Response to both reviewers

> Anonymous referee
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> *This is a very thorough analysis and interpretation of 5 years of automatic weather station data from the western flank of the Greenland Ice Sheet. The MS contains a wealth of meteorological data that will be of interest to other Greenland workers, and as such is worth publishing. There is also useful consideration of ice surface albedo feedback and sub-surface refreezing. I found the paper to be well-structured, well-written and -illustrated and easy to follow. I concur with many of the comments of Reviewer 1.*
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> Referee Xavier Fettweis
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> *This paper presents recent changes in the energy balance and the snowpack behaviours at KAN_U situated near the equilibrium line in the accumulation zone of the Greenland ice sheet. It is not the first time that energy balance and melt from in situ observations is discussed in this south-western part of the GrIS (van den Broeke et al., 2011) but KAN_U is situated in the accumulation (while measurements from the ablation zone only was presented in van den Broeke et al. (2011)) and the discussion about the snowpack changes in 2012 is interesting, innovative and deserves to be published in TC with some minor revisions only.*

We thank Xavier Fettweis and an anonymous reviewer for their enthusiastic response, encouragement and constructive comments on our discussion paper. In the following, we address all points:

> *The paper is clear and fits well with TC. The text is well written but sometimes it is hard to read due to the abundance of numbers and statistics in the text. Some simplifications when nothing important is told (e.g.: lines 1-10, pg 2883) could be made in the text by simply referencing to the corresponding tables.*

Taking also into account that Reviewer #2 found our discussion paper easy to follow, we have attempted some minor simplifications throughout the text, in order to make our quantifications more straightforward. The paragraph exemplified, we reformulated as: “Figure 9a, which depicts total monthly surface energy exchanges throughout the study period, illustrates that E_S^{Net} and E_L^{Net} dominate the SEB from May to September, while E_L^{Net} and E_H dominate the SEB during the remainder of the year. During the years exclusive of 2012 considered here (2009, 2010, 2011 and 2013), the total summer energy input to the ice sheet surface was 620–650 MJ m^{-2}. This energy reaches a peak in July. In July 2010, for example, the total energy input reached 246 MJ m^{-2}. By contrast, in 2012, the total summer energy input exceeded 770 MJ m^{-2}, and in July it reached 304 MJ m^{-2}. The 2012 total energy used for melt was 414 MJ m^{-2} (65 % higher than in 2010), of which 183 MJ m^{-2} was used for melt in July.”

> *Line 25, pg 2875 vs line 11 pg 2878: 360 or 400 kg/m3 for the snow density?*

Admittedly, it is an unclear wording. The measured average snow density is 360 kg m^{-3}, used in Table 4 in the discussion paper. The snow density used in the simulations was rounded to 400 kg m^{-3}. We have reformulated as: "Solid precipitation is added in the model based on KAN_U sonic ranger measurements, assuming the rounded average snow density found in snow-pit measurements, i.e. 400 kg m^{-3}".

> *Table 4: I am a bit surprised that we use here a mean density of 360 Kg/m3 for estimating the mean ablation rate. As snow is melting, the snowpack density should be higher.*

This is a fair point. Of course, there is density increase after melt percolates, as illustrated in Figures 9b and 11c of the discussion paper. We used this density in an attempt to estimate the mass flux melted for the first time from the initial snowpack each melt season. Arguably, this can be confusing, and not necessarily correct. Therefore, we decided to remove the estimated ablation rates from Table 4, as they are also not a crucial point in the study.

> *Where does the density uncertainty of 40 kg/m3 come from?*

This uncertainty is based on the standard deviation among the measurements from snow pit measurements conducted in spring 2013, which we now clarified in the caption.

> *Just giving the difference in snow height is for me more reliable.*

Indeed, we also think that heights are more reliable and generally should be also provided, as SMBs are sometimes a matter of interpretation, especially in the accumulation area of the ice sheet. We have now inserted the winter/summer heights in Table 4. We did, however, keep also our SMB estimates for reference. The updated Table 4 of the discussion paper is shown here as Table I.

