

Interactive comment on “Improving the snow physics of WEB-DHM and its point evaluation at two SnowMIP alpine sites” by M. Shrestha et al.

R. L. H. Essery (Referee)

Richard.Essery@ed.ac.uk

Received and published: 4 August 2010

This is a well-written paper giving a fairly thorough description of a snow model and evaluating its performance in comparison with point observations, but there have been many such papers; this one combines components of existing snow models and takes datasets that have already been heavily used in evaluating snow models. To convince the reader that another paper on this subject is worthwhile, there needs to be a unique contribution, perhaps by discussing the model’s performance in relation to the requirements of its intended applications. Another major concern is that the albedo values chosen for fresh and melting snow in the unmodified model appear very low, a suspicion which is confirmed by the comparison with observations in Figure 9. I understand

that these values were taken from SiB, but if the authors wanted to make the unmodified model look bad, then this would be a good way of going about it. Section 4.5 then reveals that an albedo parameter in the improved model was calibrated for the test sites, which invalidates any suggestion that the improved SWE simulations are due to changes in the model physics; we really need to see results from the unmodified model with more realistic parameters.

Minor comments

2.2.1 The information on layer subdivision is largely repeated from 2.2

2.2.2 Give some reference on how the surface fluxes are calculated. This will have a large influence on the surface temperature. How is the grain size used in the radiation extinction coefficients specified? Equations (11) and (12) for the canopy and surface snow layer temperatures contain another unknown: the snow layer 2 temperature. Show how the full system of equations is solved.

2.2.3 It is not clear here how IF_j and R_j are calculated.

2.2.5 Snow albedos are given for direct and diffuse illumination in visible and near infrared bands. How are they used, since these radiation components are not available in the forcing data?

4.2 Are the UDG or snow pit measurements of snow depth used in calculating the error statistics? What is the “desired accuracy”?

4.4 Figure 8 does not show energy conservation, which should be exact in both models.

4.5 Against what criteria was the fresh snow albedo calibrated?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 3481, 2010.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)