Interactive comment on "Hydrological response unit-based blowing snow modelling over an alpine ridge" by M.K MacDonald, J.W. Pomeroy and A. Pietroniro

MacDonald et al. (matt.macdonald@usask.ca)
MS No.: hess-2009-288
Response to Referee #1(M. Lehning) Comments (responses in bold)

We thank Referee 1 for insightful comments published on 9 April 2010. The comments will be considered in the preparation of the final version of this manuscript.

Response to General Comments:

...The application of (HRU) for drifting snow and hydrology appears to be novel from the presentation in the paper and it also appears that existing sub-modules have been developed and combined for the application presented here. However, the same authors have published a similar application [MacDonald et al., 2009] last year, which is neither referenced nor discussed in the current manuscript. Also similar concepts have been developed in the context of avalanche warning in France [Durand et al., 2001] and Switzerland [Lehning and Fierz, 2008], which could be discussed in this context. The paper is generally well written and has a clear structure. However, the paper lacks a clear science focus and it is not clear what we have learned from this application in particular when compared to the earlier publications by the same and other authors, which are only partly discussed...

This reviewer is incorrect in stating that no reference was made to authors' 2009 publication. Referee 1 missed that MacDonald et al. (2009) was discussed on pg.1170, l.29 of the original manuscript.

The works by Durand et al. (2001) and Lehning and Fierz (2008) have been reviewed and an appropriate discussion will be included in the revised manuscript.

Referee 1's statements that "the paper lacks a clear science focus and it not clear what we have learned from this application" and that "...the sublimation model used here would overestimate blowing snow sublimation" are thought provoking and have led the authors' to restructure the focus of this manuscript. There has been debate in the scientific community as to the importance of alpine snow sublimation (e.g. Strasser et al., 2008; Reba et al., 2009). There has been also been a debate with respect to importance of, and the estimation of, the sublimation of blowing snow (Pomeroy et al., 1993; Liston and Sturm, 1998; Bintanja, 1998; Bintanja, 2001; Bintanja and Reijmer, 2001; Déry et al., 1998; Déry and Yau, 1999; Déry and Yau, 2001; Pomeroy and Li, 2000). The snow transport models presented by Doorschot et al. (2001) and Lehning et al. (2008) neglect the sublimation of blowing snow particles.

In light of the above, the authors' have decided to refocus the manuscript towards the important of blowing snow sublimation to alpine snow mass balances in the Canadian Rocky Mountains. The results presented in the revised manuscript (and those in the original manuscript, for that matter) show that blowing snow sublimation is very important to alpine snow mass balances in this region. Our work shows that the snow

mass balance is satisfactorily closed over Fisera Ridge, indicating that PBSM and CRHM are correctly estimating sublimation at this location.

References:

Bintanja, R.: The contribution of snowdrift sublimation to the surface mass balance of Antarctica, Ann. Glaciol., 27, 251-259, 1998.

Bintanja, R.: Modelling snowdrift sublimation and its effects on the moisture budget of the atmospheric boundary layer, Tellus Serie A, Dynamic Meteorology and Oceanography 53: 215-232, 2001.

Bintanja, R., Reijmer, C.H.: A simple parameterization for snowdrift sublimation over Antarctic snow surfaces, J. Geophys. Res.-Atm., 106, 31739-31748, 2001.

Déry, S.J., Taylor, P.A., Xiao, J.: The thermodynamic effects of sublimating, blowing snow in the atmospheric boundary layer, Bound.-Lay. Meteorol., 89, 251-283, 1998.

Déry, S.J., Yau, M.K.: A bulk blowing snow model. Bound.-Lay. Meteorol., 93, 237-251, 1999.

Déry, S.J., Yau, M.K.: Simulation of blowing snow in the Canadian Arctic using a double-moment model, Bound.-Lay. Meteorol., 99, 297-316, 2001.

Doorschot, J., Raderschall, N., Lehning, M.: Measurements and one-dimensional model calculations of snow transport over a mountain ridge, Ann. Glaciol., 32, 153-158, 2001.

