Response to reviewer #1

General points

In this paper the authors use wind profile data collected during austral winter at an Antarctic automatic weather station to study how drag coeficients change as a result of wind direction variations relative to the orientation of sastrugi. It is found that as the wind changes from blowing along the axis of the sastrugi to partly across the axis, the drag coefficient initially increases substantially but relatively rapidly (over 3 hours) returns to its previous (lower) value. The authors suggest that this indicates that, following a change in wind direction, sastrugi are rapidly realigned with the new wind direction as a result of redistribution of blowing snow.

The methodology used is sound. Very strict quality control criteria were applied to the wind profile data before drag coefficients were calculated. This may explain why only two cases were chosen for analysis from a whole winter of collected data. However, it would be useful to know how many other cases (if any) could have been selected and why they were not presented here. If there are other cases that do not fit the pattern of the two described in this paper, this fact should be mentioned.

Another (and main) criterion for observing sastrugi alignment from variations in drag coefficients, as proposed in this study, is the extraordinary constancy in wind direction (at the scale of a few degrees) after a wind shift. Such particular situations are very rare. Given that significant variations in C_{DN10} are observed for wind shifts in direction of only a tens of degrees, the directional effect on C_{DN10} through realignment of the sastrugi cannot be reasonably discussed from more variable wind direction cases. Combined to the strict selection procedure, only two cases were exploitable in this context. To emphasize the singularity concerning the directional constancy for the two aerodynamic adjustment cases we describe here, we have modified slightly the title of the paper as follow: *"Two well marked cases of aerodynamic adjustment of sastrugi"*. We have also completed the paragraph starting P6011:

"The two erosion events depicted in Fig. 4 occurred respectively in March (left panels) and October (right panels) 2013, during particularly constant wind direction conditions which persisted after a wind shift of a few tens of degrees. Such an extraordinary constancy in wind direction, necessary for the following demonstration, is very rare. Combined to the strict selection procedure, only two cases were exploitable in this context.".

One weakness of the study is that no direct measurements of sastrugi orientation are available, so this parameter has had to be inferred from the measurements that were made. The authors suggest that a mini laser scanner could provide valuable information on sastrugi form and alignment. This is certainly true, but even a simple camera system could provide some useful data and might be considerably simpler to install and operate at this remote site.

A camera system has been operated at site D17 during a blowing snow event in late February 2013. This experiment revealed that only infra-red images can provide consistent information due to the lack of visibility and light (especially during wintertime) during snowstorms. Eventually, photographs made during erosion events with a sufficiently high visibility could provide an idea of the form of the sastrugi and allow to determine the alignment, while the laser-scan would allow to reconstruct the complete geometry of the sastrugi. Unfortunately, such systems are still too energy costly to be set up durably at site D17.

Overall, this is a good paper that contributes significantly to our understanding of how aeolian processes affect surface drag over polar snow surfaces. The measurements presented have been

put into the context of previous work in this field and clear recommendations have been made for future work and the development of parameterizations.

I recommend publication of the manuscript in The Cryosphere following attention to the specific points that I have listed below.

Specific points

1. P6005,l21-24: Confusing sentence, "…greater but slower…"?
   This sentence has been changed to the original sentence of Schmidt [1980]: *"…and that this increase slows with time and is slower at lower temperatures."*

