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Discussions

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Interactive comment on "Cold-to-warm flow regime transition in snow avalanches" *by* Anselm Köhler et al.

Anonymous Referee #1

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The paper by Köhler et al presents detailed measurements obtained from radar techniques (the GEODAR, already presented in a number of previous papers, and the pulse-Doppler system), conducted at Vallée de la Sionne avalanche test site, Switzerland. The authors pay attention to the cold-to-warm transition during flow propagation, by considering in parallel the temperature calculated from the snow cover model SNOWPACK. The introduction provides some general ideas on the problem of wet-snow avalanche and more particularly on the cold-to-warm transition problem with some relevant recently published papers. Section 2 presents a brief description of the two radar techniques used and the snow cover reconstruction with SNOWPACK, as well as the data sets. Section 3 describes the results by distinguishing between complete and partial warm-to-cold transitions. It is first based on one example for

each transition and two key graphs (Figs. 5 and 6) including all the avalanche events considered are then presented and shortly discussed. Section 4 is a more extended discussion on the results. The limitations of the study are discussed too. Finally, section 5 concludes the manuscript by synthesizing the main results and discussing future works to be done.

General comment:

The present paper relies on radar techniques that are advanced in situ measurements methods to "look inside the avalanches", following a number of recent studies (about GEODAR in particular). By coupling those radar measurements with snow cover reconstruction (the temperature in particular) and making use of the rough assumption that the temperature of the flowing snow is equal to the snow cover temperature, the authors are able to highlight a relation between the degree of cold-to-warm transition (partial versus complete) and the altitude where the snow cover temperature is -1°C. Though -1°C has been previously identified as a threshold temperature controlling the transition between nearly no granulation and efficient snow granulation (see the controlled experiments done by Steinkogler et al, 2015), it may appear as an arbitrary threshold.

I enjoyed the reading of the paper. The result shown on Fig. 5 is quite remarkable. The topic addressed in the manuscript is timely. I believe that the manuscript can deserve publication if the authors make an effort to revise some points. The success of Fig. 6 is somewhat counter-balanced by the result shown on Fig. 6. My main concern on the scientific content is that a sensitivity analysis to the choice of some thresholds (thickness of 0.5 m for the snow cover taken into account, temperature threshold of -1°C) is missing. Including such a sensitivity analysis to changing those thresholds is needed in order to reinforce the arguments provided by the authors on the physics of the cold-to-warm transition. How the plots shown on Fig. 5 and 6 would be changed by choosing other values of those thresholds?

I would have a request on the organization of the paper, in addition. The discussion section (section 4) is not well-organized. I invite the authors to make it much more readable. A couple of points that are direct interpretations of key plots shown on Figs. 5 and 6 should be discussed in more detail and moved to section 3.3. The discussion section could be split into two sub-sections: one for a general discussion on the main results and one about the limitations of the methods used.

Please find below a detailed list of major to more minor comments on the manuscript.

List of major/minor points:

Sec. 1 - Introduction

- page 1, line 22: [..., whereas dense flow regimes, especially warm regimes, can be diverted or even stopped.]. This sentence is somewhat reductive. I agree that rapid dry- and cold-snow avalanches are difficult to divert and stop. But some flow regimes of wet-snow avalanches can pose serious problems too. Their interaction with protection structures is sometimes very complex, due to nearly unpredictable flow trajectories around avalanche dams (see some examples in Johannesson et al., 2009; European Handbook, chapter 8). Could you please qualify your statement.

- p. 2, lines 9-10: [when liquid water is still expected to be absent.]. I would remove this statement given the fact that it is now well-established that localized melting can occur at ambient temperatures a few degrees below the freezing point (Dash et al, RMP 2006; Turnbull, PRL 2011).

- p. 2, end of line 10: the existence of that quasi-liquid layer in flowing snow has two consequences. It can increase snow cohesion on the one side (and thus increase the size of aggregates) but it may also lubricate the contacts between snow aggregates on the other side (thus enhance flow mobility). Maybe the second effect could be shortly discussed, in addition.

- p. 2, lines 20-21: [A partial transition affects only the tail of the flowing avalanche and

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the final run-out is still cold-dominated.]. This sentence suggests that the transition does not occur at the front but mostly at the tail. Could it be that such a scenario with the cold-to-warm transition occurring at the front does exist?

- p. 3, lines 13-14: why this arbitrary value of 0.5 m? The statement [Despite the crudeness of this measure, we assume that...] needs clarification. Please could you justify? Maybe you could explicitly refer to the paper section which addresses in detail the assumptions made.

