Referee's comments are in black.

Author's responses are in blue.

Author's changes in the manuscript are in red.

Anonymous Referee #2

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The paper combines different ground-based and profile measurement techniques to analyze surface energy fluxes, understand how they are influenced by clouds and their impacts on surface temperature. It provides an important closure of the SEB by calculating turbulent and conductive fluxes and includes a very useful comparison of bulk and EC turbulent flux calculations. To my opinion, the authors have made a tremendous job of combining various high-end measurement techniques to have a closure on SEB through one year of data. At the same time, the authors apply several prior assumptions limiting the learning potential from this rich dataset. The paper should be also condensed and restructured: at times, very lengthy descriptions hide the main idea, while sometimes important information is missing. I recommend this paper for publication given that the major and minor issues below are addressed.

Thank you for the detailed reading of the paper, valuable comments and suggestions. We have tried to clarify points of confusion and add context when needed in order to make the paper stronger.

Major comments:

1) It was disappointing to see that the forcing analysis is reduced only to clouds and cloud forcing is reduced only to two cases. A-priory assumptions have been made, eg, indeed liquid-containing clouds are important for SEB, however this is mostly true for summer and ice clouds play an important role in winter (and year total) SEB (as was shown by Van Tricht et al 2016). It would be useful to use these unique comprehensive data to present statistics of SEB depending on a variety of factors - including cloud LWP and IWP (if possible as this parameters is more difficult to derive), PWV, wind speed (especially its effect on turbulent fluxes), near-surface temperature and humidity gradients (and near surface stability).

    The previous title prompted expectations that the paper would specifically address forcing at Summit. The intended focus of the paper is rather the responses to radiative forcing and how we would expect the SEB to respond to a given change in forcing. There are a number of ways in which clouds can modulate the downwelling radiation at the surface and we believe it is beyond the scope of this paper to address both radiative forcing and the responses. The case studies (Fig 5,6), Fig 7 and Fig 11 link the current analysis to previous analysis of cloud radiative forcing in Miller et. al 2015. We believe the process based-relationships are useful for model validation. The relationships go beyond reporting a temporally specific snapshot of the monthly SEB values for a given subset (clear-sky, large LWP, etc . . .). Often models do not accurately capture cloud occurrence and phase. This observationally based data set can be used to validate the energy fluxes at the surface and go beyond simply looking at individual components such as the downwelling LW and investigate if the non-radiative terms would respond realistically to produce accurate surface temperatures.

The title has been changed to: Surface Energy Budget Responses to Radiative Forcing at Summit, Greenland.

It is true that ice clouds play an important role in determining the SEB. Fig 4a shows that clouds are present much of the year (~90% of the time), while the amount of liquid present has a distinct annual cycle. Thus it is true that ice clouds are important in radiatively warming the surface yet cloud phase is the dominant factor in determining the magnitude of the warming. Both ice clouds and mixed-phase clouds modulate SW radiation and thus solar zenith angle is an important consideration in CRF. Figure 7, which bins the forcing terms according LWP and insolation scenarios, supports the conclusion that clouds radiatively warm the surface year round, during times of high solar elevation and in the absence of insolation.

For emphasis we created a new subheading titled, “3.4.3 Cloud Effects on the SEB”

This subsection provides estimates of quantitative insights (Fig11) into the role of clouds on the SEB throughout the year and estimates the resulting effect on surface temperature.

2) There is no mentioning of the importance of surface snow properties for the surface albedo and its influence on the net SW flux. On p. 7 the authors are saying " The surface albedo is affected by the solar zenith angle" - it is stated that this is the only factor affecting albedo. Have the authors looked at the surface properties? Snowfall, temperature and wind conditions have a large affect on the surface snow microstructure with consequences for the surface albedo (see Carmagnola et al 2013) and thus have to be included into the SEB analysis.

Carmagnola, C. M., Domine, F., Dumont, M., Wright, P., Strellis, B., Bergin, M., Dibb, J., Picard, G., Libois, Q., Arnaud, L., and Morin, S.: Snow spectral albedo at Summit, Greenland: measurements and numerical simulations based on physical and chemical properties of the snowpack, The Cryosphere, 7, 1139-1160, doi:10.5194/tc-7-1139- 2013, 2013.

