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

Received and published: 25 February 2016

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.

References

Gerbaux, M., Genthon, C., Etchevers, P., Vincent, C., and Dedieu, J. P.: Surface mass balance of glaciers in the French Alps: distributed modeling and sensitivity to climate change, Journal of Glaciology, 51, 561-572, 2005.

Greuell, W. and Smeets, C. J. P. P.: Variations with elevation in the surface energy balance

on the Pasterze (Austria), Journal of Geophysical Research, 106, 31717-31727, 2001. Klok, E. J. and Oerlemans, J.: Modelled climate sensitivity of the mass balance of Morteratschgletscher and its dependence on albedo parameterization, International Journal of Climatology, 24, 231-245, 2004.

Mölg, T., Cullen, N. J., Hardy, D. R., Kaser, G., and Klok, L.: Mass balance of a slope glacier on Kilimanjaro and its sensitivity to climate, International Journal of Climatology, 28, 881-892, 2008.

Oerlemans, J.: Extracting a climate signal from 169 glacier records, Science, 308, 675-677, 2005.

Oerlemans, J. and Fortuin, J. P. F.: Sensitivity of glaciers and small ice caps to greenhouse warming, Science (New York, N.Y.), 258, 115-117, 1992.

Pellicciotti, F., Carenzo, M., Helbing, J., Rimkus, S., and Burlando, P.: On the role of subsurface heat conduction in glacier energy-balance modelling, Annals of Glaciology, 50, 16-24, 2009.

Sicart, J. E., Hock, R., and Six, D.: Glacier melt, air temperature, and energy balance in different climates: The Bolivian Tropics, the French Alps, and northern Sweden, Journal of Geophysical Research, 113, 2008.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-6, 2016.

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 #2

Received and published: 1 March 2016

General comments

In this paper, theoretical considerations as well as an energy balance model are employed to assess the surface energy balance sensitivity to variations in meteorological variables. The methods are applied at an automatic weather station (AWS) site on a mid-latitude glacier in the Canadian Rocky Mountains. In addition to the in situ AWS observations over the period 2002–2012, meteorological data from a reanalysis product (1979–2014) are used to force the model. Only the main melt season (June-August or May-September) is considered. The paper reads well and is written in good English. However, the methods used are not always described in enough detail, in particular regarding the energy balance model. Some model elements are not introduced at all, others are mentioned at a too late point in the manuscript. See the specific comments below for an overview.

Our apologies for the poor presentation of methods. We have completely rewritten this and moved it up front, to Section 2. It has added some length to the manuscript, but we hope that our approach and assumptions are now clear.

Apart from model parts not being described, I do not think the model and approach used are suitable for the sensitivity analysis performed in this paper. The surface energy balance contains important feedback mechanisms, which are pointed out by the authors at places in the manuscript. Although they account for albedo changes associated with increased surface melt, they do not seem to include the opposite effect of summer snowfalls on the albedo. More importantly, they do not calculate surface temperature internally in the model, while this variable is easily affected by changing atmospheric conditions. In its turn, it changes the outgoing longwave radiation and the turbulent fluxes. The authors do mention that surface temperature is generally at the melting point in the summer months, but not in the early and late melt season. Still, most of their results are presented for the entire melt season.

These are good points and we have a mixed response. We do (and did) have a parameterization of summer snow events (see Marshall, 2014), but we failed to explain it properly here. Summer snowfall is treated as a stochastic variable with a specified number of precipitation distributed over the summer. The amount of each summer snowfall is also random, between 1 and 10 mm w.e, and we use a temperature-dependent fractionation between rain and snow. This is now explained in Section 2. However, we did not have a subsurface/surface temperature model in the original submission. We have now added this to the model, and outgoing longwave radiation and the turbulent fluxes are calculated from the temperature in the upper (10-cm) surface layer. The subsurface model is run year-round at 10-minute to 1-hour time steps, solving temperature and with a simple treatment of meltwater drainage and refreezing in the upper 10 m. This is also described in Section 2.

The same applies to the theoretical derivations of the energy flux sensitivity, they also do not take changes in surface temperature into account. However, here the main results are presented for the months June-August only. This theoretical approach does present a simple method to estimate changes in the surface energy balance resulting from variability in the meteorological conditions. The results compare well to the model results, but not for all variables, suggesting some feedbacks are overlooked in the energy balance model. Whether this is a general method that can be transferred to other glaciers can only be established by similar applications on other glaciers with energy balance observations.