- p. 3, lines 20-21: [Finally, the discussion (sec. 4) brings together both result parts and the study is finished by a conclusion (sec. 5).]. This sentence is quite easy: I guess you could propose a more precise sentence, more relevant to the content of your paper in order to announce both 'discussion' and 'conclusion' sections.

Sec. 2 - Methods and data

- p. 4, line 15-16: maybe you could give (at least) one relevant reference already published for each system, the older system and the newer system.

- Fig. 1, caption, line 3 (p. 5): 'shown' (not 'show')

- p. 6, lines 12-23: this part justifies your assumptions made (in particular the 0.5 m). Please refer to this part at p. 3, line 13, in the introduction (see a previous comment).

- p. 8, Table 1: could you please provide an order of magnitude of the error/uncertainty on P_c and P_w? And thus F_t?

- p. 9, lines 10-13: is there any uncertainty on this threshold of -1°C between warm and cold regimes? Temperature is certainly a very important control parameter but other factors may come into play. Maybe you could discuss this a bit (see another comment below, on Fig. 6).

Sec.3 - Results

- p. 12, lines 3-12: this is a very interesting observation, providing a quantitative proof

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of a mechanism known from the field experience gained by some snow avalanche experts. Under a context of climate change / global warming, we may expect more events with rain occurring at high altitude on the snow cover during winter. Your measurements are relevant to this problem. Maybe you could add a short word on this point here.

- p. 12, lines 21-24: [... This discrepancy corroborates the turbulent character of both surges.]. Could you please explain better what you mean here? Do the differences stem from different positions of the devices and/or assumptions made with respect to main flow direction? As such, very turbulent flows, with significant velocities in all (3D) directions can produce different results depending on the technique used. This part needs more clarification.

- p. 13, Eq. (5): could you please give an uncertainty on F_t? (back to a previous comment on uncertainties on P_w and P_c). And report this uncertainty on Fig. 5.

- Fig. 5: it is nice to see this correlation between H_s and your F_t. Would be nice too to study the sensitivity of the plot to changing the threshold of -1°C. Would that plot be improved or deteriorated by choosing a different temperature threshold (below or above -1°C)?

- p. 14 - 15. That you use the linear fit to extrapolate and obtain the value of 860 m a.s.l. for F_t=-1 is questionable to me. Because it does concern the arrest conditions of the avalanche, I guess the effect of local topography coupled with the snow (flowing/deposited and entrained) properties is crucial. I would suggest that either you don't extrapolate or your provide more critical discussion on that result.

- p. 15, lines 3-6, and Fig. 6: I may interpret this plot showing H_s versus H_t as a proof that (i) the -1°C threshold may a bit arbitrary and (ii) other factors come into play. Those points need more critical discussion. Maybe some arguments given in the discussion should be already developed here (see another comment thereafter).

Sec.4 - Discussion

- p. 16, lines 10-11: that the flow regime in the run-out zone can be estimated when H_s is known relies on the linear fit proposed for the relation between F_t and H_s. You could be more precise here, and add at the same time that this will need further investigation: linear fit or other relation? range of F_t for which the linear fit is valid? asymptotic behaviors when F_t tends towards -1 or +1?. See also a previous comment.

- a general comment: this section is difficult to read because there are too many ideas. I would propose to put some points (in particular: entrainment at the surface versus deeper in the snow cover, effect of the topography, front dynamics) earlier in Sec. 3 and maybe extend the discussion on those points in Sec. 3, because they are direct and important interpretations of the plots shown on Figs. 5 and 6. Also, the remaining points (not transfered to sec. 3) could be a sub-section 4.1 and the discussion on the limitations of the method (starting from line 26, p. 17) could be a sub-section 4.2.

Sec. 5 - conclusions

- p. 18, line 26: the flow regime influences not only the pressure on structures but also the flow mobility (run-out: velocity and volume). Please add those points.

- p. 18, line 32: please remove "robust relation" but (for instance) use "correlation" instead or keep "relation" only. I agree that this result is very nice but this result will need further validation.

- p. 19, lines 3-6: how those values of 300 m and 500 m depend on some (arbitrary) choices you made? A sensitivity analysis (of plots on Fig. 5 and Fig. 6) to changing the threshold values for the temperature (-1°C here) and the snow cover thickness taken into account (0.5m) is missing in your study.