The reviewer brings up a good point that the albedo is affected by snow surface properties. In our analysis measured changes in albedo affect the SWnet component of the forcing term. It is estimated that the changes to the SWdownwelling and LWdownwelling by clouds and insolation are much greater than the changes to SWupwelling by albedo changes alone in the dry snow zone so we focus on clouds and insolation. Yet, the variability of the albedo for a given month will also affect the forcing terms and can be considered a forcing which induces a response of the other SEB terms.

We added this to Section 3.4 to reflect this sentiment:

"In addition, variability in surface albedo acts as a forcing, although at Summit the magnitude of downwelling radiation variations are much greater than the effect of albdeo variations on forcing terms. "

Ideally we would rely on the measurements to determine SWnet. Yet, a parameterization was needed to capture the diurnal cycle of SWnet and develop a dataset where all SEB components were estimated on the 30-minute averaged time scale. The relationship between albedo and solar zenith angle was made under all conditions, taking into account all conditions from 2011-2013. The parameterization of the upwelling SW is why the uncertainty the upwelling SW went from 1.8% prior to 2014 to 2.8% during 2014.

We have reworded this paragraph and added information because it was rather confusing.

"The surface albedo is determined by dividing the measured SW↑ by the measured SW↓ and for clear-sky days should have a minimum at solar noon. During 2014 an asymmetry in the diurnal cycle is observed in the measured albedo, where the albedo in the morning is up to 10% lower than in the evening. The NOAA/GMD measurements, which are mounted on the same fixed arm, indicate the same issue (possibly a gradual slope to the surface due to snow drifts). There is good agreement between the ETH SW↓ measurements and the total direct plus diffuse SW↓ values, suggesting that asymmetry in the diurnal cycle of albedo is likely a problem in the SW↑ component. . . . These uncertainty estimates are larger than the reported uncertainty in the measured SW components of 1.8% (Vuilleumier et al., 2014) because, in addition to Z, albedo is dependent on other factors such as the optical thickness of overlying clouds and surface snow properties."

3) It will be useful if the authors extend their linear analysis (fig. 8) to responses to multiple factors. SH and LH strongly depend on the near-surface stability, temperature and humidity gradients, and wind speed. The authors can try multiple regression or neural networks to explore the effect of several predictors.

We believe multiple regression or neural networks analysis is beyond the scope of the paper as we intend to focus on the SEB responses to the forcing terms, while building off previous research that estimates the surface energy fluxes from these predictors. In fact, the effect of these predictors is folded into the values of the turbulent energy fluxes because the input includes stability corrections, wind speed, and temperature and humidity (LH only) gradients.

It is true that our linear analysis is looking at only the first order influence, which is radiative. The RMSE values (Fig 10) of the linear fit are due to the higher order effects (such as mechanical mixing due to high winds or decreases in turbulence due to high stability). In order to incorporate the uncertainties associated with both a response term and the forcing terms we have redone the linear analysis using a different linear fit routine. This technique provides a more accurate response and a smaller uncertainty in the slope by accounting for measurement uncertainties in both x and y.

Table 1 was updated with baseline uncertainties that were not included in the previous version and text was added to describe the linear regression used.

"Performing a linear fit (fitexy, Press et al., 1992) on the relationship between the forcing and response terms, which includes uncertainties in both terms, yields a slope of -1.01 (Figure 8a), indicating that the SEB is largely radiatively driven, . . ."

Press, W. H., Teukolsky, S. A., Vetterling, W. T., and Flannery, B. P.: Numerical Recipes in C: The Art of Scientific Computing, University Press, Cambridge, 2nd edn., 1992.

We recalculated the linear fits using improved linear fit routine, updated Figures 8-11, and made appropriate text changes to ensure consistency across the full document.

Minor comments: Data description has to be made clearer. It will help to include a table with an overview of all measurements used with their basic characteristics - described in more details in the text.

