The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-67-RC2, 2018

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

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Received and published: 10 July 2018

Content of the paper

Over a period of some three decades, our capability of numerically simulating the evolution of the snow cover in some detail in 3D has developed to a level where these tools can be used in diverse applications with some confidence (notwithstanding major residual problems). Greatly developed and diversified experimental techniques—all of them installed at the Vallée de la Sionne test site in Switzerland—have given us an unprecedented, detailed view of the processes inside flowing avalanches. One of the most conspicuous of these new instruments is GEODAR, a phase-array-based radar system that eventually will allow high-resolution 3D mapping of entire avalanches through

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time. Finally, thanks to IR imaging, the role of the snow temperature in the snow cover and the flowing avalanche has become a major focal point of research in avalanche science, mostly due to work at SLF in Switzerland.

In this paper, the authors combine these three major elements—snow-cover simulation, 18 avalanche measurements with GEODAR and Doppler radar, as well as previously gained insight into the critical role of snow temperatures near melting on the flow regime of avalanches. The main objectives are (i) to verify the effect of snow-cover temperature by comparing a rather large sample of measured avalanches and (ii) to quantify in a simple way how the degree of transition from a dry-snow flow regime to a wet-snow flow regime depends on the snow-cover temperature along the avalanche path.

The measurements with GEODAR and Doppler radar allow distinction between different flow regimes, as recently shown in a different paper by some of the same authors. Moreover, they can distinguish "complete" from "partial" transitions and locate a representative transition point. From the run-out distances of the warm and cold parts of the flow, they construct a transition index F_t and relate it, for each event, to the reconstructed altitude H_s where the mean temperature of the top layers of the snow cover reached $-1°$C. They find a clear positive correlation between F_t and H_s. Similarly, warm avalanches (undergoing a full transition) are shown to make the transition to the warm-snow flow regime above the altitude H_s, whereas that point is below H_s for all cold-snow avalanches (with partial transition only).

General comments

The transition index proposed by the authors is a clever attempt to (semi-)quantitatively capture an aspect of the flow-regime transition process with a minimum number of observable quantities. In order to link it to the thermal regime of the flow, the transition altitude, H_t, is invoked and statistically compared to the altitude H_s below which the upper snow cover is warm. The authors seem to be aware of the difficulties and limitations of this approach, but it might be useful to spell them out more explicitly. From my point of view, the following points are particularly important:

- The transition index will probably be most useful for avalanches with drop heights of 500 m or more. For smaller avalanches, H_s tends to be either above the release area or below the run-out area.
- While H_s can be determined wherever and whenever there is enough meteorological data for running snow-cover simulations, finding H_t for a given event requires either detailed investigation of the avalanche deposits or measurements with a GEODAR or advanced Doppler radar.
- For use as a predictive tool, e.g. for road closures or evacuations, a plot like Fig. 6, containing many events, would be necessary. Probably, such copious and detailed data is available only for a handful of avalanche paths worldwide.

That being said, I agree, however, that the transition index and the correlation between H_s and H_t are a meaningful way of demonstrating the relevance of the thermal regime for the flow of avalanches.

The method for determining the uncertainty in the snow temperatures $\bar{T}_{2,3}$ remains unclear to me. The way I read the text, they calculate the standard deviations as $\sigma_T = [\sum_1^N (T_i - \bar{T})^2/(N-1)]^{1/2}$, with the sum extending over the computational layers used by SNOWPACK in the top 0.5 m of the snow cover. If this is indeed what they mean, I cannot see how this should be connected to the uncertainty of H_s—that uncertainty is more directly connected to the question whether a linear extrapolation of snow pack temperatures is admissible. As a consequence, I cannot assess whether the authors' approach for determining the consequent uncertainty in H_s is sound or too optimistic. The way they do it according to Fig. 1 assumes that the deviations of T_2 and T_3 from their means $\bar{T}_2$ and $\bar{T}_3$ are tightly and positively correlated. If this is not the case, the uncertainty in H_s will be much larger. This would, however, have considerable importance for Figs. 5 and 6 and for the firmness of conclusions that can be drawn from them. These issues have also been commented upon by the other reviewer and need to be addressed carefully by the authors.