Here in the theoretical model it is not possible to account for changes in albedo or surface temperature through diurnal or seasonal cycles; it is meant to be a simple tool to provide a rough estimate of average summer energy balance sensitivities. We do restrict ourselves to JJA because of this important point about the assumption that the surface is at the melting point, which holds well in these months. We agree on the need to apply this approach to other glaciers and meteorological conditions to see if it is a useful way to assess glacier energy balance sensitivity. We do not add this to the current manuscript, but emphasize in the conclusions that is needed and potentially useful. If it does prove useful, we see that this framework as we lay it out, or variants on this, would be one of the main contributions of our paper.

Another major shortcoming of the energy balance model used is that incoming longwave radiation is taken from the measurements in the sensitivity analysis and not recalculated. As incoming longwave radiation is affected by both changes in air temperature and humidity (and cloudiness, here parameterized through relative humidity changes), the sensitivities are severely underestimated.

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 for comparison.

This is also revealed from the comparison with results from the theoretical approach. The model simulations with reanalysis input serve as an application of the 'perturbation' method presented. After reading the paper, I am still not sure what this method exactly is, but it is not as novel as the authors present it to be. I think the authors mean that the energy balance model is run with anomalies imposed on the 2002–2012 in situ conditions. But in fact, they are just forcing the model with a different set of (biascorrected) meteorological data. As the connection of this exercise to the sensitivity analysis presented before is rather weak, I doubt whether this is a valuable addition to the paper.

In revising the manuscript and rethinking this part of the work, we broadly agree with this criticism. Indeed, the reviewer understands exactly what we are doing, and it is really just forcing the model with bias-corrected meteorological data from NARR, a regional reanalysis. This is not new, although relatively few mountain glacier studies forced by climate model output use the full surface energy balance rather than PDD methods. But to the reviewer's point, we drifted off into historical energy and mass balance reconstructions, when this is not at all the point of the manuscript. We have rewritten and removed much of the NARR analysis, but keep some of this as a way to explore energy balance sensitivity to meteorological variations over a longer period, 36 years, vs. the 11-year observational record. More importantly, NARR meteorological variations are in combination (vs. one at a time), with a realistic level of interannual variability (vs. idealized sensitivity tests) and implicitly including meteorologically-consistent covariance of variables.

Thank you for these high-level comments: quite insightful and the manuscript is much improved through consideration of these criticisms.

Specific comments

72-76: These lines give the impression that it is not very common to perform sensitivity studies of the surface energy balance on glaciers. The sensitivity to changes in temperature and precipitation is however assessed in numerous studies, therefore I suggest to change the word 'Several' to something more appropriate. Sensitivities to other variables are indeed less often investigated, but there are more examples than the one given here (e.g. Oerlemans (1991) and Gerbaux et al. (2005)).

This is a fair comment, we did not do justice to the literature on this. Our approach is a bit different, but this idea has been explored much more than we discussed. To our embarrassment, we were actually unaware of the Gerbaux et al. paper, completely missed it. This is now added and discussed in the context of a largely rewritten introduction. We also add a few other papers, including some of the work of Oerlemans and colleagues, in a brief review. Most emphasis to date has been on precipitation and temperature sensitivity, but this provides a useful context for consideration of the broader energy balance sensitivity – which in the end, points mostly back to temperature. We took out the word 'Several' as recommended as part of this rewrite.

105: I wonder what the net energy flux QN actually represents, the authors need to give a better description. If it is positive, it generates surface melt and is equivalent to what is often called the melt energy in other studies. But can it also be negative? The net energy as presented here seems to represent a residual flux, that should remain close to zero if the surface is not melting. Is this the case and is it set to zero then? Otherwise, it means that important processes in the energy balance are missing. Much later in the manuscript, on lines 471-473, I read that negative values are associated with refreezing. I do not think this can be assumed that simply, refreezing requires a snow/ice model which seems not to be included here.

We have not explained this well enough, and hope that it is now clear in the revised manuscript. Net energy, QN, can be thought of as a residual, but really it is the energy surplus or deficit that attends the surface energy budget at any one time. If it is a surplus and the surface is at the melting point, then this is the melt energy. If snow/ice temperatures are sub-zero, this warms the system. But QN can also be negative, which drives either cooling or refreezing. The latter two processes are now modelled properly within the subsurface model. We explain this more explicitly in section 2 now.