2. P6009, l6-10: If FlowCapt™ cannot distinguish between precipitation and blowing snow, surely the FlowCapt flux is an upper bound to the blowing snow transport?
   Indeed, the sensor accounts for all forms of wind-driven snow, from precipitating snowflakes to salting and/or suspended snow particles. Considering that the term "blowing snow" refers exclusively to eroded particles, the aeolian snow mass fluxes provided by the FlowCapt™ is in fact an upper bound to the blowing snow transport.
   Here we refer to aeolian transport of snow without discrimination on the source of airborne particles (i.e., erosion and precipitation), in line with Trouvilliez et al. [2015]. The evaluation carried on by Trouvilliez et al. [2015] shows that the second-generation FlowCapt™ acoustic device seems to underestimate the aeolian snow mass flux compared to a reference optical sensor (Snow Particle Counter; Sato et al. [1993]) especially when precipitated snow mixes with eroded airborne snow particles. As indicated by these authors, the FlowCapt™ measurement principle is based on the momentum transfer of individual snow particles to the sensitive surface, such as the piezoelectric surfaces, which means that the acoustic pressure depends on the size, the density and the speed of the particle. During mixed aeolian snow transport events (snowfall + erosion), the precipitating snowflakes' density is lower than eroded snow particles that originate from the ground. Thus, for a given aeolian snow mass flux, the particle momentum, and by extension the measured acoustic pressure, will be lower for an event with precipitation than for an event without precipitation. The particle's density, which varies from one blowing snow event to another, play a key role in the estimation of the aeolian snow mass flux by the FlowCapt™, as previously highlighted by Cierco et al. [2007]. The inability of the sensor to distinguish precipitating snow particles from eroded ones (both FlowCapt™ and SPC devices "see" all the airborne snow particles whatever their origin) thus lead to amplify the underestimation of the snow flux during mixed events. Therefore, integrated fluxes given by the second-generation Flowcapt™ should be considered as lower bound values.
   To avoid any misunderstanding, we suggest to delete the specifications mentioned about the behavior of the FlowCapt during snowfall occurrences. The corresponding paragraph thus becomes:
   "The authors reported that the instrument underestimates the aeolian snow mass flux compared to a reference optical sensor (Snow Particle Counter S7; Sato et al. 1993), especially during snowfalls. Nevertheless, the equivocal behavior of the second-generation FlowCapt™ does not affect its ability to accurately detect the occurrence of aeolian snow transport.".

3. P6011/Fig 4: What are the uncertainty bounds on the calculated C_{DN10} values? These can be deduced from the confidence limits on the log-lin profiles fitted to the wind data. Are the temporal variations seen in Fig. 4 outside these uncertainty bounds?
   For both erosion events, the confidence limits for each calculated C_{DN10} values were determined following Wilkinson [1984] and using

$$\delta a = \pm z_0 \frac{t_s}{\sqrt{N_a - 2}} \frac{\sigma_y}{\sigma_x} \sqrt{1 - R^2}$$

$$\delta z_0 = \pm z_0 \frac{\delta a}{a} \sqrt{X_{rms}^2 - \ln(z_0)^2}$$

$$\delta C_{DN10} = \pm \delta z_0 \frac{\partial C_{DN10}}{\partial z_0}$$

where δa is the confidence limit of the regression slope, t_s the Student's t parameter (here for 80%, as recommended by Wilkinson [1984]), N_a the number of anemometers (six), σ_x and σ_y the standard deviations of the x and y variables (ln(z) and wind speed, respectively), R the correlation coefficients between x and y values, δz_0 is the confidence limit of the roughness length, a the slope of the log-linear profile fitted to the wind data, X_{rms} the root mean square of the x variable, and δC_{DN10} is the confidence limit of the drag coefficient.

The highest uncertainty bounds deduced from these confidence limits reached $\pm 13\%$ and $\pm 14\%$ for erosion event 1 and 2, respectively. This is small compared to the temporal variations in the range 30% - 120% as seen in Fig.4.

We propose to add the following information to the text at the end of section 2, P6010, l23:

"The 80% confidence limits of each calculated C_{DN10} value were determined following the statistical method proposed by Wilkinson (1984). The highest uncertainty bounds deduced from these confidence limits reached $\pm 14\%$.".

4. P6011, 19-12: ECMWF analyses only "indicate" modelled precipitation, which may or may not relate to what was actually happening. Have you checked weather/precipitation observations from the nearby Dumont D'Urville station?

We asked the personnel currently in charge of weather observations at Dumont d'Urville station. Unfortunately, we were answered that there is no observation available for precipitation for the period concerned. Moreover, as Adélie Land is very prone to aeolian snow transport, these visual observations, if performed, are limited by the inability to discriminate between actual precipitation and pure blowing snow. Therefore, ECMWF operational analysis remain the most relevant and continuous support for evaluating the possible occurrence of precipitation at our measurement site.