Durand, Y.G., Guyomarc'h, G., Merindol, L.: Numerical experiments of wind transport over a mountainous instrumented site: I. Regional Scale, Ann. Glaciol., 32, 187-194, 2001.

Lehning, M., Fierz, C.: Assessment of snow transport in avalanche terrain, Cold Reg. Sci. Technol., 51, 240-252, 2008.

Lehning, M., Lowe, H., Ryser, M., Raderschall, N.: Inhomogeneous precipitation distribution and snow transport in steep terrain, Water Resour. Res., 44, W07404, 2008.

Liston, G.E., Sturm, M.: A snow transport model for complex terrain, J. Glaciol., 44, 498-516, 1998.

MacDonald, M.K., Pomeroy, J.W., Pietroniro, A.: Parameterizing redistribution and sublimation of blowing snow for hydrological models: tests in a mountainous subarctic catchment, Hydrol. Process., 23, 2570-2583, 2009.

Pomeroy, J.W., Gray, D.M., Landine, P.G.: The Prairie Blowing Snow Model: characteristics, validation and operation, J. Hydrol., 144, 165-192, 1993.

Pomeroy, J.W., Li, L.: Prairie and arctic areal snow cover mass balance using a blowing snow model, J. Geophys. Res.-Atm., 105, 26619-26634, 2000.

Reba, M. L., T. E. Link, D. Marks, and J. Pomeroy (2009), An assessment of corrections for eddy covariance measured turbulent fluxes over snow in mountain environments, *Water Resources Research*, 45, W00D38, 2009. doi:10.1029/2008WR007045.

Strasser, U., Bernhardt, M., Weber, M., Liston, G.E., Mauser, W.: Is snow sublimation important in the alpine water balance?, The Cryosphere, 2, 53-66, 2008.

A detailed point by point assessment is given below (roughly in perceived order of significance).

1) Saltation flux submodel: The saltation flux submodel as given in Eqs. (3) and (4) in the ms is questionable. First, Eq. (3) is conceptually and formally incorrect. Conceptually, the parameter p appears to be redundant because the fluxes F and EB must be zero if there is no snow transported. Thus, it appears to be a fit parameter with no information given as to how it is determined...the units of F are wrong as given in the text. Since the divergence will introduce an additional "length" dimension in the denominator, F needs to have the same units as EB, which is the unit of a horizontal and vertically integrated flux.
Second and much more importantly, the linear dependence of the saltation flux as expressed by Eq. (4) appears to clearly contradict established and newer theoretical and experimental results. When we look at the original Pomeroy and Gray paper [Pomeroy and Gray, 1990], the assumption of stress partitioning as expressed in their Eq. (2) would still allow a more conventional dependence of the flux on the cube of the wind speed, consistent with other sediment fluxes [Greeley and Iversen, 1987] ... A lot has happened since 1990 in the field and e.g. our own work [J J J Doorschot and Lehning, 2002] has shown clearly, that the linear dependence leads to massive deviations from measured saltation fluxes [Nishimura and Hunt, 2000] above friction velocities of approximately 0.4 m/s and that on the other hand the cubed dependence of flux on velocity is reproduced by a saltation model that calculates fluxes based on first principle dynamics of the saltating particles [Clifton and Lehning, 2008; J J J Doorschot and Lehning, 2002].

Regarding the saltation algorithm, the term p in Equation (3) is the probability of blowing snow occurrence (Li and Pomeroy, 1997). Reviewer 1 missed that this term was indeed discussed in the original manuscript on pg.1179, l.15-26.

Reviewer 1 identified that the notation of Equation (3) and the units given for F in the accompanying text do not align. This typo will be changed in the revised manuscript but has no impact on the results or conclusions.

