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The Cryosphere Discussions

Interactive comment on "Is snow sublimation important in the alpine water balance?" *by* U. Strasser et al.

Anonymous Referee #1

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General

This paper describes, for a single year (2003/04) and a 200 km2 large mountainous area in southern Germany, calculated snow sublimation rates from three processes: surface sublimation, canopy sublimation and sublimation of wind induced suspended snow particles. The outcome of the study is that, depending on the location and especially how exposed the site is, the various contributions of sublimation to total snowfall add up to between 10-90%. This is an interesting and generally well-written paper that finally deserves publication in TC. Before that, revisions are necessary, especially when it comes to readability (length, number of figures), which I have detailed below. Moreover, the technical quality of some of the figures in insufficient and some points need clarification. Moreover, a dedicated validation effort of the calculated sublimation is lacking.

Major comments

The title does not well represent the contents of the paper. The paper discusses a single year of sublimation in a particular area of the Alps, and this should be better reflected in the title.

The paper is unbalanced which makes it hard to read all the way to the end. The introduction takes 5 pages and reads more like a review article. Methods then cover 12+ pages, while results are covered in only 5 pages. My suggestion is to keep the results section about the same length, but slightly condense the Introduction (by 20%) and severely condense the Methods (by 50%) sections so a more balanced paper remains. For the introduction this can be achieved by reducing the amount of references (presently the list is $>$ 60 titles long!) and in the introduction limiting the discussion to the most recent work, only referring to older work when no newer papers on a certain topic exist.

The Methods section can be shortened by removing Eq. (2), which is straightforward, and Eqs. (3)-(12), which are mainly engineered (parameterized) expressions and are not required for a good understanding: several references to earlier work will do will do.

p. 306, l. 23: Something goes wrong in the argumentation against the profile method to calculate long-term sublimation. Instrument height can nowadays be perfectly well monitored by sonic height rangers. Moreover, the aerodynamic bulk method is just the vertically integrated version (from the surface to the measurement height) of the profile method. The advantage of the bulk method is that just a single measurement level is required, and that sensitivity to measured gradients decrease because the gradients are largest near the surface. But the disadvantage of the bulk method is that surface parameters need be known, such as surface temperature, roughness for momentum and for scalars (temperature and specific humidity). Moreover, atmospheric measurement height still needs to be known. The formula used in this paper, Eq. (1) on page

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314, is merely a further simplification of the bulk method, in which certain values for the surface roughnesses have been assumed. The fact that measurement height does not appear explicitly in this expression means that it has been developed for a certain fixed value (presumably 2 m) and this height must be mentioned in the text. Moreover, it must be mentioned that stratification effects are neglected and not, as stated now, that the expression is applicable over a wide range of stratifications, which is false.

This brings me to the major weakness of the paper: there is no validation of the calculated latent heat fluxes. The authors state that the individual model components have been validated, but for a paper that claims to assess the role of sublimation in the water balance, there should be at least a dedicated validation effort. If only the modelled and observed specific humidity gradients between the surface and the 2 m air (as a measure for q*) and wind speed (as a measure for u*) at a single AWS location would be given, it is better than just neglecting it. One way of doing this could be to exclude one AWS from the meteorological interpolation scheme, and compare the interpolated and observed values. Judging from Fig. 3, the Jenner II AWS could be a good test site for this.

Detailed comments

p. 305, l. 2: vapour

p. 305, l. 3: remove 'winter'

p. 305, l. 1: replace 'during ...obstacles' by 'by wind shear'

p. 305, l. 22: replace 'high' by 'large&'

p. 306, l. 22: Dyer (1974)

p. 307, l. 9: replace 'on' by 'from'

p. 308, l. 16: replace 'moving' by 'removal'

p. 320 and throughout the paper: Katabatic is written with a 'k'

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p. 324, l. 28: replace 'slided' by 'slid'

captions Figures 9 and 10: Add: 'from the surface' after 'losses'

Figure 10, 12 and 13 provide insufficient detail in the colour scheme. Consider removing.

Figure 12, 13 and 15: Why is the calculation domain different (smaller) than in the previous maps?

Interactive comment on The Cryosphere Discuss., 1, 303, 2007.