We thank M. Lehning for his insightful comments. We answered below to all his points. His comments are in bold while our answers appear in normal font.

**The discussion paper presents a fully coupled atmosphere – snow model, which is able to treat surface exchange processes, in particular drifting and blowing snow, in much detail. This is the first fully coupled model in the sense that three-dimensional meteorological RANS simulations have been extended to include the transport modes saltation and suspension and the sublimation of blowing snow. The novelty is that most important feed-back mechanisms, such as temperature and humidity effects of sublimation, between snow and atmospheric dynamics are included. The paper describes the model and presents validation against field studies. Measured wind, snow transport and snow distribution are compared to model predictions with fair overall success. Very importantly, model results on sublimation of drifting snow are discussed in much detail. Since the paper aims at presenting a fairly complete overview of the model formulation, the evaluation and discussion of the individual parts is (necessarily) somewhat short.**

**I would suggest that this discussion is for some important parts extended to increase the scientific value of the paper. In particular, it should be discussed, in how far the "double moment" approach chosen is changing results when compared to a simpler version with a representative radius for blowing snow particles, as it has been used for other studies. A further special feature of this model is the deployment of the "Canopy" vertical sub-model above the surface. This presents another example, for which the reader would like to know in how far the results would be different if this extra effort had not not been executed. In general, the paper could be improved if the effect of individual model features is discussed with respect to the final results. In this context, model features should be compared to the effect of resolution. As has clearly been stated by the authors in the discussion of the simulated snow distribution, the resolution is still insufficient to capture smaller scale drift features. Therefore, a natural question, which the authors need to answer or at least discuss, is in how far the results on sublimation will depend on this resolution as well.**

**Overall, the paper is well presented presents a wealth of rigorous work and results and should be published in TC. It is not a typical case of "major revision", however, to implement the suggestions made above and below will require a lot of extra work. A lot of smaller (not only language) mistakes have been marked and**

**additional comments have been made in the annotated version of the paper (attached).**

We thank M. Lehning for his comments that allowed us to clarify the importance of some model features in the new version of the paper. For this purpose we ran additional simulations to discuss the effect of individual model components. We decided to adopt a 1-D framework for these simulations since 3-D simulations would have required a too large amount of computational time. New 1-D simulations are presented in Sect. 5. Sect 5.1 describes model configurations while results are detailed in Sect. 5.2. Model results are evaluated using vertical profiles of blowing snow radius and fluxes measured by three SPCs during a blowing snow event different from the one simulated in 3-D.

The first set of simulations aims at justifying the deployment of the 1-D SBL scheme Canopy at the interface between Meso-NH and Crocus. Figure 5 shows that the model overestimates the snow mass in suspension when the height of the first atmospheric level is set at 3 m and that Canopy is not used. Using such configuration in 3-D simulations would overestimate the redistribution of snow by the wind. Using a version of the model with a very high resolution close to the snow surface (first level at 15 cm, Canopy not used) provides vertical profiles of fluxes and radius in good agreement with observations. Finally the use of Canopy (first Canopy level at 15 cm) in a model configuration with a first atmospheric level at 3 m gives similar results to the configuration at very high resolution. Therefore Canopy appears as a solution to provide a good estimation of the snow mass in the atmosphere without the need of a too high resolution close to the ground in the atmospheric model. Such configuration would prevent the use of Meso-NH/Crocus in complex terrain because of the computational cost. Furthermore Canopy allows the model to capture in a prognostic way the strong gradient of blowing snow concentration close to the ground and makes possible comparisons with field measurements of blowing snow fluxes.

The interest of the double-moment scheme is also illustrated. Such scheme allows the model to capture the decrease of mean particle radius with increasing height observed with the SPC (Fig 5 Left). As suggested by M. Lehning, we also compared the approach using the double-moment scheme with an approach using a representative radius for the computation of blowing snow sublimation. Fig 6 shows that the second approach tends to give higher sublimation rates close to the ground than the double moment scheme. This is due to the fact that the second approach does not represent the vertical profile of blowing snow radius. Therefore the double-moment scheme appears as a benefit in a model that represents vertical the layering of blowing snow properties close to the ground. Note that our paper does not aim at presenting a detailed analysis of the different formulations used in the computation of blowing snow sublimation. An increase in the value of the representative radius would for example give total sublimation rate closer to the double moment approach.