> *Line 16, pg 2880: these low albedo values are for me more likely the result of the snowpack erosion by the wind (making apparent old firn) than reduced winter precipitation. The regional model MAR does not suggest particular low winter accumulation at KAN_U in 2012-2013.*

We agree, and MAR is accurately suggesting substantial accumulation during winter 2012–2013 at KAN_U. We also agree that there might have been snowpack erosion at our study site (Leanerts et al., 2014). However, this erosion is unlikely to have caused exposure of old firn. By November 2012, the snow thickness was 0.6 m (Fig. 2). From that point onward, and until spring 2013, the sonic ranger measurements suggest that there was limited accumulated snow on top of that initial snowpack in autumn; hence wind might have had an effect on surface, but certainly not erode the whole snowpack, thereby exposing firn. The snow at the surface probably lost part of its reflectivity after the prolonged exposure to the atmosphere. The area received substantial accumulation in spring. This was also verified by the Arctic Circle Traverse 2013 (ACT-13) in late April 2013, when we were at the location and the snow cover was ~ 0.9 m. After two weeks, the snow cover had increased by ~ 0.3 m.

> *Lines 15-20, pg 2886: I do not see the interest of discussing NAO here. The role of NAO over Greenland is well known for explaining the recent melt increase and for me, Fig 10a as well as these 5 lines should be removed.*

Initially, our aim was to provide with a description as complete as possible, but we agree that information on the NAO, as well as its connection to recent climatic changes are readily available in the literature, and perhaps more relevant to larger-scale studies. We have now removed all discussion of the NAO, and also Figure 10a. The new version of Figure 10 of the discussion paper is shown here as Figure I.

> *Lines 5-12, pg 2887: The comparison with MODIS is interesting but a part of the MODIS based albedo decrease could be the result of the declining instrument sensitivity of the MODIS sensors1. This issue should be discussed. However the same albedo trend is also simulated by MAR (forced by NCEP-NCARv1) which also simulates the exceptional low albedo in summer 2012 (see Fig.1 next page)!*

It is, indeed, an issue that should be mentioned when discussing remotely-sensed albedo. We have now included the following in the manuscript: "Part of the MODIS based albedo decrease could be the result of the declining instrument sensitivity of the Terra MODIS sensor (Wang et al. 2012; Lyapustin et al. 2014) though updated (through 2014) comparisons between MOD10A1 and ground observations from GC-Net data (Box et al. 2012; not shown) do not indicate an obvious nor statistically significant difference."

> *According to MAR, it is the first time in summer 2012 since 1950 that significant ice lenses appear but in 1960, MAR also simulates high runoff rates due to snowpack meltwater saturation suggesting that it is not the first time that significant melt events occur at Kan_U.*

We have reasons to believe that this is partly incorrect. In detail, two firn cores were retrieved in May–June 1989 from Site J (66° 51.9' N, 46° 15.9' W, 2030 m a.s.l.; Kameda et al., 1995), ~ 36.2 km east-southeast from KAN_U (Fig. IIa). According to the deduced Melt Feature Percentage (MFP) shown in Figure IIb (Kameda et al., 2004), 1960 has a higher melt feature percentage (MFP) than usual, but assuming that strong melt in 1960 would mostly percolate into previous year's firn layers, there is no indication that 1960 stands out much.

Another observational study analyses 10 m firn temperatures based on measurements prior to 1965 (Mock and Weeks, 1966). In their analysis, the closest site to KAN_U was ~60 km south-southwest, i.e. at a lower elevation. According to this study, the estimated 10 m firn temperature at KAN_U was at that time around –14 °C, suggesting that there was no significant refreezing in that period (and therefore ice content within the firn).

The above observations come from different settings, but they provide evidence from both higher and lower elevations than KAN_U, thus bracketing out location. From these observations, we cannot explain the MAR-simulated meltwater at KAN_U in 1959–1964 (Fig. 1 of the review) saturating the snowpack and not percolating to available pore space below. We believe this to be corroborated by the concurrent summer (JJA) albedo from MAR, the high values of which do not imply meltwater presence at the surface, but rather increased snow metamorphosis.