The title of the paper is more general than its content in that dynamical aspects are more or less completely left out. However, the GEODAR data offer a unique opportunity to quantify some aspects of the dynamics: Figures 3 and 4 suggest that a major component of the avalanche first moves at a nearly constant speed, then decelerates over a period of 5–10 s, and then continues again at nearly constant speed. From the curvature of the streaks, it should be relatively straightforward to extract the deceleration, and since the location is also known fairly precisely, the retarding accelerations before, during and after the cold-to-warm flow-regime transition can be determined. This is a rather remarkable phenomenon with far-reaching consequences for modeling the flow. I do not understand why the authors hardly mention this, and I strongly encourage them to dedicate a subsection or a few paragraphs to an at least preliminary analysis.

Minor points

P1, L4,5: The sentence “The intake of . . . regime transition.” sounds strange and undecided. It is well established by everyday experience and experiments that the rheological properties of (granular) snow change significantly with temperature near the freezing point. Please find a more precise and informative wording.

P1, L8: “. . . the farthest deposit consists of cold snow.”

P1, L23: “on the flow regime”
Earlier references to this phenomenon are
Gauer, P., Lied, K. and Kristensen, K. (2008). On avalanche measurements at the Norwegian full-scale test-site Ryggfonn. *Cold Reg. Sci. Technol.* **51**, 138–155.

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Sovilla, B., Kern, M., Schaer, M. (2010). Slow drag in wet avalanche flow. *J. Glaciol.* **56** (198), 587–592.

P1, L76–86: The logical flow of this section would be improved by moving this paragraph on observations between lines 55 and 56. To make a clear connection to what follows, in L56 one could say "… is now also recognized in modeling."

P2, L1: In my view, calling the velocity-independent part of the impact pressure on obstacles *hydrostatic* is an unfortunate choice. Hydrostatic pressure is the pressure exerted by a fluid at rest, and the term "pressure" is commonly reserved for the isotropic part of the total stress. In the present case, there is no isotropy. Furthermore, the pressure drops significantly (but not to 0) once the avalanche has come to rest. The reason for the height dependence of the normal stress at impact is that the frictional forces between snow particles are proportional to the slope-normal stress, which is essentially of hydrostatic origin. It might be useful to borrow expressions from granular-flow mechanics and replace "dynamic" by "grain-inertia induced", "hydrostatic" by "quasi-static granular" or something similar.

P2, L14: "a halt"

P2, L18: "… and parts of which undergo a transition to a warm-wet regime"

P2, L20: "full" → "entire"

P2, L21–22: "… all the avalanching snow becomes warm and the final runout is determined by the dynamical properties of the warm flow regime."

P2, L30: "more slowly"

P2, L32: Some pictures and descriptions can be found, e.g., on the webpages http://snf.ngi.no/breitzug.040113.html and http://snf.ngi.no/breitzug.050212.html.

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P3, L2: “lof of attention”

P5, Fig. 1: It seems that this figure will occupy most of an entire page, thus there is no need to squeeze things to the point where they become unintelligible. A good solution might be to give the upper panels a common main heading "Avalanche VdlS #17-3030” and each of them a subheading such as “Snowcover temperature from Alpine3D” and “Snow-temperature profiles along centerline” or similar, and analogously for the lower two panels. It took me a long time to (probably) understand the intended meaning of “$\bar{T}$ of profiles $\bar{h}$ 0.5 m depth”.

P6, L7: I do not think you mean to say that temperature profiles cannot be measured automatically, but I cannot guess what you mean to say.

P6, L19: “. . . the typical volume of large avalanches in VdlS, (0.5–1) $\times 10^6$ m^3, by the typical affected are of . . . ”

P6, L30: “. . . crosses the threshold . . . ”

P6, Eq. (2): $H_b \rightarrow H_3$

P7, L1–2: What kind of “standard deviation” is meant? What kind of “law of error propagation” do you apply? From the right panels of Fig. 1 it appears that you assume fluctuations of $\bar{T}_2$ and $\bar{T}_3$ (whatever may be their origin) to be maximally correlated. If one assumed them to be maximally anti-correlated, the grey areas would become much wider at -1°C.

