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Interactive comment on "Forcing and Responses of the Surface Energy Budget at Summit, Greenland" *by* Nathaniel B. Miller et al.

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.

Major comments:

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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).

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.

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.

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?

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

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.

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

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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, Atmos. 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

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?

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

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

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

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

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Section 3.4.2 the text has to be condensed.

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

Technical comments:

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

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

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

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

p. 9" which is that determined" - rephrase

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

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

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

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