Response to Reviewer 2

We thank the reviewer for his many thoughtful remarks and suggestions. Here is how we address these.

The paper provides results of blowing snow and meteorological observations collected in katabatic wind area Adélie Land (East Antarctica) and comparison with global meteorological model and snow-pack model. The paper contributes to knowledge of blowing snow process and error estimation using atmospheric models that do not include wind driven erosion processes. The results are very interesting and appropriate for TC certainly worth being published, however the paper is not clearly finalised and sev- eral items (e.g. Crocus vs observation; Bulk vs profile methods; how improvement the models?) are introduced without a real discussion and conclusion.

Pag 2762 and everywhere The elevation distribution of blowing snow as surveyed by observation (Mahesh et al., 2003) or satellite images (Scarchilli et al., 2010; Palm et al., 2011) are not taken in account in the manuscript. SMB estimation in Terre Adélie has been reported in previous papers using AWS and ice core (Bintanja, 1998; Pettre et al., 1986; Frezzotti et al., 2004) provide complementary information to the presented result.

Here observations are reported within the lowest 7 meters above the surface. The used model parametrizes the surface fluxes only and does not describe the elevation or vertical distribution of blowing snow. A presentation / discussion of the elevation distribution of blowing snow is of interest on its own sake but we don't think it would bring much insight and additional value to the content of the present paper focusing on the very 1st meters above the surface.

Yes estimations of the SMB in Adélie Land have been previously reported but we don't see which complementary information they may provide in the framework of the present paper. The paper is not on the SMB but on processes related to blowing snow and atmospheric moisture which none of the cited paper address. Agosta et al. [2012] is the latest published report on SMB in Adélie Land, which duly refers to previous works. It is particularly cited here because it is the only one with spatial resolution information of interest for the present paper.

Nethertheless, references to Bintanja 2001 and Frezzotti et al, 2004 have been added in the text.

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Besides transporting solid water, the near-surface atmosphere transports more
water vapor than it would without blowing snow due to the sublimation of blown
snow particles. Some authors demonstrated through modelling studies that snowdrift
sublimation can exceed surface sublimation in coastal and windy Antarctic areas
\citep{Bintanja2001,Frezzoti2004}. In fact, the issue of blowing snow is not
limited to Antarctica, and historical studies  first took places in mountainous
regions. On the basis of direct in situ measurements, \citet{Schmidt1982}
calculated that sublimation amounts to 13.1 % of the blowing snow transport rate
in Southern Wyoming during blizzard events.
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Bintanja, R.: Snowdrift sublimation in a katabatic wind region of the Antarctic ice sheet, J. Appl.

Meteorol., 40, 1952–1966, 2001.

Frezzotti, M., Pourchet, M., Flora, O., Gandolfi, S., Gay, M., Urbini, S., Vincent, C., Becagli, S., Gragnani, R., Proposito, M., Severi, M., Traversi, R., Udisti, R., and Fily, M.: New Estimations of Precipitation and Surface Sublimation in East Antarctica from Snow Accumulation Measurements, Clim. Dynam., 23, 803–813, 2004.

Pag 2763 2.1 Observation data A figure with the geographic information of the site and katabatic wind drainage basin is helpful to the readability of manuscript.

Following your advice, a figure with geographic informations has been inserted.

Pag 2765 ECMWF appear to reproduce well only temperature, whereas wind (mainly in winter) is not adequately simulated. analysis.

The agreement is definitely not as good for wind as for temperature. However while temperature is generally well mixed within the lower boundary layer down to near the surface when wind blows, the wind necessarily reduces to zero at the surface. This induces stronger gradients which are harder to capture by a model. The text is changed to “ compare well with the observation for temperature and reasonably for wind”. The point is to highlight a contrast with moisture, which like temperature should not be as affected by gradient as wind, but which nonetheless disagree much more than not only temperature but also wind. The text is modified to account for less agreement for wind than for temperature.

Pag 2766 Is homogeneous ECMWF operational analysis during the analysed period? The “spin off” problem should be taken in account in the use of ECMWF

The ECMWF system to produce the operational analyses is obviously not homogeneous as this is the operational product which is continuously tentatively improved, both from the point of view of the numerical package (meteorological model, assimilation methods) and that of observation input (availability, methods for satellite data, etc). On the other hand, one major point is the spatial resolution which remained unchanged from January 2010 on. This is now reported in the text.

A significant spin-up (rather than spin off) problem with precipitation was raised in the 1st reanalyzes produced by ECMWF (ERA15, Genthon and Krinner, J. Clim, 1998). The problem was solved in the next reanalyzes (ERA40). Here, with the operational analyses, the cumulated precipitation over 3 years differs by only 3% whether the 6 or 12-6 hour forecast step is used. This is considered negligible for our application.

Pag 2768 line 8-15, it is very difficult to follow, rephrase

The paragraph has been rephrased :

Figure 4 shows the 2011-2012 records of observed relative humidity with respect to ice (RHwri) at the lower (0.87 m) and upper (6.96 m) levels on the mast. A 10-day running average is used to smooth out the shorter-term variability including diurnal and synoptic effects. All along the 2 years observations, relative humidity is very high in the range about RHwri ~ 70 %, and 10 % larger when measurements are performed close to the ground surface. A zoom on a summer episode and a winter episode is shown on figure 3. Very low RH values below RHwri = 30 % do occur, that one would expect to be related to katabatic winds, that is to be relatively dry in terms of RH, due to adiabatic warming as pressure increases downslope. Observations show that RH values close to or at saturation occur frequently as well, which is not a direct effect of katabatic process.

