We thank Reviewer 2 for his valuable contribution. We answered below to all his points. His comments are in bold while our answers appear in normal font.

The paper by Vionnet et al presents a new scheme to simulate wind-induced snow transport. The model, probably intended for operational use, is the first model directly coupled to a meso-scale model.

The model is presented in great detail, and as such clearly explained. The validation of the model is rather superficial. Jus on event is shown, with no quantitative statistics.

It is pointed out (p2196, l 16) that Meso-NH can be used up to 10 m resolution. Why is the model not run also at this resolution to compare to measurements?

Meso-NH can be used at very high resolution in complex terrain thanks to its 3-D turbulence scheme but there are some constraints linked to its dynamics. Due to the anelastic constraint, the pressure is diagnosed by solving an elliptic equation. In presence of steep orography, the convergence of the pressure solver is more difficult to reach. Specifically, the major limitation of this system is not in terms of slope, since flows over slopes of 70° have been simulated satisfactorily, but in terms of slope discontinuity: the topography used in the model must avoid a cliff-type behaviour. The second constraint is linked to the eulerian numerical schemes, that limit the time step due to the Courant number.

For example, Amory (2012) ran Meso-NH at 12 m resolution around Col du Lac Blanc. His simulation lasted 45 minutes with a time step of 0.025 s. Such duration is not long enough to simulate the total duration of a blowing snow event (average duration: 19 hours at Col du Lac Blanc, Vionnet et al, 2013). Furthermore he showed that the border of the computational domain must be chosen carefully to avoid regions where slopes are too steep in the lateral boundary conditions. Indeed such regions generate numerical instabilities.

Simulations at 10m resolution are now mentioned in the reviewed version of the paper as perspective for future model use. It appears in Sect. 7.2 where model limitations are discussed and in the Conclusion. We also removed the reference to the work of Brun and Chollet (2010) (p2196, l116 of the initial version) and referred only to the work of Amory (2012) at Col du Lac Blanc.

On p 2208, l 13 ff, it is mentioned that "intensive measurement campaigns have been performed..." Why are these data not used to evaluate model performance?

Following this remark and the comments of Reviewer 1 concerning the

model's sensitivity, we used SPC data collected during a second blowing snow event to extend the evaluation of model performance. Results of 1-D simulations are now presented in Sect 5.2. They are compared with SPC measurements of blowing snow fluxes and radius.

The model evaluation presented in this paper uses only data collected during blowing snow events without concurrent snowfall to focus the evaluation on the blowing snow scheme presented in Sect. 3. Simulation of a blowing snow event with concurrent snowfall required primary a good estimation of snowfall by the microphysical scheme of the model. For the first evaluation of the coupled model we decided to avoid this additional level of uncertainty. Simulations have been run already for a blowing snow event with concurrent snowfall observed during the measurement campaign in 2011. Results will be published in a coming paper and will constitute the next step in the model's evaluation.

The section "Results and discussion" is difficult to read, and mostly results. I expect from a "Discussion" that the results are put into context to previous work. This is mostly missing. I suggest that this section is completely restructured, and results and discussion put into separate sections. I also would expect that the model compares its improved (?) performance to other models.

We agree with Reviewer 2 that the initial version of the paper was not clear enough. Therefore, following his suggestion, Results and Discussion have been separated. The new version of the paper follows also the suggestion of M. Lehning:

- Sect. 5: results of 1-D simulations
- Sect. 6: results of 3-D simulations
- Sect. 7: general discussion

Sect. 7 compares results concerning blowing snow sublimation with previous studies. Its also detailed the main limitations of the coupled model.

The model evaluation is illustrated by comparing what the authors call "an indirect comparison". the results of Fig. 7 are impressive (also a quantitative statistic is missing). But how compare these number to the integrated results and true distribution? The section is also very qualitative, what the authors consider "satisfactorily" (p 2212, l 9), "well" (p 2212, l 23) has to be cast into statistical terms.

Following this remark, we provide in the new version of the paper quantitative statistics concerning the total snow mass transported at two levels

above the snowpack (Table 3) and a detailed analysis of blowing snow fluxes per category of wind speed (Table 4). The results are presented in Sect 6.3. The final evaluation of the 3D simulation combines qualitative and quantitative analysis. Quantitative statistics have been computed for the wind speed and direction at three AWS (Table 2) and for blowing snow fluxes at the pass (Table 3 and 4).

Correlations of simulated and observed change in snow height have not been computed contrary to Mott et al. (2010) and Schneiderbauer and Prokop (2011). Indeed Fig. 10 compares the pattern of snow erosion and deposition for two different blowing events. We clearly mentioned the limitation of this approach in the text (Sect. 6.4): *The comparison presented in this section cannot be considered as a formal evaluation of the model ability to simulate snow redistribution but aims rather at exploring what is possible with the current model resolution of 50m.*

In fact, it remains unclear if the data shown are at all connected to the text, as in the text they write "the event of 22-26 February 2011", and in the figure the write "snow depth difference measured by TLS at an horizontal resolution of 1m between 28 February 2011 and 17 February 2011" (which seems to be more than a typo by the reviewer).

