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Discussions

Interactive comment on "Modeling the snow surface temperature with a one-layer energy balance snowmelt model" *by* J. You et al.

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General:

The paper presents a modified version of an existing and well-known snow model, the Utah Energy Balance Model (UEB). The modification of the model mainly concerns the representation of a freezing front through the one-layer bulk snow cover as well as validation studies using meteorological measurements as available from standard observations. The novelty of this paper in relationship to earlier publications are these two points but not a general evaluation of the modified force-restore method or the other surface schemes, which has already been done in earlier publications. While this is clearly stated in the results section on p. 15093, the wording in the conclusion

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section and in particular in the abstract can suggest to a non-careful reader that these more substantial model features are first evaluated in this current paper.

Overall, I am unsure whether the progress reported in the current paper is substantial enough to warrant a separate publication. The impact of the freezing-front parameterization is very small and the other main results, namely that UEB does a good job in calculating surface temperature in general and internal snow energy content (with the modified force-restore method), have been known from previous publications. Since the authors very much emphasize the advantage of having a "simple“ one-layer snow model, it would be nice to at least see a more complete validation studies, which involves much more diverse snow climates and multiple years. The main focus of such a validation study could be to show that the increase in calibration parameters through the presented model development does not only (slightly) improve the results for the limited validation data sets presented but leads to a significant and transferable model improvement in a large variety of conditions, places and snow years. This has not been convincingly shown in the current study. Additional data is now easily available on request from the SNOWMIP consortia or e.g. directly for download from Col de Porte Morin et al. (2012) or from Weissfluhjoch Davos (www.swiss-experiment.ch).

The paper also attempts to advocate for simple snow models but without presenting any quantitative comparisons or discussing more detailed snow models with some specific features. E.g. Wever et al. (2014) have recently shown that even water transport in combination with phase changes can numerically be handelled with reasonable computational effort in a multi-layer snow model. These developments would suggest that the time of simple snow models is over at least for local to regional snow modelling and hydrology but maybe even for global climate models. If the authors still think simple models are needed, they should present more convincing arguments for that. I agree with the general argument that simple is beautiful but not if simplicity is paid for by additional calibration parameters.

Detailed comments are added directly to the pdf of the paper.

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10, C7980–C7982, 2014

References: Wever, N., Fierz, C., Mitterer, C., Hirashima, H., and Lehning, M.: Solving Richards Equation for snow improves snowpack meltwater runoff estimations in detailed multi-layer snowpack model, The Cryosphere, 8, 257-274, doi:10.5194/tc-8-257-2014, 2014.

Morin, S., Lejeune, Y., Lesaffre, B., Panel, J.-M., Poncet, D., David,P., and Sudul, M.: An 18-yr long (1993–2011) snow and meteorological dataset from a mid-altitude mountain site (Col de Porte, France, 1325m alt.) for driving and evaluating snowpack models, Earth Syst. Sci. Data, 4, 13–21, doi:10.5194/essd-4-13-2012, 2012.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/10/C7980/2014/hessd-10-C7980-2014-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 15071, 2013.