Please find below the list of changes and the responses to the reviewer's comments.

List of changes

- Title changed
- Abstract entirely rewritten.
- Several paragraph on part 1,3,4,5 have been rewritten and reordered aiming to improve the clarity of the message and the readability.
- Section 2 becomes Data and Model.
- Section 3 to 5 have been divided in 2 or 3 subsections.
- A map of Terre Adelie, locating D17, D10 and D47 has been added.
- On figures, unities appear into brackets
- Linear trend on figure 11 (now figure 12) dropped.
- Section 5.1 : some more words concerning the two method used to compute the turbulent fluxes have been added.
- A few more references have been added in the introduction.

Response to Review 1 :

P2760
title: I suggest to change the title, this is too specific and 'issues' is a vague term. What about 'Blowing snow in coastal Adélie Land and its impact on atmospheric moisture'?
abstract: this is characteristic for the remainder of the text. Try to structure the abstract, such that one sentence follows from the other. This version is extremely chaotic.

We agree to change the title, we liked the first part of your proposal but the last part (eg 'its impact on atmospheric moisture') seems to restrictive for us since it suggests that the study focuses on one impact whereas we discuss three impacts. Consequently we chose the following title :
Blowing snow in Adélie Land, coastal Antarctica : Three atmospheric moisture issues.
The abstract has been rewritten. We tried to explicitely describe the three impacts of blowing snow we worked on. We hoped it is less chaotic !

Three years of blowing snow observations and associated meteorology along a 7-m mast at site D17 in coastal Adélie Land are presented. The observations are used to address 3 atmospheric moisture issues related to the occurrence of blowing snow, a feature which largely affects many regions of Antarctica: 1) Blowing snow sublimation raises close to saturation the moisture content of the surface atmosphere, and atmospheric models and meteorological analyzes that do not carry blowing snow parameterizations are affected by a systematic dry bias; 2) While snowpack modeling with a parameterization of surface snow erosion by wind can reproduce the variability of snow accumulation and ablation, ignoring the high levels of atmospheric moisture content associated with blowing snow results in overestimating surface sublimation affecting the energy budget of the snowpack; 3) the well-known profile method to calculate turbulent moisture fluxes is not applicable when blowing snow occurs, because moisture gradients are weak due to blowing snow sublimation, and the impact of measurement uncertainties is strongly amplified in case of strong winds.

Comments relative to the structure :
"I think that the paper needs to be thoroughly
revised in terms of its structure. The text continuously jumps from one subject to the other, and from methods to results, which does not enhance its readability. "

P2768 L7: why not make a 'Results' section, with all current sections 3-5 as subsections?
Following your statement, the structure has been revised.
Several paragraphs have been revised and reordered in order to improve the readability and the clarity of the text. 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 have their own method subsections.

L5-10: on L1 you mention a result of models, and here you write that models are needed. Restructure: first this, than the results.
Ok, you are right, it is better after reordering the ideas as you proposed.

L21-L22: this is important information, but at this point it is unclear how you will overcome this problem. This is described further in the text, which leaves this sentence a little lonely. I suggest combining both pieces of text.
The structure of the paragraph has been modified. We hope it is more convenient now.
P2766 L5-8: here you refer to the introduction and to section 3 at the same time, whereas you should focus on section 2, i.e. the methods. This is typical for many parts of the text.
The reviewer is right, using data presented in the paper to illustrate a point in the data and methods section is premature. Thus we removed the 2 sentences "As discussed … no exception".
P2769 L5-20: once more, this distracts the reader. These are not results, but a comparison of your results with existing literature.
These lines along with lines 7 to 27 on the precedent page have been rewritten and reordered to be less chaotic and to improve readability. These lines are now part of subsection *3.1 Relationship between atmospheric moisture and occurrence of blowing snow in the observations*
P2770 : L4-18: again, this is not a result. It would fit to the methods and/or the figure caption.
This part has been moved to the figure caption.
P2772 : this entire page can be deleted or moved to the methods (in short form) or appendix. This distracts the reader from the main story. P2773 until L14: same for this part
These details have been inserted into a new subsection : *4.1 Method : Model Adaptation for Antarctic snow and blowing snow parameterization* .
L25 – P2775 – P 2776 (L6): Model details should be moved to the methods.
The whole section 5 has been reordered and cutted into two subsections. Model details are inserted in subsection *5.1 Method*.

Other specific comments :

L3: East Antarctica
L10: define 'subsaturation'
L14: up to a point
L14: becomes an issue
L16: winds
-obsolete since we changed the abstract.