The effect of model resolution on the sublimation is mentioned in the Sect. 7.1 where results concerning blowing snow sublimation are discussed. We refer to the previous study of Bernhardt et al (2010) that mentioned the dependence of simulated sublimation rates to the model resolution. However we did not quantify this effect since 3-D simulations would have been required. This will be the topic of a future study.

**1) Title: I would mention "sublimation" in the title since it is a major point in the analysis. Maybe "Simulation of drifting and blowing snow transport and sublimation in alpine terrain ..."**
We agree with M. Lehning and decided to mention the term "sublimation" in the title. The new title of our paper is: "Simulations of wind-induced snow transport and sublimation in alpine terrain using a fully coupled snowpack/atmosphere model".

**2) Abstract: Add "over the calculation domain" when stating the reduction in deposition and specify that the 5.% are based on snow mass (not snow height or something else).**
The sentence has been modified in the Abstract according to this comment.

**3) Eq.9: You should motivate this empirical equation by saying that it describes the transition between Stokes' regime for laminar drag and turbulent drag and is thus applicable for particles of all reasonable sizes (At least if I understood correctly and this is the case).**
Following this remark, we added two sentences to clarify our choice for the formulation of the settling velocity: *This expression for $v(r)$ represents the transition from laminar regime where Stokes Law applies ($1 \leq r \leq 30$ μm) to turbulent regime. It is therefore suitable for blowing snow particles since it covers their typical range of size (20-200 μm, e.g. Budd, 1966).*

**4) Model description: Since the description of the development of the double moment drifting snow model is largely identical to the original presentation in Déry and Yau (2001), this part can be shortened.**
We removed a part of the model description similar to Déry and Yau (2001). The new description of the model formulation for the suspension layer consists in 2 subsections instead of 3 in the previous version: (i) Double-moment scheme and (ii) Blowing snow sublimation.

**5) It is clear that not all details of such a complex model can be discussed. However, at places some more in depth discussion of certain assumptions would be helpful. For example, the presentation of the saltation model and its connection to suspension relies on empirical formulations for the height of the saltation layer or the questionable assumption of a wind-independent velocity of particles in the saltation layer. Such assumptions should at least be qualitatively discussed. Also, critical values such as the assumed mean radius in saltation should be given, or it should be mentioned that they are given later in the text.**
In the new version of the paper, Sect. 7.2 presents the current limitations of the coupled model. The second paragraph focuses on the limitations of the formulations of the saltation layer (height of the saltation layer, velocity of particles in the saltation layer). We also added a sentence that refers to this paragraph of discussion when presenting the model formulation for the saltation layer (Sect. 3.2.2).

We also mentioned directly in the model description our choices concerning the values of some model parameters (Sect. 3.1.1 for shape parameter, $\alpha$, and Sect. 3.2.2 for mean radius in saltation $r_{\mathrm{m}}$).

**6) Canopy: The use of this sub-model is a critical feature of the total model assembly. It should be better motivated by a scale analysis that shows (i) that the assumption of a stationary wall function formulation is insufficient and (ii) that vertical exchange dominates horizontal exchange under the scales of consideration. See also general comment on critical model features above.**
As mentioned above we carried out 1-D simulations to better motivated the use of Canopy at the interface between Meso-NH and Crocus. Results are presented on Fig. 5. We believe that this figure provides a good illustration of the necessity of representing the strong gradient of blowing snow concentration in the current formulation of mass exchange in Meso-NH/Crocus.

