

# ***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**

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## **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

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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.

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.

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.

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. 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 (bias-corrected) 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.

### 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)).

105: I wonder what the net energy flux  $Q_N$  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.

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  $h < 30$ .

192-193: More detail is needed about the roughness length scales, as there are differ-

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ent 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?

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.

212-214: I have the impression that  $Q_E$  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).

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?

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

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 losing 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.

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'.

382-389: With unit forcings, as is done here, the sensitivities to changes in the different

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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.

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.

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.

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.

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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?

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.

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

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  $315 \text{ W m}^{-2}$ . Can you give the fraction of the time with a melting surface to the total time?

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.

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

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.

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

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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.

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  $Q_N$ , which is especially illustrative for the opposite effects found for wind speed changes.

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.

### Technical corrections

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

55: 'reanalyses'

60: 'for snow and ice melt factors'

69: 'crucial to ablation on'

116: 'solar radiation that is reflected'

123:  $\phi_0$  is used in Equation (3) instead of  $\psi_0$

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

158: 'ratio of potential direct to measured' (or is measured radiation only direct radiation as well?)

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

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

189-190: Reorder: 'We estimate  $T_s$  from an inversion of Eq. (5), using'

193: 'can be'

229: 'meteorological conditions'

264: 'Warm summers generally cause'

265: 'but the energy balance is sensitive to'

286-287: 'of the response to a temperature change'

305: Remove the spaces in 100 (1 00)

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

340: 'at the AWS site'

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

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

415: Is  $Q_{S0}$  equivalent to  $Q_0$  introduced in Eq. (3)? If yes, use the same notation, if no, clarify the difference.

445: 'last two lines'

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

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

500: 'Sensitivity to albedo changes over'

507: 'directly'

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

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

528: 'induce'

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

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.

661: 'changes in most meteorological variables'

668: 'Increases'

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

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

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

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

726: 'balance'

747: 'allows for a'

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

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

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for melt as well, being the sum of the melt rate?

Table 3: Note that all sensitivities are calculated using the JJA mean values, now this is only stated for  $\delta Q_N$ . Furthermore, in the table on line 998, there is no apparent change with regard to the previous line. However,  $\delta h$  is not zero here, which should be mentioned. On line 1003, it is not  $Q_S$  (a variable that has not even been introduced) that is varied, but  $Q_0$ .

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

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.

Figure 2: Remove the figure title above the panels and add the 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 3: Please use the same variables and colours as in Figure 2.

Figure 4: 'Table 5 gives the bias and correlations.'

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

## 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

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[Interactive comment on The Cryosphere Discuss.](#), doi:10.5194/tc-2016-6, 2016.

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