Hydrology and Earth System Sciences Discussions

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Comment

Interactive comment on "Hydrological response unit-based blowing snow modelling over an alpine ridge" *by* M. K. MacDonald et al.

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General: The correct representation of lateral snow transport and associated processes such as increased sublimation in hydrological models has been a problem for decades and is still not satisfactorily solved. Therefore, the attempt to establish a simplified scheme to snow transport modelling for larger scale hydrological models based on the hydrological response unit (HRU) approach is very welcome and the paper addresses relevant scientific questions within the scope of HESS. 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

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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. The abstract states that "Snow redistribution by wind was shown to significantly impact snow accumulation . . . . . . snow sublimation losses were shown to be significant". This has been know for decades and the paper does not properly position itself between the options "simple parameterization" or "physics based description": Current under-standing is that preferential deposition is a major factor in snow accumulation [Dadic et al., 2010; Lehning et al., 2008], which is not discussed at all in this paper. It is also known that simpler models and parameterizations can make a correct description of local mass fluxes [Lehning and Fierz, 2008] and even predict lee slope loading ade-quately [J Doorschot et al., 2001]. The approach presented here is too complicated to qualify as a suitable "parameterization" and does also not recommend itself as a physics based description, since the model modules are partly physically questionable (saltation – see detailed discussion below) or incomplete (moisture feed-back on subli-mation, missing preferential deposition). I feel that adequate reference to the scientific context is not given, in particular with respect to newer results and to other work, which would indicate that e.g. the sublimation model used here would overestimate blowing snow sublimation [Bintanja, 2001; Dery and Yau, 2002; Mann et al., 2000; Xiao et al., 2000]. In defence of the authors it can be said here that our recent wind tunnel results [Wever et al., 2009] show a significant effect of saltating snow on total sublimation in a re-circulating wind tunnel setting, in which saturation effects may be similarly impor-tant as in the atmospheric boundary layer. Altogether, these are all issues that could be fixed in a major effort and if the authors could for example demonstrate that the snow transport module improves runoff simulations (e.g. the timing of the runoff), a worthwhile publication may result. 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. Formally, the use of the Nabla symbol (but it does not even look correct – it is too narrow) in the equation suggests that the del operator is used to express the divergence of the flux F. If this is the case, then the notation is incorrect because then there should be a dot between the del operator and the flux vector. Apart from the fact that formally the del operator is conventionally only defined in a full Cartesian context (3 dimensions), 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] or e.g. the extraction of wind power by turbines, at least when the particle velocity would depend linearly on wind speed. That this is a fair possibility is explicitly discussed in the Pomeroy and Gray paper and is even present in parts of their data (Figure 2). Nonetheless the authors then decide "for simplicity" to follow another branch of the highly scattered data set and propose an inverse relationship between particle velocity and wind velocity, which is already intuitively difficult. 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]. I therefore think that the simple Pomeroy and Gray formula should be used with much caution especially since similar simple formulations are available with a more correct dependence on wind speed and since the same authors write in their (non-cited) paper of last year that "With the knowledge that snow transport scales approximately with the fourth power of wind speed. . .." [MacDonald et al., 2009], which is only possible if the saltation system has this dependence since the other transport modes (suspension) approximately scale linearly with wind speed [Dadic et al., 2010; Lehning et al., 2008].

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.

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. I would expect that at least an independent application of the method to an independent catchment is required to warrant publication.

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.

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.

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.

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.

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).

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.

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

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.

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

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

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.

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