L22: what is 'frequent' and 'persistent'? specify
We used *frequent* to mean that katabatic events often occur and *persistent* to mean that these events last for a long time. We prefer keeping the two adjectives since we think that they emphasize two

different features of Adélie Land katabatic winds.

L25: give a reference
It is not clear what kind of reference the reviewer is requesting. If this is about blowing snow possibly originating from both snow fall and erosion of surface snow, this is a common fact and there is no outstanding reference for this. For instance, NOAA's national weather service glossary also states that “Blowing snow can be falling snow or snow that has already accumulated but is picked up and blown by strong wind. This same definition is also quoted by wikipedia. This does not seem to require a reference.

P2761
L1: models? Specify and quantify the impact
Ok. The following sentence has been added in order to quantify the impact.
The contribution of eroding and blowing snow to the surface mass balance (SMB) of Antarctica is estimated using models. The ranges varies from one model to another. Lenaerts et al 2012b computed that sublimation of blown particles removes almost 7% of the precipitation, considering the whole ice-sheet. Gallée et al, 2001 found about 30% along a 600 km transect in Wilkes Land.

L18: poor weather
We droped 'worst weather conditions' for 'harsh weather conditions'.

P2762
L4: through decreasing its buoyancy
We agree that mechanical energy for katabatic flow results from negative buoyancy. Decreasing buoyancy for something that is not buoyant (that is, rather, sinking) does not sound right. Rather, we replace “increasing its density” by “further decreasing its negative buoyancy”.
L5: the air is even more enhanced
Sentence replaced by :
The negative buoyancy of the air is further increased

L10: calculated
ok

L11: Wyoming? This needs a introductory sentence. Why look talk about Wyoming if your study is on Antarctica?

We decided to keep the reference on Schmidt's study in the Wyoming.
Indeed, we think it is important to note that the blowing snow issues are not restricted to Antarctica since they may have important implications in other regions. Historical studies as Schmidt, 1982 did not took place in Antarctica. Moreover, not so many studies of this kind (leading to an estimation from observatons of the rate of sublimation for airborne snow particles) have been published.

Further to your comment, the paragraph has been modified, introducing other studies from Antarctica and with an introductory sentence to justify our choice.

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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.

L16: this is partially compensated. . . How do you know? Quantify and/or give a reference.
Gosink, 1989 supports our statement :
“As the air and snow crystal mixture moves downslope, the ambient air temperature increases owing to adiabatic compression. The adiabatic warming decreases the relative humidity, favoring the sublimation of snow crystals. ”

P2763
L3-4: this is too much detail, especially for an introduction
ok , this has been removed.

L18: seldom = rare
ok

L18: remarkable persistence of strong winds
Yes you are right, constancy should be reserved for direction.

L20: They proved to be
ok

P2764
L2: interrogated = sampled
L4: run = were set up
The sentence has been rewriten :
Data are sampled with a 10'' time step, the 30-min statistics are stored by a Campbell CR3000 data logger.

L19: access was possible
ok

P2766
L10: specify the resolution of both
Both resolutions have been specified.
(about 70 km for ERA-interim versus about 16\,km for operationnal analysis since 2010)

L13: than grid points located inland
ok

L24: this is unclear. First you discuss the importance of having high resolution, and it appears that you still use precipitation from ERA-Interim (which is probably the variablemost sensitive to resolution). Explain.
General remark: please give a reference when using ECMWF data.

Although there are common references for reanalyzes products, which are widely used for research puropose (e.g. Dee et al. For ERA-I), we do not know of an integrative reference for them. The fact is that we do not use the reanalyses, we use only operationnal analysis. The website has been added as footnote for want of reference.

Moreover, for better clarity, the paragraph has been slightly modified. We hope it is better now !

The operational analyzes are shown here, rather than reanalyzes
→ The operational analyzes are used here, rather than reanalyzes

The ECMWF analyzes are used in Section \ref{p4_crocus}, as surface atmospheric boundary conditions for a snow-pack model described in Section \ref{p22_ecmwf}. While the observed temperature, moisture and wind could be (and are) used, the snow-pack model also needs input of precipitation, radiation and cloudiness. This is obtained from the meteorological analyzes. While cloudiness is really analyzed, precipitation and radiation are not. The 6- and 12-hour forecasts, produced by ECMWF with model initialization by the analyzes, are used instead.
→
The ECMWF analyzes are used in Section \ref{p4_crocus}, as surface atmospheric boundary conditions for a snow-pack model described in Section \ref{p22_ecmwf}. The snow-pack model needs input of near-surface temperature, moisture and wind but also precipitation, radiation and cloudiness. For the first group, observationnal data are used alternatively with meteorological analyzes. For the second group, (comprehensive observationnal data sets are not available) only meteorological analysis are used. It may be important to note that cloudiness is really analysed whereas precipitation and radiation are not, they are in fact forecast by the ECMWF model initialized by the analyzes.