The data presented in the initial version of the paper were coherent. Indeed the map of snow depth difference for the event of 22-26 February 2011 has been established based on TLS measurements collected before (17 February 2011) and after (28 February 2011) the event. No TLS data have been collected for dates closer to the event. TLS measurement require the presence of operators on the experimental site and this was not possible on a daily basis. We add a sentence in the reviewed version of the paper (Sect. 4) to clearly mention the dates when snow depths have been measured by TLS: *Snow depths were measured over an area of 0.54 km^2 around the pass before (17 February 2011) and after (28 February 2011) the first event. Note that TLS measurements are not available for the second event.*

Smaller remarks

p 2194, l 15 The word "interactively" is used in an unusual way. Probably you mean "coupled". I could not see any interaction by a user of the model. We agree with Reviewer 2 and believe that the use of "interactively" in the initial version of the paper was confusing for the reader. Following his suggestions, we modified all the mentions to "interactively" and "interactive" into "in a coupled mode" and "coupled".

p 2194 l 24 The sentence "These studies ..." and following is very wordy, but does not mean much. Probably you mean: We

understand now the importance of the main processes (which) to simulate blowing snow in mountains. Following this remark, we changed this sentence and modified also the previous sentence according to: *"However, previous works have shown that atmospheric models can be run at high resolution in complex terrain to simulate in coupled mode meteorological situations such as wildland fire (Mandel et al., 2011, resolution of 100 m) or scalar dispersion (Michioka and Chow, 2008, resolution of 25 m). These studies were successful at capturing the flow structures in complex terrain. As a consequence, atmospheric models can be applied to the coupled simulation of blowing snow events in alpine terrain."*

p 2197 l 1 What is the importance of the sentence "Previous versions..."? Just a reference? This sentence was initially included in the paper to mention previous applications of Crocus for the study of wind-induced snow transport. Following the general comment of Reviewer 2 concerning the too large number of references in the paper, we removed this sentence in the new version of the paper.

p 2199 l 5 Sentence is not understandable to the reviewer. Following the advice of Reviewer 1 the model's description has been shortened and this sentence removed from the paper.

p 2204 l 11 use -> uses Correction included.

p 2207 l 11 correct: of a smaller timestep We use the plural: *"smaller time steps"* following the suggestion of Reviewer 1.

p 2208 l 7 The sentence is quite meaningless This sentence has been removed from the paper.

p 2208 l 13 ff The following paragraph is out of context This paragraph has been reformulated and the mention to the "intensive measurements campaigns" removed. The new paragraph focuses on the data available for the two blowing snow events used for the evaluation of Meso-NH/Crocus presented in this paper:

In the following sections, we propose a first evaluation of Meso-NH/Crocus using data collected at Col du Lac Blanc during two blowing snow events in 2011. Their main characteristics are given in Table 2. In-situ measurements collected during both events include: (i) meteorological conditions (wind speed and direction, air temperature) at three automatic weather stations (AWS) located around the pass, (ii) vertical profile (up to 3.5 m) of wind speed on a meteorological mast at the pass and (iii) vertical profile of blowing snow fluxes using three Snow Particles Counters (SPC, Sato et al., 1993) at the pass. Additionally, the evolution of snow depth was followed for

the first event using data from a Terrestrial Laser Scanner (TLS, Prokop et al., 2008). Snow depths were measured over an area of 0.54 km^2 around the pass before (17 February 2011) and after (28 February 2011) the first event.

p 2210 Please split Results and Discussions in two separte sections. As mentioned earlier Results and Discussions have been separated in two sections.

p 2211 l 14 area -> areas Correction included.

General: there is an enormous amount of studies cited. often as a list of 2-3 authors. Do I have to read all? Is there some importance? Please restrict yourself to the most important, and explain why this reference is relevant. Citations have been restricted to the most important ones and lists of 2-3 publications have been removed when not relevant (see for example the Introduction in the reviewed version of the paper). Overall the total number of citations decreased by 10.

References All references have at the end a meaningless number (first paper: 2214) correct Numbers were added by the editorial office. They will be removed prior to final publication.

Figures The fontsize of most figures (5, 7,9, is just at the edge of being legible, too small in general. We believe that this limitation in the font size of figures was mostly due to the resizing of figures in the Discussion version of the paper to fit the Discussions format. This will be adjusted in the final publication.

Fig. 5: What is the meaning of the color bars? The caption of Fig. 5 has been re-written to precise the meaning of color bars. We added the sentence : "*Color bars show elevation (m) in **(a)** and wind speed (m s^{-1}) in **(b)**.*"

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