P7, L4: “are” → “is”

P7, L7: “. . . domain is sliced into . . . ”

P7, L12 ff.: This is an importnat passage, please describe this in somewhat more detail.

P7, L13: “temperature $\bar{T}$”

P7, L17: “at the station VDS2. The event #13-3019 . . . ”

P7, L19–20: “. . . reflect the pattern of warm and cold temperatures reasonably well”

P7, L25: “an approximate” → “ a minimum”

P7, L28: “. . . (VDS3) first became operational . . . ”

P8, Table 1: The asterisk in “(McElwaine* et al., 2017)” should be removed in the table legend. Also on P19, L25.
the column ‘GEODAR timestamp’ is difficult to read. Please use ISO notation YYYY-mm-ddTHH:MM, with the letter T separating date and time.

P8, L2: “such as photographs and data from the flow . . . ”

P8, L2: I have never encountered the notion “terrain registration procedure”, and a search in Google does not immediately turn up useful results. Please explain what you mean or use an established notion.

P8, L3: “thought of as a transfer”

P8, L8: Scatterbtains like me have already forgotten that this abbreviation was defined and last used only four pages ago. . .

P8, L9: The term ‘starving–stopping mechanism’ was not introduced literally before, but the readers will probably guess that you mean the same mechanism as referred to on P2, L12.

P9, L5–6: “. . . in the photographs in Fig. 2.”

P9, L16: “themselves”

P9, L18: “extent”

P9, L19: Too sloppy language – a flow regime is not an area or a deposit.
“with the same colors”

P9, L21: “sort of lateral resolution” – please formulate this more precisely and in non-colloquial English.

P9, L22: “When the most distal deposits are cold, . . . ”

P9, L23–25: Do you think that starvation is necessary in this case, or could it be enough that the front picks up warm snow and experiences higher friction? Then it would be possible for the tail to run up on the body and front.
Do you mean to say that it is (theoretically?) obvious that flows in the warm regime are slower than those in the cold regime, or do you refer to GEODAR measurements? It might be best to remove this sentence. If you keep it, you may want to write something like “The flow velocity differs markedly between the cold and warm flow regimes.”

P10, Fig. 2: The insets lack axis labels.

P11, Fig. 3: The plots from the Doppler radar on the right-hand side raise a question at closer examination: The length of the range gates is 25 m in the line of sight according to information given in Sec. 2. This may correspond to about 30 m along the flow direction. At a dominant velocity of approx. 30 m/s, the front should take about 1 s to cross the range gate, which is compatible with the plots. However, between $t = 12.5$ s and 15 s, a bi-modal velocity distribution with dominant velocities diminishing from 10 to 1 m/s in the first surge and from 20 to 15 m/s in the second.

P11, L22,26: “farthest”

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P11, L1: “farthest”

P11, L10–11: Suddenly, there seem to be different warm flow regimes. Do you perhaps mean different parts of the avalanche that are in the warm flow regime?

P12, L7: “decline_”

P12, L8–9: “. . . changed rather gradually . . . ” – the dominant velocity inside the range gate diminishes at a rate of up to 0.8 g, akin to an emergency stop with a car!
I cannot see velocities as high as 30 m/s in the second surge in Fig. 3 except right when it enters range gate 18.
“. . . , the velocity distribution ranges from . . . to . . . "

P12, L15: “from the left-hand side”

P12, L16: “influenced” → “wetted”

P12, L17: Strange sentence – how can an “altitude H_s” . . . “visually summarize” a “snow cover”???

P12, L22: “discrepancy” → “difference”

P12, L29: “farthest”

P13, L7: Should one perhaps mention explicitly that this transition point is *not* a material point but more akin to a shock front?

