaciers discussed in papers such as Oerlemans (2005) and Sicart et al. (2008) and references therein.

Introduction rewritten to better describe past studies on this question, atmospheric controls on mass balance, as well as previous studies using sensitivity analyses.

Ln 66 – "capture the impact of shifts" perhaps add "in other climate variables such as".

Revised as suggested.

Ln 75 – while perhaps not commonplace, surface energy balance – mass balance models have been used extensively to investigate glacier-climate interactions and sensitivity (Gerbaux et al., 2005; Greuell and Smeets, 2001; Klok and Oerlemans, 2004; Mölg et al., 2008). Please revise.

Agreed, more commonplace than we conveyed. Several studies along these lines will be included in the revised submission.

Ln 85-90 – The introduction needs to more clearly define what is being examined the sensitivity of surface energy balance components, or melt, or mass balance? and over what time period – the sensitivity of melt to summer meteorology or annual climatology? The results should then align with the objective defined. Certainly interannual variations in air temperature will impact the fraction of rain vs snow and thus the winter accumulation and from this the albedo and melt through the timing of the snow-ice transition. If the authors wish to examine the sensitivity of mass balance or melt to climate change it is imperative that modelling is conducted over full seasons.

Apologies for our lack of clarity here. This will be rewritten. We have not aimed to examine the sensitivity of annual mass balance, rather just the summer (melt) season surface energy balance and summer melt. That said, we of course agree that summer energy balance and melt will be sensitive to the winter snowpack. This is implicitly included in our model/observations for the study period, 2002-2012, as we initialize each summer melt season with the observed May snow depth (mm w.e., based on winter mass balance surveys that are carried out each May). Hence we do not model the snow accumulation through the winter, but observed interannual variability in the snow accumulation is included as an initial condition for the summer melt season simulations. Interannual variability in measured and modelled summer albedo therefore includes this influence, although we have not isolated or examined it. This will be discussed in the revised manuscript.

Unfortunately, we do not have a good model or empirical understanding of winter snow accumulation sensitivity to meteorological conditions at this glacier. We have 15 years of winter mass balance data from this site, but that is limited when it comes to statistical modelling, and winter mass balance does not have a significant correlation with simple metrics, such as mean winter temperature. It is much more synoptically governed, e.g., responding to variability in Pacific storm tracks. Hence we do not include a direct model of winter or annual mass balance here, or of the sensitivity of winter mass balance to climate variability. We certainly agree that this is necessary in model-based studies over longer time periods (e.g. climate change studies), where temperature-dependent processes such as rain/snow fractionation need to be included in model-derived winter snow accumulation.

Our focus is on the summer energy and mass balance, and we agree that summer melt is sensitive to the winter snowpack, through its influence on albedo. Hence we introduce a new sensitivity test to explore the effects of different winter snow accumulation, bw, on summer energy balance and melt. A new Figure 7 presents these results, and we have a broader discussion of these influences on the summer melt season. In the revised NARR work, we also include a brief analysis of the effects of winter mass balance variability (as modelled in a simple way) on summer melt.

Ln 143 – It is contradictory to state a sophisticated model is 'needed' if you go on to use a parameterization that does not perform these calculations. Perhaps it would be accurate to state that one needs to take into account the profile of lower tropospheric water vapour, cloud and temperature.

Clarified as suggested; we mean only to emphasize that ours is a simplistic parameterization of something that is complicated to calculate rigorously. But the parameterization still has some skill, vs. for instance reanalysis-based estimates of incoming longwave radiation or the null hypothesis of assuming the mean value.

Ln 206-214 – This paragraph appears to be out of place. Please move to introduction.

Removed from here, with some of this content retained in the revised introduction.

Ln 240 – It is ambiguous how the diurnal cycle of is parameterized. Please explain.

This detail is now added in section 2, which describes the model. We apologize for the lack of clarity. Our methodology is simple, so we were not sure this warranted the space, but it is not documented elsewhere and it is important to describe the methods plainly and explicitly. Where we use a 'directly observed' surface energy balance, we drive the energy balance model with observed 30-minute data (including measured albedo and outgoing longwave radiation). Where we do sensitivity tests or run the model with other meteorological input, such as from climate models, we follow the following procedure, which allows for internal (e.g. albedo) feedbacks:

(i) we input the daily mean variables for all meteorological fields, as well as daily minimum and maximum temperature;

(ii) a diurnal temperature cycle is parameterized as a cosine wave with a lag to give min/max temperature at 04:00 and 16:00 (as per local observations), with an amplitude $A_T = (T_{max} - T_{min})/2$;