171: This equation implies that $h > 30$ for all times, is this indeed the case? It would be neater to include a minimum condition, in case $h < 30$.

Good point, to be more general we now add this minimum condition as part of Eq. 9, to maintain physically bounded values in the parameterization. We did not record days with mean h below 30%, but they are of course possible in principle.

192-193: More detail is needed about the roughness length scales, as there are different ways to derive their values and treat/calculate them in the model. According to the cited Marshall (2014) paper, constant values were used for all three length scales with a predefined ratio between them (mentioned much later in line 326-327). Their values were obtained by closing the surface energy balance. This should be mentioned here as well. I also wonder whether values differ for snow and ice surfaces?

Revised and added in Section 2.

205: The paper does not mention how the subsurface conductive heat flux is calculated, is a vertical model used to keep track of snow/ice temperatures, densities and water content? Please add a few lines.

Now explained in Section 2 – a 10-m subsurface model is newly added.

212-214: I have the impression that QE is generally positive on mid-latitude glaciers during the melt season, or slightly negative. See for more examples the tables in Ohmura (2001) and Giesen et al. (2009).

This is mixed to our knowledge. Small positive latent heat fluxes have been reported for sites in the Alps (Greuell and Smeets, 2001; Klok and Oerlemans, 2002). Giesen et al (2009) looked at the surface energy balance over two glaciers (60 &61 N) in Norway that are more maritime influenced, and had more strongly positive latent heat fluxes (9 and 16 W/m^2). However, the results for latent heat flux in Table 2 in Ohmura (2001) indicate negative latent heat fluxes for a number of studies, and this generally reflects more dry, continental climates such as the Rockies. In our study, mean daily Q_E is negative 77% of the times from May to Sept and 66% of the time for JJA, and weakly negative overall.

231-233: Which percentage of the data needed to be gap-filled? Do you mean that factors are derived for months when data from both AWSs were available?

We have added two sentences to discuss the percentage of missing data, and clarified how we assign the factors. They are based on monthly values for all available joint data over the 11-year period. 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).

237-239: Is the mean daily value taken from the same day in other years?

Yes, exactly. So for May 10, for instance, we have 10 values from 2002-2012, but one year is missing at both weather stations; so we use the 10-year mean value from the glacier AWS to gap-fill here.

239-244: I am puzzled why the authors chose to use daily input data with imposed daily cycles instead of running the model at the resolution of the observations. There is a slight gain in efficiency, but at the cost of loosing important information to calculate the surface energy balance fluxes. Especially for the sensitivity of the surface energy balance, it is important to have enough detail. Perhaps the climate model output has a lower time resolution, but then a daily cycle can be imposed there. In any case, it would be better to provide details about the daily cycle here, where the first questions arise and not at the later point in the paper.

This is valid as well, of course, and we do our reference observationally-driven runs forced by the actual 30-minute AWS data. However, as we discuss now in the manuscript, we moved to daily forcing (with parameterized diurnal cycles for the temperature and shortwave radiation) for two main reasons: a) climate model and reanalysis forcing is typically only available 8x, 4x, or once daily, and we are looking ahead to a surface energy balance model that can be driven in this way, and b) some of our parameterizations and gap-filling strategies are better suited to daily values, such as our parameterization of incoming longwave radiation in Eq. (7). We looked at 30-minute parameterizations of this form, but they do not perform as well. This is a central part of our sensitivity tests, to approximate humidity feedbacks on incoming longwave radiation, so we are pushed towards daily inputs. That said, we do lose efficiency despite this, by running at 10-minute time steps for the subsurface temperature model (which takes Q_N as an upper boundary condition); so lacking efficiency in any case, this model could certainly be adapted for finer meteorological input forcing, if available.

324-325: Air density is also assumed not to vary with temperature changes. Instead of using 'independent of temperature', which is of course not true, it might be more accurate to use 'can be assumed constant for small temperature changes'.

Good point, revised as suggested.

382-389: With unit forcings, as is done here, the sensitivities to changes in the different variables cannot really be compared. Better compare the effects on the different energy fluxes only per variable and leave the comparison between variables for the standard deviation-based forcings later in the section.

Agreed, this paragraph was to highlight the same point you comment on. Our purpose here was to explain the conditions when the unit forcings are (and are not) likely on the glacier, in order to explain why we performed the 1σ of all perturbation in the end.