Some data are described in every detail, while others are just mentioned. For example, the radiosonde data used in the analysis have to be explained including the manufacturer characteristics. There are known biases of humidity measurements at cold temperatures - how do they influence the results?

We acknowledge that a clear description of the data is important and have attempted to make the appropriate changes to the document to improve the data description in the manuscript.

We added Table 2, which summarizes all the measurements used in this study.

"Table 2 summarizes the measurements made by the various instruments described below."

Possible biases in the radiosonde humidity measurements do not affect our analysis because the humidity profile is not used in this study.

We added more radiosonde information in the measurement section and removed the mention of humidity measurements:

"Twice daily Vaisala RS92 radiosondes (0 and 12 UTC) from the Integrated

Characterization of Energy, Clouds, Atmospheric State, and Precipitation at Summit (ICECAPS, Shupe et al., 2013b) project are used to directly measure the atmospheric temperature with an uncertainty of 0.5°."

In addition, we moved much of the information from the end of the Measurement section and created a new section "2.6 Cloud Properties and Precipitable Water Vapor".

To address our shortcoming of inadequately describing the cloud property measurements, in section 2.6 we added: "The liquid present cloud fraction for a given month is the number of LWP samples greater than 5 gm^-2 divided by the total number of samples. During May and June 2014 the microwave radiometer measuring 23.84 and 31.40 GHz was off site for repairs and thus LWP and PWV are unavailable for these months. A 35-GHz Millimeter Cloud Radar (MMCR) determines vertically resolved cloud presence. Monthly cloud fractions are calculated using a MMCR detection threshold of -60dBz, retaining sensitivity to most hydrometeors"

"MMCR derived cloud fraction (solid) and MWR derived liquid present fraction . . ." was added to the caption of Figure 6 to link these results to section 2.6.

p. 5: Please describe the instruments at the NOAA/GMF meteorological tower, which, as the authors say, are the primary source of the near-surface measurements

We added the sensor information in Table 2 and added in the text:

". . . temperature measurements (Logan RTD - PT139 special order) with a specified resolution of . . ."

p 5: "The specific humidity at 2 and 10 m, which is needed for deriving LH, is calculated from CIBS relative humidity and temperature measurements in combination with NOAA/GMD temperature and pressure measurements.": what do you mean "in combination"? NOAA and CIBS towers are located 1 km apart... Do you take the average values? How CIBS RH are measured - are these Picarro at 50m tower? What about the data gaps then (they are only until Dec 2013) - are the NOAA RH values used after that? This is not clear.

The description of meteorological measurements has to be made clear. A photograph of both NOAA and CIBS towers will be helpful - as well as a table summarizing all instruments as I mentioned above.

The wording you reference in the manuscript was confusing regarding how specific humidity was determined. The 30-minute averaged temperature and RH values were used to calculate the vapor pressure via calculating the saturation vapor pressure using the Goff-Gratch formulation.

" The specific humidity at 2 and 10 m, which is needed for deriving LH, is calculated from the CIBS relative humidity, CIBS temperature and NOAA/GMD pressure measurements. The saturation vapor pressure, at a given temperature, is calculated using the Goff-Gratch formulation and then multiplied by the relative humidity to get the vapor pressure. Specific humidity is proportional to the ratio of the vapor pressure to the difference in vapor pressure and air pressure. To provide continuity in the LH estimates the meteorologically derived specific humidity values are used as input to the LH flux calculations, while direct measurements of water vapor are used to estimate the uncertainty in this technique during overlapping time periods. . .”

p. 6: " The percent error, using the Picarro measurements as truth, at the 2 and 10 m levels are 53% and 30%, respectively": how were these errors estimated and what are the reasons for such high uncertainty values? are Picarro and meteorological measurements done at the same levels 2 and 10m or as you say the height varies depending on local snow accumulation - and how much is the difference in height then?