We have completed the sentence starting P6011, l9, as follow:

"The occurrence of precipitation may affect the detection of erosion events because the FlowCapt™ sensor does not distinguish between eroded (saltating particles and/or suspended particles of snow) and precipitating snow particles. No visual observation of precipitation from the nearby Dumont d'Urville station were available for the period concerned. Moreover, as Adélie Land is very prone to aeolian transport of snow (Trouvilliez et al. 2014), these observations, if performed, are limited by the inability to discriminate between actual precipitation and pure drifting snow. Here we used the Operational analyses of the European Center for Medium-Range Weather Forecasts (horizontal resolution of ~16 km) to evaluate the occurrence of precipitation at our measurement site. We assumed that both events were pure erosion events after finding negligible precipitation rates for the fully continental grid point including D17. ".

5. (i) P6011, section 3: For the first event, how long had the wind been blowing from around 140 degrees at above the threshold value? Was this long enough for the sastrugi to become aligned with the wind before it changed direction to 160 degrees?

The first erosion event began after a 4-day period without aeolian transport of snow. The drag coefficient is poorly documented during that period (only a few values), primarily because of too low wind speeds. Before the wind shifted to 160° on JD 88, it has been blowing from 140° above the erosion threshold for roughly half a day, which corresponds to the sastrugi streamlining timescale suggested by Andreas and Claffey [1995].
As the drag coefficient, at the beginning of the first event, corresponds to the value associated with wind-aligned sastrugi (C_{DN10} = 1.3–1.5 x 10^{-3}) and the wind speed is > 15 m/s and above the erosion threshold, we considered that, at this time, the wind was blowing in the direction of the prior sastrugi pattern before it shifted toward 160°.

(ii) For the second event, there is little snow flux during the period before the wind direction changed, so how confident can we be that we know the sastrugi orientation during this period? I think it is worth noting that there is a strong correlation between wind direction and drag coefficient during period A2, with the lowest drag coefficients occurring for a wind direction of around 140 degrees, suggesting that this was the sastrugi alignment before erosion started and the wind changed direction.

We have added the following sentence P6012, l05:
"[…] CDN10 was between 1.3–1.6 10^{-3}. C_{DN10} and wind direction were strongly correlated during this period, with the lowest drag coefficients occurring for a wind direction of around 140°, suggesting that this was the sastrugi alignment before erosion started and the wind changed direction. […].".

6. P6013, l13 onwards: I think the suggestion is really that the presence of blowing snow may affect C_{DN10} by introducing an additional source of surface drag. The wind profile measured well above the saltation layer will then reflect the total drag – i.e. that due to surface roughness plus the additional contribution from the saltation layer. I'm not sure whether you are suggesting that the Owen parameterization is wrong or that, in your observations, variability in C_{DN10} due to Owen's effect are swamped by those due to sastrugi alignment.

Significant variations in C_{DN10} during period A_2 are observed despite roughly constant friction velocity (from JD 285 06:00 UT to JD 286 12:00 UT, Fig. 4). In addition, even though the second-generation FlowCapt underestimates the aeolian snow mass flux, this sensor is a good detector of blowing snow occurrences [Trouvilliez et al. 2015], so we can be confident that erosion of snow was essentially absent during A_2. Thus, a saltation layer would barely exist, and the additional drag caused by saltating snow particles would contribute negligibly to the measured C_{DN10}. Given the previous remark (5.ii) suggesting that the wind direction (relatively to the orientation of the sastrugi) is the most influent factor dictating C_{DN10} during this period, we concluded that Owen's relation is not able to provide a good estimate of the roughness length (or C_{DN10}) during this period. The following order-of-magnitude calculation is proposed as an additional argument to our conclusions. The first half of period C_1 is characterized by a wind speed of about 27 m s^{-1}, an aeolian snow mass flux about 350 g m^{-2} s^{-1}, and C_{DN10} about 1.5 x 10^{-3}. Let's suppose the Owen's relation to be valid at our measurement site. We then need to determine the corresponding alpha coefficient (z_0=alpha u_*^2/g). In part C_1, C_{DN10} = 1.5 x 10^{-3}, that is, z_0 = 3.3 x 10^{-4} m, and an upper value of u_* for this period is 1.2 m/s. Dividing z_0 by u_*^2 and multiplying by g yields alpha = 2.7 x 10^{-3}. Now let's verify if with such a value for alpha, we can explain the increase in C_{DN10} during period B_1 with the Owen's