404: This subsection title is not well chosen, since the sensitivity of all energy balance fluxes to changes in meteorological variables is considered in this section. Net solar radiation is an energy flux itself and it is not the variable that is changed in this subsection. Instead, the effect of changes in top-of-the-atmosphere insolation, atmospheric transmissivity and surface albedo on the energy balance are the subject of this subsection. Please change the title accordingly.

Good point, well caught. We changed the subtitle to “Change in Net Shortwave Radiation”

471-473: As mentioned before, to get a good estimate of the amount of refreezing meltwater and the associated heat release, a vertical snow/ice model is needed. Here, refreezing occurs whenever air (not surface?) temperature is negative, regardless of the amount of available water. If the period before has been cold as well, there will not be any water present. Even if water is present following a melt event, there may not be enough to release the amount of heat following from Eq. (2). I therefore think this is not a good way to compute refreezing and would either neglect it altogether or use a proper subsurface model.

Agreed. A subsurface model has been added, including a (simple) model of meltwater drainage. It does not influence things much in JJA, but does improve our shoulder-season results especially in May where there are cooler temperatures and an effective snow aquifer.

476-478: I do not understand how sensitivity analysis can be done if measured longwave radiation is used. Incoming longwave radiation needs to be adapted for different temperature and humidity. If the authors first show (in a figure) that using Eqs. (6) and (7) gives good correspondence with measured incoming longwave radiation, then they can use these equations with new temperature and/or humidity. This would largely reduce the difference in sensitivities to temperature and humidity changes obtained from the theoretical approach and the energy balance model. Outgoing longwave radiation should also be allowed to change, unless the surface temperature is always at the melting point. But for negative air temperature anomalies, the surface temperature will often be lower as well.

The longwave fluxes are now allowed to change, based on the humidity anomalies and longwave parameterization (LW in) and the modelled surface temperature (LW out). Also, incoming shortwave is modified based on the clearness index parameterization. We still do some simulations without these feedbacks, to control for the magnitude of different fluxes and feedbacks, but our ‘default’ model allows these to change, in accord with these suggestions.

481-483: Many questions arose here, concerning the implementation of the changes in the energy balance model. Changes in air temperature will affect the fraction of precipitation falling as snow/rain, is this included in the model? Is snow depth tracked in the model to determine changes in the moments of ice (dis)appearance? How is albedo treated if the ice appears earlier than in the observations, is an ice albedo prescribed then?

The revised section 2 on methodology hopefully answers these questions. In short: changes in temperature will affect the rain/snow fraction in summer, but not the winter accumulation – this is assigned based on observed May snow accumulation (winter mass balance) for each year. Snow depth is tracked, along with snow albedo evolution and a shift to ice albedo when the snowpack has been removed.

486-488: The authors should make clear here which part of the year is used in the analysis. They mention that anomalies are applied to the entire year. But nowhere, except in the title of Fig. 2, it is mentioned that the analysis is performed over the months May-Sep.

Hopefully clear now. The simulations are year round, and melt is permitted year-round, but 99% of the melt occurs in the months of May-Sept and ~80% in JJA. For calculating and reporting mean energy fluxes and melt, we mostly report JJA, but also note MJJAS in places – explicitly noted in these case.

538-542: The albedo feedback has a smaller effect for negative temperature perturbations. Is this because increases of snowfall events are not included?

This is now discussed. It is due to a 'saturation' condition where cooling causes the snowpack to survive the summer melt season, i.e. with no transition to darker ice. Further cooling has less impact. Warming influences cause the snow to melt away sooner and this effect does not 'saturate' in the same way.

557-560: How representative is the assumption of a melting surface, this can easily be judged from the measurements. In Table 2, I see that especially in May, outgoing longwave radiation is considerably lower than 315 W m−2. Can you give the fraction of the time with a melting surface to the total time?

We have been able to do away with this assumption now, probably the largest improvement in the model. Now the surface is free to drop below 0C, and this feeds into the calculations of LW out, QH and QE. Because this is interesting, we added a new Fig. 3a that shows the annual cycle of Ts (daily mean values for 11 years). Indeed, surface temperatures drop below 0 degC on most nights through the summer (97% of March nights, and 71% of the time in JJA). Mean monthly values for Ts during the melt season are: May -2.8, June -0.9, July -0.4, Aug -0.6, Sept -1.6. So yes – assuming the surface is always at the melting point is not valid. That said, our results have not changed much with the subsurface/Ts model, perhaps because the melt energy (positive QN) is generally during the day, when Ts=0.