> *Finally, while some runoff still occurs in 2013 (while the summer was cold) as a result of the 2012 summer induced snowpack compaction, runoff disappears in summer 2014 suggesting that we need several successive summers as 2012 to have a significant snowpack degradation.*

Correct, and a very good point. Hopefully, with the present study we communicated effectively that the years 2010–2011–2012 were three consecutive years of unusual atmospheric conditions that resulted in negative net mass budgets and increased melt. The exceptional condition of the firn in 2012 was in part preconditioned by the two previous years. This can be understood by the analysis of the subsurface temperature measurements (Fig. 11a), and is the subject of another study (Charalampidis et al., under review). We should add at this point, that we are very pleased to see agreement on runoff between MAR and our results for the years 2012 and 2013.

> *Some RACMO (or eventually MAR) outputs could be added in the manuscript to put the 2012 summer in a longer term perspective instead of using Kangerlussuaq measurements.*

A very good point, indeed. While in the present study we tried to base our argumentation exclusively on observations, it is our aim to increase the spatiotemporal perspective of our investigations using RCM output, which will be in fact the subject of a forthcoming study.

References

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Table I. Surface height changes and mass budgets (measured in winter and calculated in summer) at KAN_U in meters and m w.e., respectively, and ablation duration. The uncertainty of the surface height change is estimated at 0.2 m. The mass budgets are calculated assuming snow density of 360 kg m^{-3} (the average density of the uppermost 0.9 m measured on 26 April 2013), with uncertainty estimated at 40 kg m^{-3} (standard deviation among the snow-pit measurements). The snow density assumption was not needed in 2012 and 2013 when actual density measurements were conducted.

	winter height change	winter budget	summer height change	summer budget	net budget	ablation period
2008–2009	+1.64*	+0.59*±0.15	–0.71	–0.26±0.08	+0.34*±0.12	01/06–19/08
2009–2010	+0.70	+0.25±0.08	–1.22	–0.44±0.09	–0.19±0.12	30/04–05/09
2010–2011	+1.02	+0.37±0.08	–1.13	–0.41±0.09	–0.04±0.12	28/05–13/08
2011–2012**	+0.70	+0.25±0.08	–1.80	–0.86±0.14	–0.61±0.16	27/05–24/08
2012–2013***	+1.24	+0.45±0.09	–0.75	–0.27±0.08	+0.18±0.12	29/05–17/08

* value inferred from Van de Wal et al. (2012)