(iii) a diurnal cycle for incoming shortwave radiation is parameterized as a half-cosine wave (values above 0), with a period $T(d) = 2h_s(d)$, where d is the day of year and h_s is the number of hours of sunlight on day d. Sunlight hours can be calculated as a function of latitude and day of year (see the revised text). A lag is specified to give peak shortwave radiation at local noon, and the amplitude of the cosine wave is specified from $A_{SW} = \pi\, Q_{Sd}{}^{\downarrow}/2$, where $Q_{Sd}{}^{\downarrow}$ is the mean daily incoming shortwave radiation. This last relation is derived from integrating the area under the cosine wave and equating it to the average daily value. This treatment implicitly includes daily cloud effects that will reduce incoming shortwave radiation (via $Q_{Sd}{}^{\downarrow}$), but distributed evenly through the day; this neglects any systematic tendency for e.g. afternoon vs morning clouds. For simplicity, we also neglect the effect of zenith angle on atmospheric transmittance (i.e., lower transmittance for larger atmospheric path lengths in the morning and late afternoon), although this could be built into a more refined model.

(iv) we assume that wind, incoming longwave radiation, air pressure, and specific humidity are constant through the day, held to the mean daily value.

(v) albedo is modelled on a daily basis, decreasing as a function of melting (cumulative PDD) or increasing in the event of summer snow falls (see the text);

(vi) relative humidity has a diurnal cycle following temperature, which impacts incoming longwave radiation where we parameterize this from near-surface conditions;

(vii) subsurface and surface temperature (T_s) and Q_C are modelled with 10-minute to one-hour time steps (chosen for stability of the temperature solution), and T_s is used in the calculation of outgoing longwave radiation, sensible heat flux, and latent heat flux (via q_s). The model is run year-round;

Taken together, this gives an estimate of 10-minute to one-hour melting,

(viii) meltwater percolates and either refreezes or runs off based on a simple drainage model, described briefly in the text. This is part of the snowpack model used to calculate T_s and Q_C.

(ix) the snowpack depth and surface albedo are updated and the integration continues through the year.

(x) winter snow accumulation is not directly modelled, but winter mass balance (the May snowpack) is treated as an 'initial condition' for the summer melt model. It is set to measured values of bw, which are from winter mass balance observations that are carried out each May, including a snow pit at the AWS site. For purposes of the subsurface temperature model, snow accumulates linearly through the winter (October to May) to reach the annual observed value of bw.

Ln 261 – Theoretical sensitivity – As discussed in the general comments, a robust assessment of sensitivity needs to consider the full range of meteorological variation. The results of the theoretical and empirical sensitivity differ in important ways and thus, the theoretical sensitivity cannot be said to add anything beyond the standard of modelling the full season. Either this section needs to be removed, or developed further into a distinct methodology that is validated at a number of sites.

We have retained this section, but rewritten it in places and added some analysis to take it a bit further and permit some direct comparisons with the empirical/numerical model. One thing that it shows, for instance, is the strength of different feedbacks relative to the idealized situation where only one variable changes. It also provides a basis for thinking about meteorological perturbations, e.g. if temperature increases, do we assume that specific humidity stays the same, such that RH will drop, or do we assume that qv will increase, to maintain constant RH? This is introduced in the theoretical sensitivities, and then used as two 'end members' in the empirical model. This is also true for estimation of atmospheric radiation feedbacks that can be roughly parameterized from the humidity – it is introduced in the theoretical discussion and then applied in the model.

Ln 468 - Please explain why daily time steps were used when the computational cost of hourly sub-hourly steps is not great? Much important information is lost at a daily time step, even with a parameterized diurnal cycle and further discussion of the effects on the results is warranted.

This is now discussed more clearly. In fact, we use sub-daily time steps (right now, 10- or 30-minute), and the reference energy fluxes are based on the 30-minute AWS data. But for a more flexible model that can be driven by climate model reanalyses or projections, for instance, we developed the model to work with daily inputs, along with parameterizations of the diurnal cycle (see above) and sub-daily time steps to capture the important diurnal processes.

Ln 478 – Please state what fraction of data are missing/gap filled, in particular the incoming longwave data.

This will be added to Table 1. It depends on the variable of interest. For most AWS variables, such as temperature, data coverage is 63% annually for the period 2002-2012 (2519 of 4018 days). 90% for the core summer months, JJA (909 of 1012 days), and 86% for MJJAS (1441 of 1683 days). The longwave radiation sensor was installed in July 2003 so there is more missing data. Coverage is as follows: annual - 46% (1835/4016 days); JJA – 76% (773/1023 days); MJJAS – 70% (1184/1683 days).

