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Interactive comment on "Drifting snow measurements on the Greenland Ice Sheet and their application for model evaluation" *by* J. T. M. Lenaerts et al.

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The paper introduces autonomous drifting snow observations performed on the Greenland icesheet recorded in the fall of 2012. It consists in one month of unique dataset, including meteorological variables (snow height, friction velocity, wind direction and velocity, humidity) and drifting snow flux and snow particle size distribution measured thanks to a Snow Particles Counter set up at 1 meter. This dataset is used to evaluate the regional atmospheric climate model RACMO2, which includes the PIEKTUK-B drifting snow model. It was shown that the model is able to simulate the near surface climate and to capture the observed drifting snow event. Nevertheless the drifting

snow transport is significantly overestimated. It is a well-written and interesting paper introducing new data. But different points, including data processing and numerical ?simulations, still require further clarifications before the paper is published.

We thank the reviewer for the valuable comments, which are responded point-by-point below

p 22 / line 25 : drifting snow sublimation (Suds) instead of drifting snow erosion Could you shed more light on individual SMB components in the SMB equation as it was done in (Leanarts et al., 2012b). The equation and a schematic illustration will be useful. I suppose that in the present paper Snow erosion (ERds) is the transport divergence (div.Trds) representing a net mass loss at the surface ? Otherwise, at first reading, it is not clear how account is taken of the snow accumulation. Moreover the variable was later used without being introduced (p 31/ line 8)

We will introduce an equation and description of the SMB in the revised paper

p 23 / line 17 : I did not understand why (Cierco F-X., F. Naaim-Bouvet and H. Bellot, 2007. Acoustic sensors for snowdrift measurements: How should they be used for research purposes? Cold Regions Science and Technology, 49, 74-89.) is reflective of the sentence "the contribution of drifting snow sublimation and erosion to the SMB remains poorly constrained.

we will add "...poorly constrained in absence of reliable observations (?)"

p 23 / line 19 :SPC was not deployed by (Gallée, H., Trouvilliez, A., Agosta, C., Genthon, C., Favier, V. and Naaim-Bouvet, F. Transport of Snow by the Wind: A Comparison Between Observations in Adélie Land, Antarctica, and Simulations Made with the Regional Climate Model MAR, Boundary-Layer Meteorol., 146, 133–147). It is the case now but it was not introduced in this paper. You can replace the reference by (Nishimura and Nemoto, 2005)

Thanks, this will be changed in the revised manuscript.

p.25 line 23 : It is not clear for me if the electric motor is controlled by SR50. I suppose it is the case but please could you clarify and indicate the position of SR50 in Figure 3.

Yes it is. This will be specified and included in the revised manuscript.

p. 26 / line 6 : It is not necessary to introduce measurements which are not later used.

We would like to keep the mentioning of these measurements, since they will be used in a companion paper.

p. 25 / line 18 : 917 kg.m-3 instead of 917 g.m-3

Thanks, this will be changed in the revised manuscript.

p. 26 / line 3 : How is calculated the friction velocity shown in figure 6a. Thanks to ultrasonic anemometer (direct eddy covariance measurements) or vertical wind speed profile ? Do the two methods match?

The u_* data shown in Figure 6a are composed of 5 minutes averaged vertical wind speed profiles. These provide a more continuous dataset than sonic anemometer data. The comparison between both datasets after appropriate corrections and selections is very good with a standard deviation of the differences between the 5 minutes averaged values of about 0.06 m/s and a mean difference of 0.003 m/s. This note will be included in the manuscript.

p. 28 / line 14 : How is the snow density estimated? Is there any measurement available?

We have no measurements of snow density on site, but we base this value on available literature. 300 kg/m3 for snow density is a typical value to the dry snow zone in Greenland, see e.g. Hawley et al., 2006 (GRL). This reference is added to the text.

p 29 / line 5 : The snow-particle sensors are very sensitive and return a signal even if only one snowflake crosses the sampling area: the SPC-S7 is able to detect trace

precipitation. The value of 350 microm obtained for SW direction is quite high and may be not representative due to a low particle number. What is the particle number and the associated wind speed for these values.(figure 5d)

We suspect that the signal from these "large" particles indeed stems from precipitated particles, as the reviewer also suggests. This is clearly indicated in the text.

figure 7 : Why is the observed number of snow particles per particle size not drawn in figure 7 during the snowfall event ? There should be a sudden increase in the detection of larger snow particles as observed in figure 12d.

The observations suggest no clear increase of large particle sizes, although RACMO2 suggests that there is snowfall. Presumably the model falsely simulates snowfall and/or the snowfall rate is too low to be picked up by the SPC. This is clarified in the manuscript.

p 30 / line 26 : it is written that the simulated particle sizes are underestimated (figure 10). That is probably true. Nevertheless you must emphasize the fact that SPC is not able to detect particles smaller than 50 ?m. Moreover the number of particles with a median diameter of 44.82 ?m is generally underestimated. This is one limit of this sensor, even if it is probably the most efficient drifting snow sensor on the market.

Thank you for pointing this out; although we do not think that this could greatly affect the total transport, we will include this comment in the revised manuscript.

p 31 / line 8 : The variable Trds has not been introduced.

