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Discussions

Interactive comment on "Simulation of wind-induced snow transport in alpine terrain using a fully coupled snowpack/atmosphere model" *by* V. Vionnet et al.

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General:

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).

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. . . . . ."

2) Abstract: Add "over the calculation domain" when stating the reduction in deposition

and specify that the 5.3% are based on snow mass (not snow height or something else).

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).

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.

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.

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.

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).

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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.

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.

References: Bintanja, R.: Modelling snowdrift sublimation and its eff ect on the moisture budget of the atmospheric boundary layer, Tellus A, 53, 215–232, 2001. Déry, S. and Yau, M.: Simulation of blowing snow in the Canadian Arctic using a double-moment model, Bound.-Lay. Meteorol., 99, 297–316, 2001. Groot Zwaaftink, Mott, R., Lehning, M., 2013: Seasonal simulation of drifting snow sublimation in Alpine terrain. Water Resour. Res., 47, 10.1002/wrcr.20137. Schirmer, M. and M. Lehning, 2011: Persistence in Intra-annual Snow Depth Distribution Part II: Fractal Analysis of Snow Depth Development; Water Resour. Res., doi:10.1029/2010WR009429. Schirmer, M. , Wirz, V„ Clifton, A., Lehning, M., 2011: Persistence in Intra-annual Snow Depth Distribution Part I: Measurements and Topographic Control; Water Resour. Res., doi:10.1029/2010WR009426.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/7/C1466/2013/tcd-7-C1466-2013-supplement.pdf

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Interactive comment on The Cryosphere Discuss., 7, 2191, 2013.

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