law: u_* reaches to 1.7 m s^{-1}, the Owen's law predicts z_0 = alpha x $(1.7)^2/g$ = 7.8 x 10^{-4}, that is, C_{DN10} = 1.79 x 10^{-3}. This last value is well below the observed one of 3.3 x 10^{-3}. We can still discuss the value of alpha, which is not really a constant, but this calculation shows that, even if Owen-like effects are present, they are much weaker than the increase due to the change in wind direction.

The conclusion is that, in our measurements, the variability in C_{DN10} due to Owen's effect are swamped by those due to sastrugi alignment. We have clarified the argumentation made from P6013, l13 onwards by replacing the following paragraph (starting at P6013, l23)

"However, aeolian snow mass flux peaks did not match C_{DN10} peaks. Moreover, significant variations in C_{DN10} were observed in the absence of aeolian snow transport (Part A2, Fig. 4). Here the height of the saltation layer was probably not a major determinant of roughness parameters. Owen's relation, which has often been invoked to describe momentum transfer over mobile surfaces, would thus not be confirmed by our measurements."

by this more complete one

"However, significant variations in C_{DN10} strongly correlated to the wind direction were observed during roughly constant friction velocity conditions and in the absence of drifting snow (Part A_2, Fig. 4). Moreover, aeolian snow mass flux peaks did not match C_{DN10} peaks for both erosion events. A simple order-of-magnitude calculation allows the assessment of Owen's relation during a period of drifting snow at our measurement site. From 06:00 UT on JD 88 to 06:00 UT on JD 89 during part C_1, the wind speed is around 27 m s^{-1}, drifting snow is active with an aeolian snow mass flux around 350 g m^{-2} s^{-1}, and u_* and C_{DN10} are about 1.1 m s^{-1} and 1.5 x 10^{-3}. According to Eq. (5), the corresponding value for α is 2.7 x 10^{-3}. Using Eq. (5) with this value of α to predict the increase in z_0 during period B_1 when u_* reaches 1.7 m s^{-1} yields z_0 = 7.8 x 10^{-4} m, that is, C_{DN10} = 1.79 x 10^{-3}. This last value is well below the observed one of 3.3 x 10^{-3}. Here the height of the saltation layer was probably not a major determinant of the roughness length, and the variability in C_{DN10} (or z_0) due to Owen's effect was presumably swamped by those due to sastrugi alignment."

7. P6016, l21: Presume you mean "real-time observations of the form of the sastrugi..."? This sentence has been changed to *"real-time observations of the distribution (size, abundance, orientation) of the sastrugi..."*

Response to reviewer #2

General points

The paper describes an interesting data set that documents the influence of a shift in wind direction on sastrugi alignment. Two short periods are described during which an increase in wind speed together with a shift in wind direction leads to a new orientation of the existing sastrugi within a time scale of hours. The main message of the paper is that this temporarily leads to a marked increase in form drag and a decrease in saltation mass flux. The paper is well written, concise and has rather the form of a letter than of a full-length paper. The results are described in sufficient detail and conclusions are supported through the material presented. In the discussion, it is speculated how often these events may occur. And along these lines, I have my major suggestion. I would encourage the authors to present more of the sufficient detail and conclusions are supported through the material presented. In the discussion, it is speculated how often these events may occur. And along these lines, I have my major suggestion. I would encourage the authors to present more of the valuable data from the met station and show how often roughness changes occur in the course of the whole Austral winter. If they also have FlowCapt data from the whole time period, I would present them, too. In summary, I believe that the potential impact of the paper could be much enhanced by 1) analyzing longer time series, which appear to be available and 2) publishing the data along with the publication. This would even be valid if no detailed documentation of the surface is available for most of the time.