Describe the choice of blowing snow flux threshold of 300 g m-2 s-1

The choice is described lines 3-4 p 2768: ”is used here to highlight the saturation effect”. The value is admittedly “large” (line 20) to extract the cases most affected by blowing snow.

Temporal variability of blowing snow and relative RH during the two years should be shown.

We do show the temporal variability of RH on figure 3. However, this is smoothed with a 10-day running mean filter because shorter term variability would make the plot unreadable on a 2-year plot. Blowing snow has an even sharper short term variability which would equally show blurred on a 2-year plot. A 10-day running mean would make limited sense to illustrate that occurrences of blowing snow and high atmospheric moisture are related since the variability of both is much shorter than 10 days. This is why we elect to summarize the information of the relation between blowing snow and atmospheric moisture on figure 4, rather than tentatively comparing full time series of RH and blowing snow.

Pag 2770 Comparison with other atmospheric models are interesting, but is hanging without any discussion, develop or remove

While we do not think a full discussion and conclusion is appropriate here we agree that a warp up is necessary. We complete with:

All models thus lack a source of atmospheric moistening, and they fail to show a definite increase of atmospheric moisture with wind speed as observed. Among the possible interpretation is the fact that none of the models account for occurrence and evaporation of blowing snow.

Paragraph “4 snow-pack modeling” and part of “5 bulk and profile moisture flux calculation” should be in methods

The general presentation of the Crocus model, adaptations for antarctic environment and parameterization for blowing snow, the atmospheric forcing and generalities are indeed in the data and method section. Specific aspects to running the model at D17 and in particular parameter adjustment are provided in section 4 because, precisely, parameter adjustment requires that the model is run and thus that model results are presented. Much of section 4 is the presentation and discussion of the simulation results, which obviously do not belong to the data and method section.

However to improve the structure of the paper and then its readability, several paragraphs have been revised and reordered. Moreover sections have been explicitely divided into subsections to help the reader not to be lost. For each section, introductory sentences have been added.
We decided not to write one big Method section and one big Results section. We tried that but the

Method section was heavy and the final result was not satisfactory since results of section 3 helped to design the method for section 4, and results of both section 3 and 4 lead to the study discussed in section 5. Finally, section 2) *Data and Method* becomes section 2) *Data and Model* and presents the tools we used for the all study : observationnal data, meteorological analysis data and the snow-pack model. Section 4 to 5 now have their own method subsections.

Pag 2776 The flowcapt threshold 4g m-2 s-1 is two order of magnitude less than that used previous, explain the choice of thresholds.

… occurrences with and without blowing snow are distinguished using a Flowcapt threshold of 4 g/m2/s. This is much lower than the threshold used in section 3 to separate the strongest blowing snow cases. The threshold here allows to characterize a strong impact of even light quantities of blowing snow on flux estimation by the profile method. Figure 8 shows an approximately equal number of cases above or below the threshold.

Pag 2776 and 2777 It is not clear why MO theory that does not include blowing snow and katabatic condition could be applicable in D17 condition.

Even if we are not sure it will work, we though it is worth trying. Indeed, as you know, MO similarity relationships are widely used either with observational data for example to compute fluxes with the profile method or in atmospheric model. We think that any attempt to evaluate the robustness of the MO theory or the profile method based on it is of interest.

Classical arguments supporting the inadequacy of the MO theory in katabatic conditions are generally based on two arguments : the presence of a low level jet and the presence of a very stable boundary layer. Generally, on km-sized glaciers, katabatic jets are located around 10 m above the surface(as in the case presented by Grisogono et al ,2001 and Denby et al, 2000). These very low level jets imply a collapsing of the surface layer in which the MO theory is applicable.
In our case, Figure 9 shows the katabatic nose is far above the tower. The wind exhibits a very nice logarithmic profile on the tower, supporting the fact that the tower is in the surface layer. Moreover, neutral conditions prevails on this layer. We think that MO theory does apply in OUR katabatic conditions when no blowing snow. So, the profile method should do as well, but our results show : " provided that measurement uncertainties are very small ".

In contrast, we think the validity of the MO theory is questionable in case of blowing snow.
This is a point raised by our results, and discussed at the end of the paragraph.

Pag 2778 It is not correct to use an average of snow fall, see seasonal variability of precipitation in Antarctica (e.g. Marshall, 2009; Bromwich et al., 2011)

We agree that the average snow fall is a very crude estimation of the instruments height uncertainty. But, this part is about orders of magnitudes. As stated 2 sentences later "...this is a debatable choice … but the fact that the impact of height errors are weak compared … is way beyond this uncertainty". This is the important point here.

Pag 2779 Fig. 8 shows a very small agreement between Obs profile and Crocus bulk, also in absence of wind.

Bulk and profile are parameterized methods, none of which if fully flaw free even not considering the blowing snow issue. There are indeed also differences in low wind / blowing snow content cases but they are much weaker than when blowing snow occurs. We state (lines 18-19 p 2776) that “The agreement … tends to be better when no blowing snow is detected”. We don't go into the discussion of why the 2 methods disagree somewhat even without blowing snow as this is beyond the scope of the paper.

Pag 2780 Line 7-9, it is not clear the meaning

The paragraph have been modified :

Uncertainties on blown snow concentration measurements (section \ref{p21_obs}) are too large to expect for a reasonable estimation of the density gradient. Consequently this particular point could not be addressed here.

Fig. 6 Gill, red and black curves are not visible

This is precisely the point: there is no sensitivity to wind speed at this site as shown by the three curves being merged. This point is reported (lines 12-13 p 2771).

Fig 7 it is not clear the different initial condition of the two red line of Crocus

The initial conditions are the same for all runs, but the reference (0) for snow height variations is arbitrary. As stated p 2773 line “The reference snowpack is that of 1 January 2011”.