Ln 492 – The feedbacks need to be clearly explained here, as equation 14 indicates there will be positive feedbacks that will enhance the variation of incoming longwave with humidity.

Atmospheric temperature increases enhance the longwave radiation. However, the humidity has a reverse relationship with the temperature change (Eq. 14). As a result, a good amount of temperature increase is cancelled with the response of vapour pressure. We have expanded the discussion on this.

Ln 517 – It is essential that incoming longwave vary with humidity for an assessment of sensitivity to be robust. By using measured and parameterized data this becomes ambiguous and parameterized data should be used exclusively.

Agreed, we now use parameterized longwave radiation as the default in the model, and we only use measured LW fluxes when we wish to control for this.

Ln 517 – You have the opportunity to include the effects of humidity on incoming shortwave radiation (through equation 9). As you note, this can overwhelm influence on incoming longwave radiation (Ln 319). The inclusion of this effect would be novel application of the empirical model.

Agreed again, we had explored this in the theoretical sensitivity but not in the empirical model. It is now included as the default treatment: atmospheric clearness τ changes with the humidity.

Ln 524 – Your results indicate the feedbacks are important (Ln 541) and your conclusions should echo this more strongly.

We had thought that we had emphasized this in the conclusions, but will state this more clearly and strongly.

Ln 560 – This assumption is likely to be incorrect and the effect of subsurface heat fluxes needs to be considered (e.g. Pellicciotti et al. (2009).

Now rectified through a complete year-round subsurface model, see above. In fact, Q_C is minor in the summer months here, on average, but the surface temperature does drop below 0°C frequently, particularly in May and September. This is now captured.

Ln 574-576 – Further explanation of this method is needed i.e. how did you treat variations in moisture - as changes in qv or in RH? If the former, then perhaps you will overestimate the actual variation as qv variations at lower altitudes will be larger.

This is an interesting point, we had not thought of that. We do use the specific humidity from NARR, which originates from a grid cell with an elevation of 2216 m. This is about 450 m below the glacier AWS, so it is not terrible, but there will potentially be larger variations in qv, incoming LW, etc., from this altitude effect. Perhaps even larger an effect will be the temperature variability in summer months over a non-glacierized surface, which can warm up above 0°C. We will add a brief discussion of these sources of uncertainty.

Ln 586 – This statement seems to contradict the previous statement that most important radiative inputs are not well correlated on an inter-annual basis and that the variance of the shortwave does not correspond with the in-situ. As there is distinct seasonal variations in air temperature and solar radiation, these variables are heavily auto-correlated and a more meaningful correlation would remove the seasonal trend before correlating variables between NARR and in-situ data.

We no longer discuss this.

Ln 629 – As biases in NARR results only happen to cancel and thus produce correct estimates of melt energy, these results cannot be considered robust enough to provide a meaningful interpretation of the inter-annual variations in the surface energy fluxes. Either the interpretations need to be carefully explained in this light, or further work is needed to demonstrate acceptable model skill.

We have changed the focus and presentation of the NARR results, and believe that the new discussion is more relevant to the manuscript and grounded on these points. Because of the large biases in NARR and the questionable skill in the annual energy balance and melt reconstructions, vs. the observations, we no longer present the NARR-driven simulations as mass balance reconstructions. We actually think this may be possible, through more work to assess model skill, but here we restrict the analysis to the covariance of NARR-driven net energy fluxes (summer melt) and different meteorological variables. Our aim is to see how the theoretical and empirical sensitivities hold up when multiple variables are perturbed at once, in a meteorologically consistent way. The means and variances of the NARR-based energy fluxes (Table 5) are close enough to the observed values to permit this comparison, with the important exception of the shortwave radiation. This is discussed.

Ln 654 – The approach presented in this paper has already been fairly well established in the literature (see comment for Ln 75) and so some additional novelty needs to be displayed here.

We have rewritten to try and better address what is new in our approach.

Ln 763-765 – Further explanation of the differences between theoretical and empirical sensitivities is needed.

Agreed, we have added this to the discussion, as well as the NARR-derived sensitivities.

Ln 770-771 – The trends in energy fluxes need to be more closely tied into the results of the sensitivity study.

This discussion now removed, cf. Ln 629.

Many thanks for the detailed and thought-provoking review. Whether the revised manuscript is acceptable or not, it is certainly improved and our work going forward has benefitted from many of these ideas.

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