** estimate based on snow-pit densities from May 2012

*** estimate based on snow-pit densities from May 2013

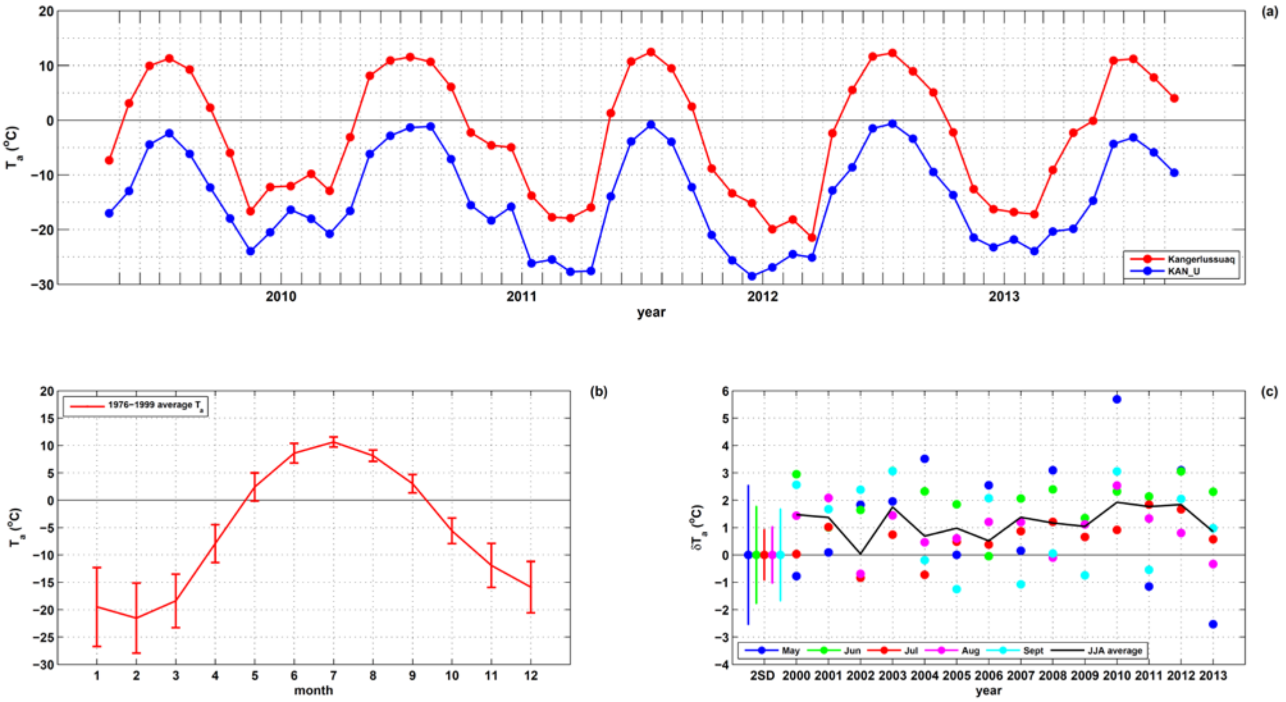

Figure I. (a) Monthly air temperature from Kangerlussuaq and at KAN U. Correlation coeffcients: 0.97 for the extent of the KAN_U data, 0.66–0.99 for the months individually, minimum being January. **(b)** Monthly reference period (1976–1999) air temperature at Kangerlussuaq. **(c)** Monthly (May to September) and summer (June-July-August average) air temperature anomalies at Kangerlussuaq for the years 2000–2013. Error bars indicate two standard deviations.

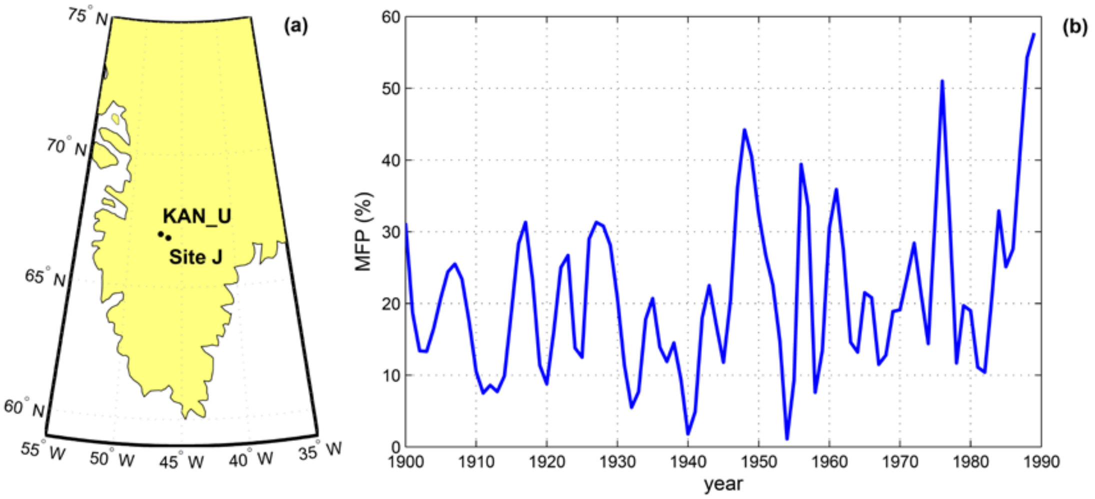

Figure II. (a) Location of Site J (Kameda et al., 1994) with respect to KAN_U. **(b)** Melt percentage data from the top part of a firn core retrieved in 1989 at Site J by the Japanese Arctic Glaciological Expedition (JAGE89; Kameda et al., 2004).