616-621: Why include the shoulder months in the analysis if they are not represented well in the model? Although it would still be better if the processes themselves would be included in the energy balance model.

We think including the shoulder months could give a better representation of the model's efficiency for different meteorological conditions. Also, since we are introducing this method to be used for different locations, the importance of these months can vary. Therefore, we report both JJA and MJJAS results. That said, it is more valid to report this now that Ts is being modelled. Our attempt to include the shoulder season in the initial model helped to illuminate the need for this subsurface model. And we would still say that our September results are not as good as we would like, and point to the need for a better model of the 'end of summer/start of winter' transition, heralded by the arrival of snow that persists. We discuss this as a place to invest in for future efforts.

694-695: Summer snow events also bring additional mass to the glacier, further reducing the net melt.

Yes, now commented on; it was included but not discussed.

757-758: Please be more specific about which feedbacks are actually included. Only the internally modelled snow-aging is described in lines 529-536, it is still not clear to me to what extent and how the snow/ice transition and snowfall events are included.

These are hopefully clear now in the expanded Section 2.

Table 6: In general, I think the manuscript contains a relatively large number of tables and a small number of figures. Especially this table contains too much information to serve a purpose and it also needs to be compared to another table. Please make the comparison easier, by visualizing the monthly energy balance fluxes for the in situ data, the NARR perturbed data (and optionally the NARR raw data) together in a figure.

This Table has been removed, along with this content. Our Figure/Table ratio is higher now, and NARR analyses are mostly just in Figures.

Figure 2: As longwave radiation is (not yet) allowed to change, the effect of net radiation corresponds to the effect of net shortwave radiation alone. Better present it this way and add a line for net longwave radiation, when it is also varied. I would also like to see a line for the summed effect on QN, which is especially illustrative for the opposite effects found for wind speed changes.

We now have lines for longwave and net radiation, also QN.

Figure 6: Why is albedo shown for JJA instead of MJJAS, as the other variables? I would like to see the net shortwave and net longwave radiation separately instead of net radiation, as these are treated individually throughout the manuscript. I do not think it is necessary to show both net energy and melt, because they are directly related.

This Figure has been removed.

Technical corrections

40: I would not consider the word 'banal' fit for scientific papers, please rephrase. Revised and removed.

55: 'reanalyses' Revised.

60: 'for snow and ice melt factors' Revised.

69: 'crucial to ablation on' Revised.

116: ’solar radiation that is reflected’ Revised.

123: _0 is used in Equation (3) instead of 0 Revised.

150: As Kwadacha Glacier is not the subject of this paper, better rewrite as ’At two study sites’ Revised.

158: ’ratio of potential direct to measured’ (or is measured radiation only direct radiation as well?) The measured radiation is the combined direct and diffused solar radiation.

159: Include a reference here, is it the paper mentioned in the next line?

The reference, Ebrahimi and Marshall (2015), has been added now.

186-187: Split into two sentences: ’ and q ... humidity. Measurements... levels, at the surface-air... and at height ... surface.’ Revised.

189-190: Reorder: ’We estimate Ts from an inversion of Eq. (5), using’ Revised.

193: ’can be’ Revised.

229: ’meteorological conditions’ This section is rewritten.

264: ’Warm summers generally cause’ Revised.

265: ’but the energy balance is sensitive to’ Revised.

286-287: ’of the response to a temperature change’ Revised.

305: Remove the spaces in 100 (1 00)? Revised.

332: Include the dot on m as in Eq. (2) Revised.

340: ’at the AWS site’ Revised.

392-393: Split into two sentences: 'Following Eq. (9),' Revised.

407: I wondered what was meant by 'solar variability' and found the answer in line 424-425, better move it here. Revised and rewritten.

415: Is QS0 equivalent to Q0 introduced in Eq. (3)? If yes, use the same notation, if no, clarify the difference. Revised.

445: 'last two lines' Revised.

495-496: Mention that results for simultaneous changes in temperature and humidity are not shown here. Revised.

497-500: These lines belong in the figure caption, not in the main text. Revised and rewritten.

500: 'Sensitivity to albedo changes over' n/a as this section is rewritten.

507: 'directly' This section is rewritten.

510: 'The sensitivities computed with/resulting from the surface...' Revised.

512-514: These may be advantages, but are these effects included in the model used here?

In rewriting we have tried to be more clear on what is and is not included; in general, there are advantages to the model in that it permits most feedbacks to be included, but they can also be 'turned off' to isolate and understand different influences.