L28: in various studies
ok

P2767
L2: a horizontally one-dimensional, vertically multi-layered physical model
ok
L3: calculates the surface snow height at hourly time steps
ok
L5: disposal = balance
ok

P2768
L13: values (below 30
ok
L14: values
ok
L24: remove 'or null'
ok
L26: homogeneous = constant
The sentence has been modified.
Moistening by the sublimation of the wind blown snow particles results in the vertical profile to be much more homogeneous.

P2769
L1-4: remove, unnecessary
The sentences have not been removed in order to respond to the comments of the second review.

P2770

L6: for two climate models
ok

L23: too dry
ok

L27: AMRC AWSs ? give a reference/link
A link to the official website has been added in footnote at the first occurrence of the acronym AMRC AWS.

P2771
L2: located on the Ross ice shelf
ok

L4: lost to the surface
'lost for the surface' has been changed to 'lost by the surface'.

L25: not difficult in a 10 year time series
To be more precise, the mean 2010 accumulation along the GLACIOCLIM-SAMBA stakes system was not only one of the highest but the highest on record.

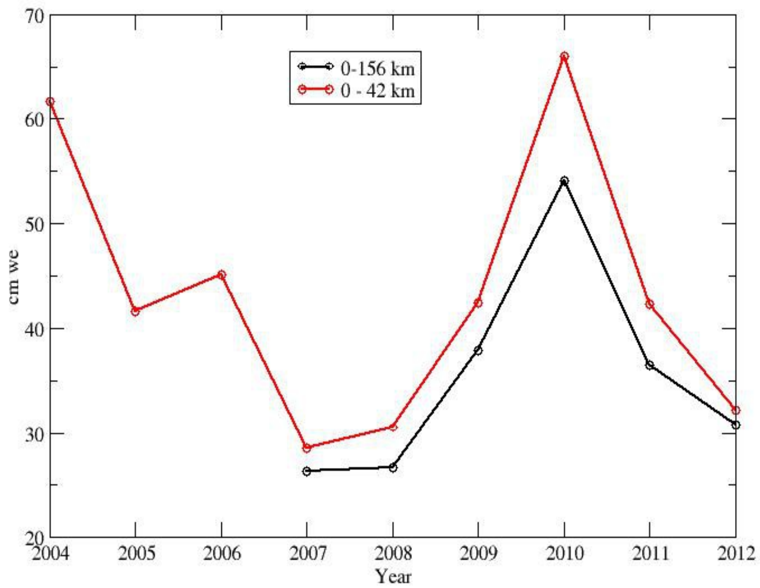

Illustration 1: Accumulation

Text 1: Accumulation along the GLACIOCLIM SAMBA stakes

P2774
L18: four simulations
ok
L20: I don't see a small impact. Sublimation changes with a factor of two!
The impact is small. We suppose the reviewer has compared the lines S2 with S1 or S4 in the table. The lines to be compared are S2 and S3. The assosciated fluxes are -11.1 W/m2 and -13.0W/m2. We support the impact is small. For a better clarity, we had the label of the lignes (S2 and S3) within brackets in the text.
L22: unexpectedly

ok

L25 – P2775 – P 2776 (L6): can be omitted entirely. The MO-theory is known and does not have to be explained.
We prefer keeping the details of the profile method. Indeed we think that the equations illustrate the text ; having the equations in mind helps to understand why uncertainties are enhanced in case of strong winds and weak gradients.

P2776
L18: FlowCapt threshold. . . can be moved to methods/figure caption
The sentence have been kept here in order to respond to the comments of the second review.

P2779
L10: I agree with this statement in terms of RH, but not in terms of temperature. I would advise to give the statistical significance of the linear trend, since I am quite sure it is not significant for temperature. In that case, I suggest removing it.
The reviewer is right, the linear trend is not of statistical significance. We removed it. The goal was not to exhibit a linear trend but to show the decrease. The figure has been arranged.

FIGURES
General comment: put the units of the displayed variable between brackets to enhance readability
ok

Figure 10: dispersion = variability
The word dispersion has been 'dropped' for 'spread'.

Response to Review 2

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.

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.

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 av-erage is used to smooth out the shorter-term variability in-cluding diurnal and synoptic effects. All along the 2 years ob-servations, relative humidity is very high in the range about RHwri ~ 70 %, and 10 % larger when measurements are per-formed 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 rela-tively dry in terms of RH, due to adiabatic warming as pres-sure increases downslope. Observations show that RH val-ues 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”.