Reviewer 1 stated that the saltation and suspension algorithms used are incorrect and should not be used, partly because they have not been developed in the last 10 years. We reject this comment based on the following discussion. The saltation algorithm developed by Pomeroy and Gray (1990) was based partly on extensive field observations in a level location with fully developed saltating flow. The suspension algorithm developed by Pomeroy and Male (1992) was based partly on extensive field observations in a level location with fully developed suspended flow over the range of measurement heights (< 3 m). The algorithms were derived from robust concepts initially expressed by Bagnold (1941) and Shiotani and Arai (1953) and used for transport of sand, snow and dust. Both model results are consistent with the original observations and further detailed tests in the Scottish Highlands (Pomeroy, 1991). Tabler (1991) independently and extensively tested the saltation algorithm in the USA and found it ideal for

estimating snow transport along with suspended snow models. Further field tests of the saltation and suspension algorithms as part of PBSM have been satisfactory in prairie, arctic and alpine environments (Pomeroy and Essery (1999), Pomeroy and Li (2000), MacDonald et al. (2009)). The reviewer insists that saltating snow transport rates increase with the cube of wind speed and that suspended snow transport rates increase linearly with wind speed. The first assertion goes back to Budd et al. (1966) and led to absurdly high estimates of snow transport near the surface that have long been rejected (Pomeroy and Gray, 1995). The second assertion requires that the mass concentration of suspended snow and the upper height of suspension are invariable with wind speed. These thoughts are completely inconsistent with decades of field observations and physically based model calculations in Canada, UK, USA, Antarctica, Russia and elsewhere and hence with our current understanding of the blowing snow and other two phase flow phenomenon. To suggest a complete revision of our understanding and modelling of blowing snow transport would require an extensive, novel and compelling field observation and modelling programme that is outside of the scope of this paper – which is to better estimate blowing snow sublimation in alpine environments.

Reference:

Budd, WF, Dingle, WRJ, and Radok, U.: The Byrd snow drift project: outline and basic results, Studies in Antarctic Meteorology, AGU Antarctic Research Series, 9, 71-134, 1966.

Li, L., Pomeroy, J.W.: Probability of occurrence of blowing snow, J. Geophys. Res., 102, 21955-21964, 1997.

MacDonald, M.K., Pomeroy, J.W., Pietroniro, A.: Parameterizing redistribution and sublimation of blowing snow for hydrological models: tests in a mountainous subarctic catchment, Hydrol. Process., 23, 2570-2583, 2009.

Pomeroy, J.W., Gray, D.M.: Saltation of snow, Water Resour. Res., 26, 1583-1594, 1990.

Pomeroy, J.W., Male, D.H.: Steady-state suspension of snow, J. Hydrol., 136, 275-301, 1992.

Pomeroy, J.W., Gray, D.M.:. Snow Accumulation, Relocation and Management. National Hydrology Research Institute Science Report No. 7.,   Environment Canada: Saskatoon, 144 pp., 1995.

Pomeroy, J.W., Li, L: Prairie and arctic areal snow cover mass balance using a blowing snow model, J. Geophys. Res.-Atm., 105, 26619-26634, 2000.

Shiotani, M., Arai, H.: A short note on the snowstorm. In: Proc. 2nd Japanese Natl. Congress of Applied Mechanics, 217-218, 1953.

Tabler, R. D., C. S. Benson, B. W. Santana, and P. Ganguly.: Estimating snow transport from wind speed records: estimates versus measurements at Prudhoe Bay, Alaska. *Western Snow Conference Proceedings* 58: 61-72, 1990.

2) You should state clearly that Eq. 21 only describes the reproduction of total mass in the whole domain and Eq. 22 is a measure of how well the mass is distributed on the diverse HRUs.

Thank you. The meaning of *MB* and *RMSE* measurements will be expanded upon in the revised manuscript.
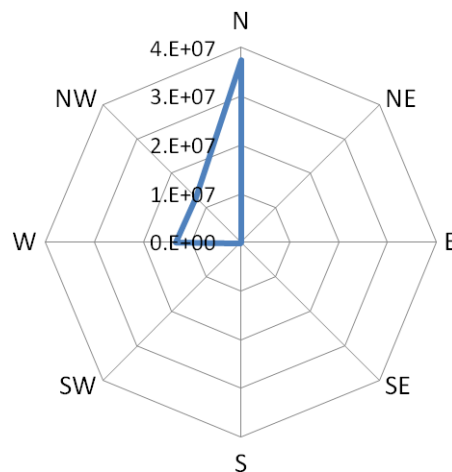
3) One main problem is clearly that the HRUs have had to be selected a posteriori given the observed snow distribution. Therefore, the question whether the simplification using HRUs is actually a practical possibility cannot be answered.

