Reviewer 1

General

The paper is well written. It can be accepted after minor revision.

We thank the reviewer for the enthousiastic response.
We address the reviewer's comments below.

Specific remarks

1) Introduction and 2.1: How is runoff calculated? Do you consider perculation and refreezing? Is there any way to validate runoff (river runoff data)? The uncertainty of runoff in the mass balance of the ice sheet has to be discussed.

Surface melt, percolation, refreezing and meltwater runoff are calculated by a snow scheme (Ettema et al., 2009), as indicated in section 2.1. Runoff is very hard to evaluate, since there are no in-situ measurements available. However, modeled precipitation and surface mass balance have been extensively evaluated using available in-situ observations (Ettema, 2009), and total mass balance (SMB-D) shows good agreement with GRACE (van den Broeke et al., 2009). Implicitly, this implies that the modeled runoff is realistic. We have added a sentence to section 2.1 stating this.

The uncertainty in meltwater runoff is currently estimated at ~20%. In this paper, however, the primary focus is on drifting snow, and therefore we feel that an extensive discussion on runoff uncertainty would reduce the focus of the MS. Instead, in the revised MS, we now refer to a paper (Van Angelen et al., 2012), in which this issue is discussed in more detail.

2) P1614, line 11: what does it mean that (background) albedo is prescribed by MODIS data? This should be computed by your albedo scheme.

The albedo scheme performs a prognostic calculation of snow grain size and albedo, not that of ice. To still account for spatial variability of ice albedo in summer (e.g. the so-called dark zone in western Greenland, Wientjes et al., 2010), we used MODIS data to construct a background ice albedo field. A reference is now added to the paper that describes the albedo scheme in detail (Van Angelen et al., 2012).

3) Conclusions: Include a discussion of GRACE measurements. Are they consistent with your SMB?

Van den Broeke et al. (2009) show that SMB from RACMO2, together with observed ice discharge, shows a good agreement with GRACE. We added this paper to the references.

4) Table 1: add a comparison for high wind speed (>10m/s).

As shown by Ettema et al., 2010 (Fig. 7), the correlation between observed and modeled mean 10 m wind speed does not decrease with higher wind speeds. This is indicated in

the revised text:
"High wind speeds that drive drifting snow are also well represented by RACMO2 and no clear underestimation is seen, so the model resolution appears sufficient to represent local wind climates on the ice sheet (Ettema et al., 2010a)."

5) Fig.5D: change scale for better visibility.
This is changed in the revised manuscript.

Reviewer 2: R. Dadic

We thank Ms. Dadic for her constructive comments.
We will respond to her reviews point by point below.

General

My main revision point is the discussion about trends (from Figure 9). The authors discuss trends since 1990 and it is not clear why that year is chosen.

We agree with the reviewer that this was not clear from the original text of the manuscript. We selected 1990, because it marks the beginning of an increase in atmospheric temperatures and surface melt, and a decrease in the surface mass balance of the ice sheet (Ettema et al., 2009). Before 1990, wind and temperature were rather constant (Figure 9). Because we want to show the impact of these atmospheric changes on the surface state of the ice sheet and hence on the sublimation (drifting snow and surface) we selected this period to calculate trends. .

We agree that the trend in drifting snow sublimation is strongly influenced by the years 1995 and 1996, when SU_{ds} was large. This is reflected in the large standard error that is provided along with the trend (in fact, the standard error is equal to the trend), as indicated in the revised text:

“ The large uncertainty in the trend of SUds is due to strong interannual variability, and a longer time series is required to reach significance at a 95 % level.”

For the reasons stated above, we would like to keep Figure 9 as it is, while changing the text according to the reviewer’s suggestions.

In section 3.2.3, line 8, we added:

“ Since 1990, following a rise in atmospheric temperatures, meltwater runoff has been increasing (Ettema, 2009), only partly compensated by increased snowfall (Van den Broeke, 2009), which led to a strong decline in SMB. Fig. 9 shows the recent trend of SU_s and SU_{ds}.”

Another helpful addition to the paper would be including scatterplots when discussing the correlations (spatial, figures 4 and 5) between e.g. a) snow density and amount of drifting snow or b) wind speed and drifting snow.

We agree and included a new Figure (Fig. 6), showing the correlation between mean snow density, mean wind speed and mean RH2m versus mean SU_{ds} and TR_{ds}, respectively.

Specific comments

P1611, L7: What is the K-transect?

The K-transect is a transect in southwestern Greenland, along which automatic weather stations are installed and maintained by IMAU. This is now indicated in section 3.1 of the revised manuscript.

• P1611, L7–8: Is there any validation of how well the model captures snow den- sity? If there is, it should be discussed, because it is quite relevant for this study.

Unfortunately, there are no reliable observations available of surface snow density on Greenland. The used parameterization for surface snow density is based on observations from the Antarctic ice sheet, where good agreement was found between observed and modeled near surface snow densities (Lenaerts et al., 2012), We decided to use the same relation for Greenland. This is now explained in section 2.1.

• P1614, L5–7: It is actually not grain size, but optical grain size, effective radius or the specific surface area, which influences the albedo, and which is discussed in Flanner and Zehnder 2006. Please correct in the text.
Thanks, this is corrected.

• P1614, L16: I assume that AIS is the Antarctic Ice Sheet.
Yes, as stated at the same page, at line 8.

• P1616, L22–23: How do your estimates of the Greenland SMB compare with other estimates of the GrIS. Please discuss.
We added to the manuscript, at page 1618, line 15 (where we discuss the integrated GrIS SMB):

" This estimate is comparable to other recent GrIS SMB estimates that are assembled by Rae et al., 2012, which are mostly in the range 320-450 Gt/yr"

• P1617, L1: The process of increasing densification rate is well known and and was not first suggested by Lightenberg 2011. Please cite original references, such as Wakahama (1974).
We added Wakahama (1974) to the references.

• P1617, L2–4: Some original work on threshold friction velocity should be cited here, such as Li and Pomeroy (1997), Pomeroy et al. (1993) or work by Schmitt R.A. in the 1980-ies. Liston and Sturm (1998) have a fairly good reference list for threshold friction velocity.
We added Li and Pomeroy (1997) and Schmidt (1981) to the references.

• P1618, L1–6: Please discuss why the drifting snow sublimation (5c) is different from the drifting snow transport (5a). Is it because of temperature and rH differ- ences in regions with the same wind speed? Maybe scatterplots between these two values might help (with different colored points for grid cells with a certain temperature and relative humidity).
That is correct, SU_{ds} is strongly dependent on the RH of the surface layer, whereas drifting snow transport mainly depends on wind speed and can continue even the air is saturated or supersaturated.

To illustrate this, we included the scatter plots as a new figure 6 and added to the text (P1618, line 7):

"The non-linear nature of drifting snow is evident from Figure 6. Both SU_{ds} and TR_{ds} strongly depend on wind speed, but SU_{ds} is limited when the surface layer becomes saturated. Drifting snow sublimation is therefore most active in relatively warm, windy regions where the surface layer is relatively dry."

Except for figures 3, 6, and 9, the fonts on the figures are too small. Please adapt fonts so they match those in e.g. figures 3, so they are readable without having to blow up the figures.
We revised the figures and adapted the fonts to enhance readability in all figures.

Figure 1 is not particularly useful and can be omitted.
We are of the opinion that Figure 1 is useful to illustrate the model domain, topography and relaxation zone and to support the section on the numerical setup.

References

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