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The Cryosphere Discussions

Interactive comment on “Investigating the dynamics of bulk snow density in dry and moist conditions using a one-dimensional model” *by* C. De Michele et al.

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Dear Colleague,

We would like to thank you for the comments, which are really very interesting. We will consider these carefully while revising the manuscript.

Here we would like to specify that:

1. The title will be modified as kindly suggested;

2. As a general consideration, we would like to make clear that, here, we intend to develop and test a simple snowpack model, with particular emphasis on the snow

density dynamics, which has to be useful for regional hydrology considerations. As a consequence, we work with few input data, but, at the same time with a solid physically based model able to reply to forcings modifications.

3. Your comment about soil heat exchanges is interesting, and probably more details are needed in the manuscript. As a simplifying element, we consider the soil as a thermal inactive boundary, since it is in agreement with SNOTEL settings where the snowpack is not in direct contact with the soil, it lies on a snow pillow, which can be interpreted as an isolating barrier. Furthermore, the effect of soil thermal fluxes on snowpack in natural conditions is probably limited to the first days of the accumulation season.

4. Concerning the cold content of the snowpack, we are really interested in this aspect, and we will surly rethink about it in the next developments of the model (at least, we will give more details in the revised version of the manuscript, stressing the importance of this issue). As a simple way of accounting for it, we could increase the temperature threshold of the degree-hour process, to force the snow melting to begin when the air temperature is greater than a positive value.

5. As for rain-on-snow events, we do think that rain has an important effect on settling and melting phenomena. Nonetheless, we assume that the evolution law for the snow density considers uncoupled mechanical and hydraulic forcings. As first attempt, we tried to model dry density only starting from mechanical forcings. We are aware that many rheological formulations are available, which characterize the evolution of snow density as also function of liquid water content (as highlighted also by, for example, Marshall et al., 1999, in addition to Vionnet et al. 2012), but the different contributes of pore saturation and wet compaction on snow density dynamics are hardly noticeable with this kind of measured data. As for total mass simulations, we noticed a tendency to underestimate SWE which, in a sort of way, is opposed to this problem. Anyway, future developments of this work are intended to precisely characterize the mechanical contribute of liquid water.

6. Concerning the valildation of the liquid water dynamics, it represents our main aim in the future. We did appreciate your indication of NASA remote sensed data, and we will surely try to validate our results on them.

Concerning the specific comments, we would like to answer to some specific questions you presented:

- Number 9: As for the 0°C threshold in section 2.1, we would like to specify that it is just a theoretical consideration, which has no effect on the model formulation. As visible in the development of the model, no temperature threshold is imposed to the existence of hw , which can exist even if air temperature is below or equal to 0°C, and which is bound to snow melting and direct outflow. We are going to fix this misconception.

- Number 17: since the snow temperature profile is bilinear (if air temperature is negative), no integral is numerically performed, since the mean snow temperature can be easily calculated adopting a weighted average between a first layer of mean temperature equal to TA/2 and a deep layer of mean snow temperature equal to 0°C.

- Number 18: please refer to our 2 reply.

- Number 23: please refer to point 4 of this answer. We are going to clarify the role of the “editing” threshold.

- Number 30: no wind effect is considered.

- Number 32: this summary will be provided.

- Number 37: we will insert a description of accuracy and errors in the revised version of the article.

- Number 38: The “temperature filter” has been adopted to remove small fluctuations existing in snow depth data and due to temperature oscillations (flutter). In fact, since snow depth data are derived from the measure of the travel time of a ultrasonic signal in the air (which is sent by a sensor, reflected by snow surface and then received

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by the sensor itself), and since the speed of the signal in air is strongly temperature-dependent, snow depth data at the hourly scale can vary quickly with temperatures (even of some inches), without real snow events occurring (this can be noticed analyzing daily data series, which are controlled and validated by SNOTEL). The instrument does measure air temperature and correct the speed of the signal, but turn out to be inefficient with quick variations. As a consequence, since every positive increment of snow depth is considered in the model as a solid event, it is necessary to distinguish between real hourly events and noise. The filter is based on the assumption that temperature fluctuations and recorded snow depth oscillations are related. We are preparing a new contribution that will specifically focus on this issue and on the extension of the analysis on many other SNOTEL stations, anyway we will make clear this point in the revised version of the manuscript.

- Number 41: Nash-Sutcliffe coefficients are calculated only onto the existence period of the snowpack.

- Number 45 Concerning the comparison between SNOTEL measurements considered and Swiss measurements considered by Techel and Pielmeier (2011), we would like to point out that the comparison is only qualitative. This comparison is reasonable because: 1) the duration and timing of the accumulation season (October-March) and melting season (April-July) are the same, 2)Mean winter air temperatures in the considered SNOTEL sites are respectively of -0.6 °C for S1 and -3.5 °C for S2 (on the considered periods), which seem to be of the same order of magnitude of Alpine sites as cited in Marty and Meister (2012) and as reported by MeteoSwiss sites at the same heights.

- Number 48: Only hourly data have been used in this contribute, both in calibration and validation.

- Number 49: it is possible, and we will develop a better precipitation editing to account for these problems.

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