The authors stated in the original manuscript that future work will involve generalizing HRU delineations based on terrain characteristics. This will be expanded upon in this revised manuscript

The authors believe that the focus of the revised manuscript on alpine blowing snow sublimation warrants publication since this has not been studied in depth in the Canadian Rocky Mountains or elsewhere.

4) The description of the set-up of the HRUs makes it difficult to understand how the system works for varying wind directions. From the description on p. 1184, l.6 ff one would assume that the order of sequence is fixed for the HRUs. This cannot work for changing wind directions but this problem is not discussed.

The HRU set-up assumes that all snow transport occurs in one direction throughout the simulation (i.e. the sequence is fixed). It will be discussed in the revised manuscript why this is a suitable assumption at this location. Snow transport rates scales approximately with the fourth power of wind speed (Pomeroy and Gray, 1992; Essery et al., 1999). The following figure shows a fourth power of wind speed rose (sum of fourth power of wind speeds binned by direction) for the observed wind speed and direction at the Fisera Ridge ridge-top station over the study periods. It is clear that northerly, and to a lesser extent northwesterly and westerly, blowing snow events dominate snow redistribution at this location.

References:

Essery, R., Li L., Pomeroy, J.: A distributed model of blowing snow over complex terrain, Hydrol. Process., 13, 2423-2438, 1999.

Pomeroy, J.W., Gray, D.M., Landine, P.G.: Modelling the transport and sublimation of blowing snow on the prairies, Proceedings of the Eastern Snow Conference, 175-188, 1991.

5) Roughness lengths (Eq. 6): In alpine terrain, Eq. (6) may not apply. As shown nicely by e.g. [J J J Doorschot et al., 2004], the roughness length is determined by terrain and other surface features and does not even respond significantly to changes in the snow surface let alone saltation.

Doorschot et al. (2004) studied the aerodynamic roughness length in a non-steady state flow environment where advection of turbulent energy likely overwhelmed surface effects. We have found similar mountain environments (Helgason and Pomeroy, 2005) and have found that the advected turbulence does affect the shear stress but primarily via horizontal turbulence and so much less for vertical scalar transport. That is why it is possible to model snow-atmosphere exchange in such complex terrain using roughness lengths that correspond to snow measured in open flat environments rather than surrounding terrain. Doorschot et al's findings are inconsistent with observations over two-phase flow in the atmosphere for over 70 years in North Africa, America, Canada, Russia (Bagnold, 1941; Tabler, 1980) that are summarized by Chamberlain (1983) and recently tested by Sherman and Farrell (2008). These observations were made from the deviation of the wind speed profile over the saltating surface. Nevertheless, we thank M. Lehning for point this out as it raises an uncertainty in this turbulent exchange and it will be referenced in the revised paper.

References:

Bagnold, R.A.: The physics of blown sands and desert dunes, Methuen, London, 1941.

Chamberlain, A.C.: Roughness length of sea, sand and snow. Bound.-Lay. Meteorol., 25, 405-409, 1983.

Doorschot, J.J.J., Lehning, M., Vrouwe, A.: Field measurements of snow-drift threshold and mass fluxes and related model simulations, Bound.-Lay. Meteorol., 113, 347-368, 2004.

Helgason, W.D., Pomeroy, J.W.: Uncertainties in estimating turbulent fluxes to melting snow in a mountain clearing, Proceedings of the Eastern Snow Conference, 62, 129-138, 2005.

Sherman, D.J., Farrell, E.J.: Aerodynamic roughness lengths over movable beds: comparison of wind tunnel and field data, J. Geophys. Res., 113, F02S08, 2008. doi:10.1029/2007JF000784

Tabler, R.D.: Self-similarity of wind profiles in blowing snow allows outdoor modelling, J. Glaciol., 26, 405-419, 1980.