The discussion of the effect of sastrugi realignment on a longer time-scale (the full year 2013 for this dataset) includes a seasonal discrimination (summer vs winter) because of changes in erodibility of the snow surface affecting the ability of the sastrugi to orient themselves in the prevailing wind direction. This is the hypothesis defended in another publication (Amory et al. [2016], submitted to Boundary-Layer Meteorology) currently under review, in which we present the whole dataset (including FlowCapt data) and refer to the conclusions of the present paper for a more general study. As a first step, in the present paper we suggest to present the data needed to discuss the aerodynamic adjustment process from two particularly well marked events only (as it is proposed in the current version).

The next Figure (Fig. 1 - from Amory et al. [2016]) shows monthly median values of drag coefficients for the period December 2012-December 2013. This figure highlights clear seasonal variations in drag coefficients over the observation period, with high monthly median values in summer for which the presence of abundant sastrugi was visually confirmed (Fig.2), and lower values in winter for which no observation of the occurrence of sastrugi at the surface could be performed. Strictly speaking, there is no proof that the adjustment process of the sastrugi is responsible for the low drag values along the winter. Nevertheless, we believe that the variations in roughness length presented in Fig. 4 can only be reasonably explained by a dynamical behavior of sastrugi-like roughness elements, in line with Andreas and Claffey's [1995] study. Then, considering that these monthly median winter values of drag coefficient compare well with the range proposed here for sastrugi-parallel winds (i.e., $1.3 - 1.5 \times 10^{-3}$), we concluded that the sastrugi streamlining process might be very active all along the winter season.
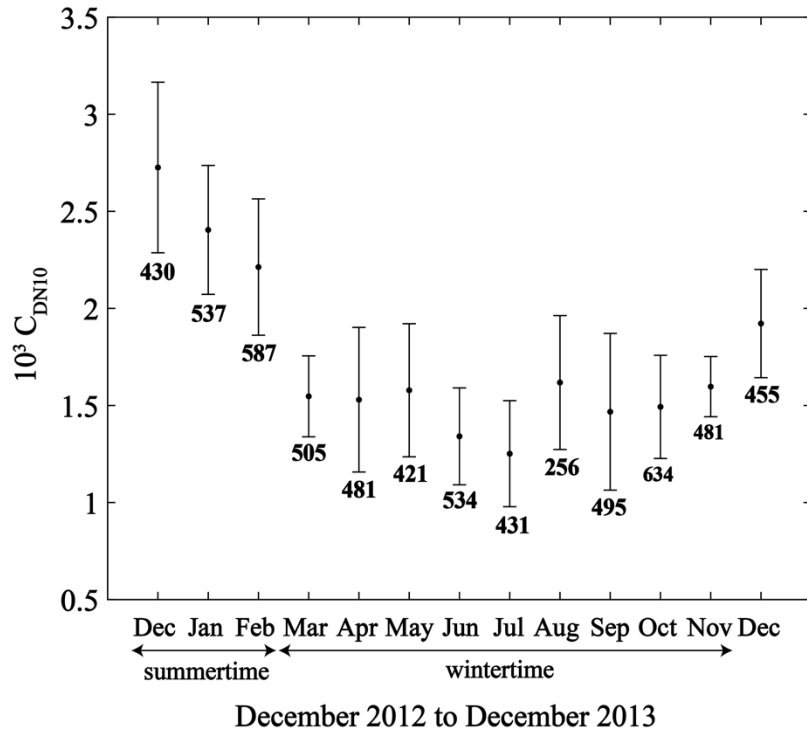
Fig. 1 - Monthly median values of profile-derived C_{DN10} from December 12, 2012 to December 30, 2013. The error bars are the onefold interquartile range of the monthly data. The number under each dot corresponds to the number of C_{DN10} values within a month.

Fig. 2 - Photograph of the sastrugied snow surface at D17 on 11 February 2013. The stake is 2 m in height.

Specific points

1. Abstract l. 19: is this just restating the increase (to 120%) from above? In this case, I would cancel the repetition.
   This sentence has been changed to *"as C_{DN10} increases, the aeolian snow mass flux can decrease (to 80%) [...]"*.

2. Abstract l. 24: orders of magnitude of what?
   We completed the sentence as it follows: *"[...], but also provide orders of magnitude in terms of changes in drag coefficients and aeolian snow mass fluxes as well as sastrugi streamlining timescales, [...]"*.