You have assumed that Picarro humidity measurements as truth - can you provide more justification? There have been different results of comparing Picarro with independent humidity measurements and also estimating the accuracy of the field measurements compared to the laboratory measurements (eg Aemisegger et al 2012, Bonne et al 2014). Aemisegger et al 2012 found that the water vapour mixing ratio uncertainty can be quite high in the field and depends on calibration frequency and other effects. I am not an expert in this but invite the authors to include more detailed comments how Picarro measurements were done and used to derive water mixing ratio and their quality. Aemisegger, F., Sturm, P., Graf, P., Sodemann, H., Pfahl, S.,Knohl, A., and Wernli, H.: Measuring variations of 18O and 2H in atmospheric water vapour using two commercial laser-based spectrometers: an instrument characterisation study, At- mos. Meas. Tech., 5, 1491–1511, doi:10.5194/amt-5-1491-2012,2012. Bonne, J.-L., Masson-Delmotte, V., Cattani, O., Delmotte, M.,Risi, C., Sodemann, H., and Steen- Larsen, H. C.: The isotopic composition of water vapour and precipitation in Ivittuut, southern Greenland, Atmos. Chem. Phys., 14, 4419–4439,doi: 10.5194/acp-14-4419- 2014, 2014

We have calculated percent error, thusly:

Percent error = 100*(q_met – q_picarro)/q_picarro

Meteorologically derived percent errors are large because the uncertainties in q_met are large relative to the amount of water vapor that exists in such a dry environment. The Picarro, CIBS temperature and CIBS RH are all mounted on the 50m tower so any change in local accumulation will uniformly affect them all.

The reviewer is correct in saying the Picarro measurements are not absolute truth. Yet, they are well calibrated and thus we compute the percent error of the meteorologically derived values using the calibrated values. The details of the calibration are beyond the scope of this paper so we have provided the Bailey et. al. 2015 reference.

We changed the text reflect the fact that the percent error is calculated in reference to the Picarro measurements and removed the word “truth”.

“The percent error, relative to the Picarro measurements, at the 2 and 10 m levels . . .”

p. 6: Same comment as for other measurements - please include a table and technical description of the ground-base remote sensing equipment used to derive cloud properties. "in operation since May 2010" - until the present time? no data gaps or measurements issues?

We have added information in Section 2.6 and summarized the instruments in Table 2.

p. 6, section 2.2: where are the ETH radiative sensors are located wrt the NOAA tower and CIBS?

All measurements are with in 1km of each other, thus 30-minute averages should account for much of the local variability on this relatively homogeneous surface.

The relative position of the ETH measurements is described in the text: “The radiation station is located between the 50m tower and the NOAA/GMD met tower.”

p. 12 section 3.1 title: why mentioning the period in the title? remove it.

Changed title to: “Temperature Profiles”

p. 12, line 22: "free troposphere above ~500m": very often boundary layer height (which is the lower value of the free troposphere) in the Arctic extends above 500m and the authors also contradict themselves as on line 17 they speak about synoptic influences at 1-5km

Changed to: "free troposphere above ~1 km"

p. 12, section 3.2 title "Case studies" - should reflect more precisely the content (eg, "Cloud forcing case studies")

done.

Section 3.4.2 the text has to be condensed.

These are the main findings of the paper and thus we left much of this analysis intact.

A new subsection (3.4.3) was created to separate the estimated cloud effects from the SEB response analysis.

Fig. 11: please remove the period from the figure and leave it in the caption

done

Technical comments:

abstract: .. "calculate estimates of..." - replace with "estimate"

done

p. 2: trending.. please use a less colloquial word (eg, showing a trend)

done

2.1 Section title has to be more precise, eg Meteorological and snow measurements

done

p.5: Root Mean Square -> capitalization not needed

done

p. 9" which is that determined" – rephrase

changed to: “which was determined”

p. 12, line 5, last sentence: repetition (rephrase)

We combined the sentences to remove the repetitive nature of the sentences. “A warm or cold pulse at the surface propagates to deeper portions of the GIS over time and can take days to influence the temperatures at 1-2 m depth.”

p. 13, line 23: on the 10th of November

done

p. 20, line 8: LWup should be without minus

done