6) Radiation model: The role of shading is not represented and not discussed in your radiation model. We recently found [Helbig et al., 2009] that even multiple reflections from terrain may be important and shading is certainly a factor that is represented in most distributed and semi-distributed hydrological models.

Reflections from adjacent terrain are captured by the radiometer measurements at the ridge-top station. The radiation model adjusts flat-plane radiation measurements (i.e. the ridge-top station measurements) for aspect and slope, and these are applied to the other HRUs. Thus all HRUs do receive some input of reflected radiation. However, the model does assume that all HRUs will receive the same contribution of reflected radiation relative to total incoming radiation as measured at the ridge-top station. This may not be the most accurate approach but is certainly adequate for this site and, in particular, for this application because the authors' are focusing on modelling snow accumulation and not so much snowmelt. Certainly this method produced excellent radiation for snowmelt modelling in the same environment (DeBeer and Pomeroy, 2009)

To the authors' knowledge, Reviewer 1 is mistaken and topographic shading is <u>not</u> represented in most distributed and semi-distributed hydrological models (see Ivanov et al., 2004; Liston and Elder, 2006; Pomeroy et al., 2007).

All of the above assumptions, limitations and explanation will be discussed in the revised manuscript.

References:

Ivanov, V.Y., Vivoni, E.R., Bras, R.L., Entekhabi, D.: Preserving high-resolution surface and rainfall data in operational-scale basin hydrology: a fully-distributed physically-based approach, J. Hydrol., 298, 80-111, 2004.

Liston, G.E., Elder, K., A Meteorological Distribution System for High-Resolution Terrestrial Modeling (MicroMet), J, Hydrometeorol., 7, 217-234, 2006.

Pomeroy, J.W. et al., 2007. The cold regions hydrological model: a platform for basing process representation and model structure on physical evidence. Hydrological Processes, 21(19), 2650-2667.

DeBeer, C.M., Pomeroy, J.W.: Modelling snowmelt and snowcover depletion in a small alpine cirque, Canadian Rocky Mountains, Hydrol. Process., 2009. DOI:10.1002/hyp.7346.

7) p. 1187 l.20ff: The sentence suggests an erroneous coupling of the surface energy balance in your model or a wrong interpretation. Over a melting snow cover, higher winds will typically increase turbulent transfer of sensible heat towards the snow but COOL the snow via latent heat (sublimation). Thus, increased latent heat because of increased wind will REDUCE the energy available for melt. This is why snow often survives longer in warm dry environments compared to warm wet environments.

The authors agree that statement referred to here was not clear in the original manuscript. A different snowmelt model was used for the revised manuscript. The statement will be revised to be more clear.

Response to Minor Comments:

p. 1169 l.10: The reference to one of the authors at this place makes it look as if this had first been described by this author. Please reference an early work (e.g. Tabler, Meister, Budd).

References to Dyunin and Kotlyakov (1980), Föhn (1980), Schmidt et al. (1984) and Meister (1989) will be included at this place in the revised manuscript.

References:

Dyunin, A.K., Kotlyakov, V.M.: Redistribution of snow in the mountains under the effect of heavy snow-storms, Cold Reg. Sci. Technol., 3, 287-294, 1980.

Föhn, P.M.B.: Snow transport over mountain crests, J. Glaciol., 26, 469-480, 1980.

Meister, R.: Influence of strong winds on snow distribution and avalanche activity, Ann. Glaciol., 13, 195-201, 1989.

Schmidt, R.A.: Comparison of snow drifting measurements at an Alpine ridge crest, Cold Reg. Sci. Technol., 9, 131-141, 1984.

p.1178 l.21: Sentence does not make any sense. If you assume stationary conditions (this is o.k.) then shear stress must be constant in time not only at some height but at all heights.

Agreed. The sentence should read "Up to z_b, shear stress is assumed constant ($d\tau/dt=0$) and suspension occurs under steady-state conditions ($d\eta/dt=0$).

p.1179: Please give also reference to the original Thorpe and Mason formulation of sublimation.

