

Simulation of snow distribution and melt under cloudy conditions in an alpine watershed

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*General comments*

The study is definitely interesting and the results presented definitely worth publishing, given the scarce experimental evidence collected and published on the water and energy balance of high elevation catchments.

There are a number of major and minor aspects that should be addressed, as detailed below.

Major comments relate to unclear elements of the proposed model and of data analysis.

Usage of the English language is poor at times, the manuscript should be by a native speaker or a professional editor.

I recommend publication after a major revision.

*Specific comments*

**Pg.3190 line 5:** Abstract; the word “discriminate” in the sentence “A linear interpolation method was used to discriminate daily snow cover area under cloudy conditions..” is probably not correct, since the surface cannot be observed when cloudy;

**Pg.3190 line 9:** Abstract; is snow water equivalent also simulated? Why then is not mentioned in the list of variables which have been simulated?

**Pg.3190 line 21:** rewrite “could be obtained through RS of near-infrared” as “can be observed at near-infrared...”

**Pg.3190 line 23:** Introduction; not clear what is actually meant here by Snow Water Equivalent; please define prior to introducing eq. (1);

**Pg.3191 line 6:** what do you actually mean by “snow existence durations”? the combination of these two words is particularly confusing;

**Pg.3191 line 10:** the sentence “The discrimination of snow existence under clouds..” should be re-written by replacing “discrimination” with “detection” or “assessment”

**Pg.3191 line 19:** the sentence “Abundant data were wasted because it was...” is obscure, please rewrite.

**Pg.3193 line 7:** rewrite the sentence “Resolution of the MODIS data was degraded from 500 to 50m in order to match the DEM” as e.g. “the MODIS data were resampled to match the 50 m resolution of the DEM”

**Pg.3193 line 19:** eq.(2) cannot hold for any number of cloudy days, so a threshold should be introduced to avoid interpolating linearly over excessively long gaps;

**Pg.3197 line 21:** what is actually meant by  $\tau_d$ ? diffuse transmittance is a very peculiar concept, since diffuse light is by definition not transmitted but scattered; direct transmittance would make more sense, given the comments in the main text;

**Pg.3198 line 3:** the sentences “... $\tau_d$  is assumed to be the same over the small watershed (30.27 km<sup>2</sup>).” and “Complex alpine terrain modified the exchange of direct and 5 diffused solar radiation...” appear contradictory to me, please clarify.

**Pg.3198 line 11:** angle Z undefined in the sentence “Z is the angle between solar beam perpendicular to the slope”; between ... and what?

**Pg.3198 line 13:** neither text or equations make clear why two angles Z and  $k_s$  are needed here; Z seems to be sun zenith angle + slope, with the latter being  $k_s$ . It would be clearer using sun zenith angle (for a horizontal surface) and slope.

**Pg. 3199 eq.21:** this equation implies several assumptions which are not explained. If it has been found in literature, then a proper reference is missing.

**Pg. 3200 line 5:** an illustration of the accuracy of linear interpolation for gap-filling the snow cover time series would be very helpful here.

**Pg.3201 line 5:** validation by comparing model-based estimates of snow depth with measurements raises a major question. First, how is snow dept calculated? Could not find any equation giving estimates of snow depth. Second, I understand that having snow cover and snow water equivalent (SWE) it may be possible to estimate snow depth, but the authors must explain exactly how, especially taking into account that (assuming I understood it correctly) the SWE as calculated is an average over a period of time and it is not clear how accurate it can be when used to estimate snow depth on a given day.

**Pg.3201 line 7:** given the values in Fig. 3 a RMSE = 33.9 cm seems too large, please check;

**Pg.3202 line 1:** “refrozen phenomenon” should be “refreezing phenomenon”

**Pg.3202 line 21:** units of soil heat flux density are not correct, should be  $Wm^{-2}$ ; do not use flow, if you mean (as it seems) soil heat flux density. Statements are rather confusing, soil heat flux density is considered constant and small during the snow season, the larger and variable, then still rather small, but still significant in relation with the energy balance of the snow pack. Some reference numbers or an illustration would be interesting. These are also difficult measurements to do, so readers are likely to appreciate it.

**Pg.3203 line 13:** conclusions do not do justice to the interesting work and results presented, the authors are encouraged to elaborate further.