528: 'induce' Revised.

558: What is the 'summer melt season'? May-Sep or Jul-Aug?

Both are examined, but in revisions we have tried to clarify to which we refer. MJJAS is the melt season, really, but JJA the core melt months.

609-610: The wording should make clear that these energy fluxes are not taken from the NARR reanalysis, but calculated with the energy balance model using NARR meteorological forcing. Further down, 'NARR-based' is used frequently, this is already better.

Revised thoroughly, throughout this section.

661: 'changes in most meteorological variables' Revised.

668: 'Increases' Revised.

669: 'through the sensible and latent heat and incoming' Revised.

692-693: 'fraction of time with surface temperatures at the melting point' Revised.

698: 'as in the simple experiments presented in this paper' Revised.

699: What is meant with 'everything', please be more specific here.

Revised. As the previous sentence starts with 'meteorological variables' we now revised the second sentence by referring to these variables.

726: 'balance' Revised.

747: 'allows for a' Revised.

771: Just write 'Net solar radiation', as longwave radiation is not allowed to change.

n/a now, as we have removed discussion of trends.

Table 1: Write out the definition of 'summer melt season' in the caption. Use SI units for air pressure (Pa or hPa)

The months and summer are now clear, and units are changed from mbar to hPa.

Table 2: Caption: 'Mean monthly surface energy balance components/fluxes and monthly melt totals.' All details about the location can be left out, this can be read in the text and is also included in the caption of Table 1. Can you use symbol notation for melt as well, being the sum of the melt rate?

The caption of Table 2 has been revised.

Table 3: Note that all sensitivities are calculated using the JJA mean values, now this is only stated for δQN. Furthermore, in the table on line 998, there is no apparent change with regard to the previous line. However, δh is not zero here, which should be mentioned. On line 1003, it is not QS (a variable that has not even been introduced) that is varied, but Q0.

The caption is revised to indicate that the results are for the summer mean, and the descriptions have been clarified wrt the perturbations. Qs changed to Q0, thanks for catching this.

Table 6: 'NARR-based mean monthly...'

The table's caption is revised (Table 5 now).

Figure 1: Either note that KG indicates Kwadacha Glacier, which is mentioned once in the paper or remove the dot and zoom in on the map around Haig Glacier. I suggest to do the latter.

We will zoom in, as suggested (now Figure 2).

Figure 2: Remove the figure title above the panels and add the melt season period to the caption. Include a legend to indicate the different fluxes and remove from the caption, this makes the figure and caption easier to read. Showing albedo changes as absolute or relative (%) values is not exactly the same, if you like to use the same scale as for shortwave radiation, then just say 10 x albedo change. Since the x-axis label also only mentions the shortwave perturbation, it may be a better solution to use the upper x-axis to indicate the albedo scale and title. Furthermore, 'SW' is now used for shortwave radiation instead of S, please be consistent with notation throughout the manuscript.

Figure revised as per the suggestion.

Figure 3: Please use the same variables and colours as in Figure 2.

Revised, now consistent.

Figure 4: 'Table 5 gives the bias and correlations.' This figure has been removed.

Figure 5: More tick marks are needed on the x-axis, at least for every five years. This figure has been removed.

Many thanks for this unusually careful and detailed review. Lots of insights and valuable suggestions. Much appreciated.

References

Gerbaux, M., Genthon, C., Etchevers, P., Vincent, C., Dedieu, J. P.: Surface mass balance of glaciers in the French Alps: distributed modeling and sensitivity to climate change. Journal of Glaciology, 2005, 51, 561-572, doi:10.3189/172756505781829133

Giesen, R. H., L. M. Andreassen, M. R. van den Broeke en J. Oerlemans: Comparison of the meteorology and surface energy balance on Storbreen and Midtdalsbreen, two glaciers in southern Norway. The Cryosphere, 2009, 3, 57-74, doi: 10.5194/tc-3-57-2009.

J. Oerlemans: The mass balance of the Greenland ice sheet: sensitivity to climate change as revealed by energy-balance modelling. The Holocene, 1991, 1, 40-48, doi:10.1177/095968369100100106

Ohmura: Physical basis for the temperature-based melt-index method. J. Appl. Meteor., 2001, 40, 753–761, doi:10.1175/1520-0450(2001)040<0753:PBFTTB>2.0.CO;2

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-6, 2016.