Reference to the original Thorpe and Mason formulation will be included in the revised manuscript. However, Schmidt provided the most comprehensive examination and application of this concept.

p.1180 l.5: Very strange equation, please give the units of M. What is a "thermal quality of snow"? Please define your variables with units.

This is a well known and accepted equation in snow hydrology for over 30 years. It is a standard equation for estimating the depth of snowmelt from a coupled mass-energy balance that accounts for liquid water existing in the snowpack before the major melt period (e.g. Gray and Male, 1980; Gray and Landine, 1988; Pomeroy et al., 2007; DeBeer and Pomeroy, 2009).

The units of *M* shall be included in the revised manuscript.

In the revised manuscript, *B* is defined as the fraction of ice in a unit mass of wet snow.

References:

Gray, D.M. and Male, D.H. 1980. Handbook of Snow. Pergamon Press, Toronto.

DeBeer, C.M., Pomeroy, J.W.: Modelling snow melt and snowcover depletion in a small alpine cirque, Canadian Rocky Mountains, Hydrol. Process., 23, 2584-2599, 2009.

Gray, D.M., Landine, P.G.: An energy-budget snowmelt model for the Canadian Prairies, Can. J. Earth. Sci., 25, 1292-1303, 1988.

Pomeroy, J.W., Gray, D.M., Brown, T., Hedstrom, N.R., Quinton, W., Granger, R.J., Carey, S.K.: The cold regions hydrological model: a platform for basing process representation and model structure on physical evidence, Hydrol. Process., 21, 2650-2667, 2007.

p. 1180 l.23: Please reference or motivate the seemingly empirical equation.

The revised manuscript uses SNOBAL as the snowmelt model, therefore this equation was not used.

p. 1181: Does your canopy model account for increased longwave emissions from the warmer plants?

Yes it does. This model component will be mentioned in the revised manuscript.

p. 1188, l.25ff: I am not sure why [Bernhardt et al., 2009] really needed 220 wind fields.
The same principle is used in the earlier publication by [Raderschall et al., 2008], which only uses a few wind fields to reconstruct the snow distribution at the Gaudergrat ridge.
Again, this application is not discussed, however.

Reference to Raderschall et al. (2008) will be included in the revised manuscript.

The discussion of how many wind fields Bernhardt et al. needed is outside of the scope of this manuscript.

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MS No.: hess-2009-288

Response to Referee #2 Comments (responses in bold)

We thank Anonymous Referee #2 for insightful comments published on 10 April 2010. The comments will be considered in the preparation of the final version of this manuscript.

Response to General Comments:

The paper is well written and has a logical structure. Nevertheless, it seems to be a little unbalanced. The model description is very detailed with lots of formulas. This part should be shortened and some well-known formulas should be excluded and the original literature should be cited instead. The chapters 4, 5, 6 and 7 on the other hand should be extended a little bit and it should be proofed if some of these sections can be combined.

Some of the equations presented in Section 3.2 (Prairie Blowing Snow Model) will not be included in the revised manuscript.

Chapter 2.2 describes some features that were measured. It would be good for the general understanding if the authors would provide some information about the scope of the measurements. "A shrub count was performed for diving the roughness of the surface: : :." for example. Furthermore, is it true that the snow survey is followed the modeled transect (1173 line 6)? Or are the HRU's (and therewith the modeled transect) defined over the findings of the field campaign?

The revised manuscript will include more details about the shrub measurements (i.e. ruler measurements, etc.)

It is true that the HRUs were defined based on the observed snow depth along the transect. This will be clarified in the revised manuscript.

Chapter 4: I think this chapter is essential for understanding the model concept. Hence, the reader should precisely understand in which way the different HRU's were defined. I think the authors should extent this part a little bit. What does e.g. "is located from 127 to 243m" mean? What exactly is the criterion that defines if a new HRU is needed or not? Is it just the SWE (of 07/08 or 08/09 or of both years?) or also vegetation, topography and so on?