P13, L8: An alternative explanation would probably be that material flowing into the range gate later is significantly slower and therefore stops more easily. In order to decide this (rather relevant) question about piling up, one would have to approximately reconstruct the flow of avalanche parcels across the range gate, adjusting the longitudinal profile of velocity so as to reproduce the recorded intensity distribution in the velocity–time plot.

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P13, L14 and throughout rest of the text: It is never mentioned that F_t is used to multiply some other physically relevant quantity. Therefore, it should be called a transition *index* rather than a transition *factor*.

P14,15: Figures 5 and 6 are somewhat large in this manuscript. Shrinking them by about 50% so that they can be placed side by side on a single page might still be sufficient.

P14, L3–4: "... have _ transition factors $F_t = -0.18$ (...) and $F_t = +0.31$ _ (#13-3019)."

P14, L6: To be pedantic, one ought to say something like "..., and the set of values is well distributed over this range".

P15, Fig. 6: "Altitude of transition, H_t, against altitude of the $-1°$C _ line, H_s."
"happen" → "occurs", "characterizes"

P15, L1–2: If one thinks about the dynamics of avalanches and the topography of Vallée de la Sionne, it is obvious why the naïve extrapolation fails, but it would probably be helpful to some readers if this was explained.

P16, L1: "1700" → "1800"???

P16, L9: "superficial" → "surficial"

P16, L10: Do you mean to say that the concept of H_s can be applied only to Vallée de la Sionne? What is then the value of your approach ?

P16, L16: "ldots of deeper_ and therefore warmer layers

P16, L20: "results in" → "undergoes"

P16, L27: "structures" → "texture",
"show_ that"
"can produce completely warm-wet deposits"

P16, L31: “higher” → “more”

P17, L5: “altitude at the end”

P17, L5: “gently inclined runout area”

P17, L12–13: “. . . able to hold back mass from . . . ” – This can easily be (mis-)read as you suggesting that tension forces are exerted on the front by the tail.

P17, L15: “more importantly”

P17, L27: “may play a role in”

P17, L27: “with regard to”

P17, L34–35: This assumption does not really affect what you have done because you have not really considered the rheology and mechanics of flowing snow. This will, however, become important when one tries to take this approach from an empirical method tied to a specific site to a general one, applicable to any avalanche path.

P18, L15: “relatively gentle”

P18, L19: “As an example,”

P18, L23: “common in large”

P18, L24: “occurs”, ”. . . of snow grains causes . . . ”

P18, L28: “unexpectedly long”

P19, L3: “almost all large”

P19, L7: “. . . as a first step in developing a method for predicting the. . . ”

P19, L22: "... mitigation procedures be adapted..."

Recommendation

This paper contains a number of novel aspects, in particular the combination of several advanced experimental techniques in avalanche dynamics with snow-cover modeling. The topic of thermal effects in avalanche dynamics has recently attracted much interest, thus the paper is undoubtedly timely. The concepts discussed here may also help guiding future modeling efforts.

The data presented in this paper is unique due to the GEODAR. As far as this can be judged from the outside, the data analysis and the snow cover simulations appear to be sound.

The authors have—presumably deliberately—adopted a phenomenological approach and not tried to interpret their data through semi-quantitative or quantitative models. I personally think that a simple physical analysis, e.g. an estimate of the different components of the energy balances of the considered avalanche events along their paths, would be highly interesting and add value to the paper. Such an analysis might give some indications as to the predictive power of the transition index for avalanches in the Vallée de la Sionne and how it might be used as a predictive tool in other avalanche paths. I do not, however, insist on this point.

As mentioned under "General comments", I think it is a real pity that the authors do not present at least a preliminary analysis of the dynamical aspects that the GEODAR images put right under the readers' noses. Adding such an analysis would significantly increase the value of this paper.

Contrary to Reviewer #1, I do not have objections against the organization of the paper. I find the presentation to be logical and (except for some details mentioned above) easy to understand. The figures are informative and well executed; some minor modifications have been indicated above. However, the writing style, grammar and spelling

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definitely need attention to the details.

All aspects considered, I recommend the paper for publication in The Cryosphere after minor corrections (and the mentioned additions).