**7) Pattern comparison: In the section, in which you compare the simulated snow distribution to a measured one from a different event, you should better justify, why you did not simulate the event from which you have data. At least, you could reference work that shows that deposition patterns are similar for different storms (e.g. Schirmer and Lehning, WRR, 2012; Schirmer et al., WRR, 2012).**
The evaluation method of Meso-NH/Crocus is now presented in Sect. 4. In this section we justify why we simulate the event of 18-19 March 2013 from which we do not have TLS data. It is written as follows in the new version of the paper:
*We selected the 18-19 March event as a case study for two main reasons: (i)*

*the shorter duration of this event compared to the 22-26 February event (Table 1) makes it less computationally expensive to simulate in 3-D and (ii) the occurrence of snow transport without concurrent snowfall during most of the event allows us to focus the validation on the blowing snow scheme detailed in this paper.*

To better justify the comparison of patterns of snow erosion and deposition, we add a reference to the work of Schirmer et al (2011) in Sect 6.4: *This event presents similar conditions to our case study in terms of mean wind speed and direction (Table 1). Schirmer et al. (2011) have shown that individuals storm arriving from the same direction produce similar patterns of snow depth changes at the Wannengrat catchment (Swiss Alps).*

**8) Discussion of sublimation: In general, this section is very good and probably the most important result presented. When comparing event vs. seasonal rates of sublimation, you should also state that seasonal rates must be lower simply because drifting and blowing snow is only present during a fraction of the total time. This is confirmed by a recent publication of Groot et al. (WRR, 2013), in which seasonal sublimation rates are compared to rates for a single event. Therefore, one can also clearly state that the high rates obtained for the Berchtesgadener Alps but probably also those for the Arctic (albeit the higher wind speeds there may make a difference) are not entirely compatible with your results and those by Groot Zwaaftink.**

Results and discussions concerning blowing snow sublimation are now presented into separate parts following the comments of Reviewer 2 (Sect. 6.5 for results and Sect. 7.1 for discussions). In the discussion we still present a comparison with results of previous studies concerning seasonal sublimations rates. The difference between rates for a single event and seasonal rates is now clearly mentioned and we referred to the recent study of Groot Zwaaftink et al. (2013). The new version of this paragraph is written as follows:

*Other studies focused on the impact of blowing snow sublimation over a whole winter at different spatial scales. For the Berchtesgaden park in Germany (210km2), Strasser et al. (2008) have found that 4.1 % of snowfall is lost by blowing snow sublimation. When including gravitational snow transport, the seasonal average loss is lowered to 1.6 % of annual snowfall for the same area (Bernhardtet al., 2012). In the Canadian Rocky Mountains MacDonald et al. (2010) estimated that sublimation losses reach 17 to 19 % along a mountain crest (length: 210m). Based on our results for a single event (reduction in snow deposition by 5.3 %), we can expect lower seasonal sublimation rates at Col du Lac Blanc since blowing snow events occur only during a fraction of the total time (10 % on average at Col du Lac Blanc, Vionnet et al., 2013). Groot Zwaaftink et al. (2013) confirmed this state-*

*ment for the Wannengrat alpine catchment. They found that only 0.1 % of snowfall is lost by blowing snow sublimation over a winter while snow deposition can be reduced by 2.3 % for a single event (GZ11).*

**9) Self regulation of blowing snow sublimation: Since the unrealistic result of total sublimation reduction for drifting snow has been obtained (in my opinion) by a problem with the vertical model structure (Bintanja, 2001), I suggest to cancel the corresponding sentence. I think your results are very realistic.**
Following the suggestion of M.Lehning, we removed the sentence concerning the "self regulation" of blowing snow sublimation in the discussion.

**Please also note the supplement to this comment: http://www.the-cryosphere-discuss.net/7/C1466/2013/tcd-7-C1466-2013- supplement.pdf**
We corrected all the points mentioned in the supplement except:

- Page 5 Point 2: the naming "Section" and "Sect." used in our paper is coherent with the requirements for Authors available on the journal website.

- Page 7 Point 1: we use the abbreviation "yr" for "years" as asked by the Editorial Board.

Our corrections appear in red in the reviewed version of the paper.