TRds will be introduced in the revised manuscript with the inclusion of the SMB equation

p 33 / line 26, you could also add “close to 3-4 the French Alps” (Naaim-Bouvet F., Naaim M., Bellot H., Nishimura K., Wind and drifting-snow gust factor in an Alpine context, Annals of Glaciology 52(58) 2011, 223-230.

Thanks, we will add this in the revised manuscript

p 34 / line 1 : It is written that the time-integrated Trds were not significantly altered by the introduction of a variable alpha that increases with height. But what's happen with a alpha value set at 5 or 7 instead of 2 ?

This is a justified issue raised by the reviewer, also raised by the other reviewers. To discuss the sensitivity of the drifting snow module to its input parameters, we have performed a sensitivity analysis, varying (a) drifting snow density (500,700,900 kg/m3), (b) mean saltation particle radius (100,200,400 micrometer), (c) shape parameter α (2,5,8), and (d) friction velocity (-10% and +10%). This sensitivity analysis clearly demonstrates that the drifting snow model is highly sensitive to the input parameters, in particular to (b) and (c) as the resulted flux can vary several orders of magnitude within the applied range of input. For this sensitivity analysis, it appears that the contribution of saltation to the simulated flux at 1 m height is overestimated in the model; if we suppress saltation by applying a larger particle radius, the resulting transport is largely decreased. If we increase the mean saltation particle diameter to 200 micrometer (default 100 micrometer) and alpha to 5, both of which are assumed to be more representative of the observed conditions, we get much more reliable (within one order of magnitude) simulated transport fluxes in both cases (24 and 26 September 2012); it appears that saltation in PIEKTUK is overestimated in the default case, leading to much too high fluxes. This is an important result, which will be presented in detail in a separate table and a detailed discussion. The minimum detectable snow particle diameter of the SPC is 50 micrometer. The omission of very small particles could lead to an underestimation of the observed transport, although we do not believe that this will greatly influence the results. We will add this comment in the revised manuscript.

You have a very nice and unique dataset but you only compare measurements with the final output values of the models, i.e. drifting snow fluxes, temperature, humidity

and wind speed. But what about u*t and z0? Some effects may balance each other and may lead to a positive assessment of the model even if the physics of phenomena is not fully represented. It would be a little bit hazardous to state that the threshold friction velocity in RACMO2 is universally applicable (p 29 line 15) because timing and frequency of drifting snow events qualitatively agree for one event. Sure it is a nice result but it is still a first step : you can compare directly the threshold friction measured thanks to ultrasonic anemometer or vertical wind speed profile and SPC and to drawn them in Figure 6a. These new data will reinforce previous conclusions. How is calculated zo in the model ? Is there any feedback due to presence of particles or the formation of sastruggi, ripples ? It could be also interesting to compare measured and calculated z0. It seems (it is difficult to see in Figure 6) that snowfalls detected on 24th and 26th December did not lead to a decrease in u*t.u*t is always greater than 0.4 m.s-1. According to Guyomarc'h and Merindol (1998), Ut10m could be 4-5 m.s-1 for fresh snow. What is the value of z0 in your experimental site ?

As illustrated in Figure 6a, simulated and observed u_* values are in good agreement for 5 minutes averaged values. We choose not to compare model and measured values of z_0, since these are fixed in RACMO2 during winter conditions (10^{-3} m), a value that is chosen because it yielded the best comparisons between RACMO2 and observations on Antarctica (Reijmer et al., 2004). Modelling the variations in z_0 in a model is challenging as it requires information on many parameters, such as the formation of sastrugi and ripples, the history of the snow, variations in wind direction and the influence of drifting snow, features that are clearly visible in the supplementary movies. However, with a grid that is much coarser than the typical size of these local features that influence the threshold friction velocity, RACMO2 is unable to resolve these. Therefore, we choose to keep z_0 constant and make the threshold friction velocity a function of the snow density (~0.4 m/s). We realise that this assumption likely underestimates the temporal variability of $u_{*,t}$. Analyzing this variability in detail would be interesting, but in the light of the relative simplicity of the RACMO2 description

of $u_{*,t}$, it would a subject that is outside the scope of this paper. We will analyse this in a forthcoming paper.

Reijmer, C.H., E. van Meijgaard and M.R. van den Broeke, 2004. Numerical studies with a regional atmospheric climate model based on changes in the roughness length for momentum and heat over Antarctica, Boundary-Layer Meteorol., 111(2), 313-337.

References Van Wessen et al., 2013 is missing.

Thanks, we will add this in the revised manuscript

Please check the references and correct typographical errors : Van de Wal instead of van de Wal, Vionnet V., Guyomarc'h G., Naaim-Bouvet F., Martin E., Durand Y., Bellot H., Bel C. and Pugliese P instead of Vionnet V., Guyomarch G., Bouvet F.N., Martin E., Durand Y., Bellot H., Bel C. and Puglise P. . ..

Thanks, we will correct this in the revised manuscript

Interactive comment on The Cryosphere Discuss., 8, 21, 2014.