The spatial extent of HRUs were mainly based on the observed snow depths shown on Figure 4, with the exception of the Forest HRU because its vegetation characteristics are much different from the rest of the transect. Though the HRUs were selected based on the observed snow depths, these HRU selections also reflect topographic effects on solar radiation (i.e. northfacing vs southfacing vs canopy). The observed snow depth pattern

along the transect was similar in 2008/2009 to that in 2007/2008. The statement "...is located from 127 to 243 m..." refers to the spatial extent of the HRU as shown on Figure 4.

All of the above will be clarified in the revised manuscript.

The chapters 5 and 6 are pretty short. Can you combine 5 and 6 and probably the first part of 7, were you just describing the results? I think you could probably shift the content of chapter 7 from 1187 / line 1 to 1187 / line 22, to 6.

Sections 5 and 6 will be combined in the revised manuscript.

The authors prefer to leave the first part of the existing section 7 (from 1187 / line 1 to 1187 / line 22) within the *Discussion* section since this text describes the model results.

Chapter 7: It should be very clear if you discuss model results or observations or if you discuss model results with respect to observations... The HRU concept was mentioned as an important factor for an enhanced model performance and it is mentioned that the results are satisfying. Nevertheless, there are some simplifications. The authors use a fixed sequence for the transport of snow from one HRU to another for example. They also give the answer that this concept works out well. However, a discussion how and if a negligence of different wind directions and therefore transport directions is influencing the results would be beneficial.

It will be made clear in the revised manuscript if the authors are discussing model results or observations.

The assumption of using a fixed HRU sequence for snow transport at Fisera Ridge is indeed suitable. Snow transport rates scales approximately with the fourth power of wind speed (Pomeroy and Gray, 1992; Essery et al., 1999). The following figure shows a fourth power of wind speed rose (sum of fourth power of wind speeds binned by direction) for the observed wind speed and direction at the Fisera Ridge ridge-top station over the study periods. It is clear that northerly, and to a lesser extent northwesterly and westerly, blowing snow events dominate snow redistribution at this location. A discussion of this, including this figure, will be included in the revised manuscript.
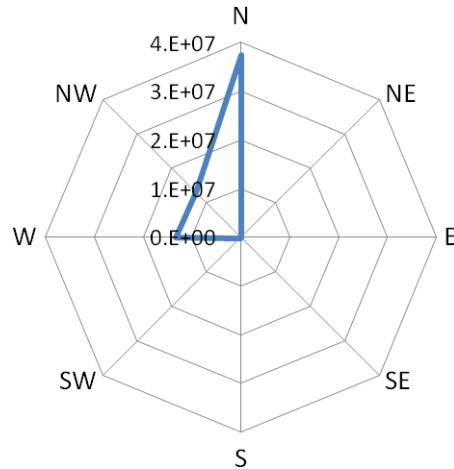
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References:

Essery, R., Li L., Pomeroy, J.: A distributed model of blowing snow over complex terrain, Hydrol. Process., 13, 2423-2438, 1999.

Pomeroy, J.W., Male, D.H.: Steady-state suspension of snow, J. Hydrol., 136, 275-301, 1992.

Response to Specific Comments:
Side 1169, line 5: The snow cover can also isolate the ground and does therefore not necessarily cool the ground.

The authors stated that "Snowcover increases the surface albedo and cools the surface…". In the original manuscript , we were referring to the surface that interacts with the atmosphere (i.e. the snow surface) which is cooler than a bare ground surface. This statement will be clarified in the revised manuscript.

Side 1171, line 5-11: I would suggest splitting up this one sentence into 2 or more sentences.

This section will be clarified in the revised manuscript.

Side 1171 and side 1172: Figure 1 should be cited here.

Figure 1 will be cited at this location in the revised manuscript.

Side 1172, line 26: I would exclude 2006/2007.

Mention of 2006/2007 will be excluded in the revised manuscript.

Side 1173, line 1-2: There are : : :. Please specify

This sentence will be clarified in the revised manuscript.

The caption of the tables is very short. One should understand the table without reading the text. So, can you please extent the caption?

The table captions will be extended in the revised manuscript.