3. Abstract l. 27: I would add "...aeolian snow transport models and general drag parameterizations for weather, climate and earth system models".
   Changed accordingly.

4. Introduction l. 2: I don't think "metric-scale" is correct here. You probably want to say "scale of meters".
   This sentence has been changed to *"Sastrugi are elongated ridges of wind-packed snow 1 to 2 meters in length [...]"*.

5. Introduction l. 23: Very awkward and contradictory formulation, please rephrase. What is a "greater but slower decrease in the increase rate"?
   This sentence has been changed to the original sentence of Schmidt [1980] (see our response to specific point n°1 of reviewer #1).

6. p. 6009 l. 7 ff: Maybe also mention earlier FlowCapt validations?
   To our knowledge, the evalution of Trouvilliez et al. [2015] is the only existing one dealing with second-generation FlowCapt™ sensors. The original design resulted in significant errors in estimating the aeolian snow mass flux [Cierco et al., 2007]. The sensors used in this study are of a more recent design, which significantly improves problems with estimating aeolian snow mass fluxes [Trouvilliez et al. 2015]. To avoid a mix-up between the two generations of sensors, we propose to focus on the second-generation devices only.

7. p. 6007 l. 19 (and elsewhere): This is a logarithmic and not semi-logarithmic profile.
   Changed accordingly.

8. p. 6013 l. 7: What do you mean with "for a given set of particles…"
   This sentence has been replaced by *"for a given erosion threshold"*.

9. p. 6013 l. 13ff: I suggest that this effect is properly discussed and in more detail. First of all you should extend the discussion to Raupach (1991), who gave an improved relationship, which is more physical in terms of the feed-back on the flow, especially limiting the stress reduction close to the surface. This is quite important since the reduction of shear stress near the surface is crucial in limiting the growth of the mass flux (Groot Zwaaftink et al., 2014).
   See our response to the next comment.

10. p. 6015 l. 2ff: It is an open question in how far the shear stress at some height

can be used to predict the skin friction in case of surface roughness and other obstacles. See also the recent discussion on how to predict surface peak shear stress and surface shear stress distribution in case of obstacles in Walter et al. (2012).

The context of Raupach [1991] and Walter et al. [2012] regarding shear stress distribution at surface is quite different from our situation here because of the non-erodible character of roughness elements, while sastrugi are erodible roughness elements. Non-erodible elements such as rocks or vegetation mask a portion of the erodible substrate surface, causing a decrease of the exposed ground area per unit ground area compared to a bare surface [Raupach et al. 1993]. Therefore, by concentrating the force of horizontal wind gusts on their frontal areas, these non-erodible roughness features decrease the erosion potential of the ground that they lie on. Following on from Raupach [1991], Raupach et al. [1993] proposed to split the total surface stress over an erodible surface covered with non-erodible roughness elements into two components τ_R and τ_S acting on the roughness elements and the underlying (substrate) surface respectively, so that

$$\tau = \tau_R + \tau_S$$

This drag partition scheme is not fully applicable to a sastrugi-covered surface because the substrate surface and the overlying roughness elements share the same physical nature. Substantial differences in snow physical properties between wind-packed sastrugi and the surrounding substrate surface could be considered, but the occurrence of sastrugi alignment actually involves the occurrence of sastrugi erosion that precludes such a discrimination. Therefore, the masking effect can be ignored and the momentum exchange at sastrugi level can either result in momentum dissipation or erosion.

This implies that a more realistic prediction of surface shear stress distribution and, thus, erosion over sastrugi-covered snowfields would require time-dependent partitioning between the various sources of drag. Furthermore, the improved relationship concerning stress partition at the ground surface initially proposed by Raupach [1991] involves the Owen's relation, which does not fit our measurements (see our response to specific point n°6 from reviewer #1).

For all these reasons, we would like to refrain from mentioning previous work on stress partition in the discussion of the paper when dealing with non-erodible roughness elements.

Finally, we have added the following sentence P6015, l06:

"[…]. This is quite important since the reduction of shear stress near the surface is crucial in limiting the growth of the mass flux (Groot Zwaaftink et al., 2014). For erodible forms.. […]".

References

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