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The Cryosphere Discussions

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Interactive Comment

Interactive comment on "Thermal energy in dry snow avalanches" *by* W. Steinkogler et al.

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We thank the reviewer for the very detailed comments. The insight and criticism helped us to produce a better and more accurate analysis, which will be presented in a revised paper version.

1 General comments 1.1 Summary of goals, approaches and conclusions Steinkogler et al. studied the thermal properties of three artificially released dry avalanches. The aim was to estimate potential sources of thermal energy in an avalanche. As a minor aim the authors wanted to investigate the application potential of an infrared camera for measuring the spatial temperature distribution of an avalanche. The temperature distribution of the avalanches was quantitatively measured with thermometers regularly used in snow pits, snow depth probes with attached thermistors, and rather qualitatively with an infrared camera. With snow profiles the authors could identify warmer snow

temperatures in the deposition of the avalanche than anywhere else in the undisturbed profiles next to the avalanche. The authors concluded with these observations that entrainment cannot explain these relatively warm snow temperatures in the deposition zone. After theoretically excluding other sources, friction was discussed to be the main reason for this observation. Furthermore, the authors concluded that warming due to friction was mainly dependent on the elevation drop, while warming due to entrainment was dependent on many factors related to the specific avalanche. Lateral temperature variations in the depositions of the avalanche were related to assumed flow regimes of the avalanche.

1.2 Evaluation of the overall quality

RC: Studying the temperature distribution in an avalanche is worthwhile and will provide relevant verification data to avalanche dynamic models. The identification of friction as a major thermal energy source in an avalanche is new to my knowledge. Also, the use of an infrared camera is new for this purpose, although the relevant results were obtained by traditional measurements. The purpose of the work is clear and appropriate methods were used to address the questions (except for the validation of the infrared camera, see comments below). The manuscript is written in a clear and concise manner and in good English. The figures are well prepared and add to the understanding of the manuscript. The relevance of this manuscript lies in the identification of friction as major thermal energy source. This impact can be enhanced with a better literature review of other measuring and modelling attempts of friction in dry snow avalanches.

AC: We now provide additional literature on the proposed measuring and modeling approaches. Special focus is put on the potentially large variations of thermal energy due to entrainment.

RC: Furthermore, I think it is within the scope of such a contribution to include an avalanche dynamic model with the purpose of (i) to initially verify the friction implemen-

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tation (which was mentioned in the Conclusions as an outlook) and (ii) to better quantify the thermal energy contribution of friction considering mass entrainment and deposition along the path. This would shift the rather descriptive manner of this manuscript to a more analytical manner, and would more clearly answer the question how relevant friction is for avalanche dynamics.

AC: We agree with the reviewer on the fact that a more detailed conceptual treatment of the thermal energy contributions is needed. We therefore now present a mass dependent calculation which addresses the reviewer request to improve the descriptive manner of this part of the paper. This includes a representation of the bulk effect of entrained snow, which may be at a different temperature compared to the released snow. We do not aim to include a more complex numerical avalanche dynamics model in our paper as we aim to provide initial experimental data that can be used to further verify and improve such models. The additional analysis that will be presented in the revised version maintains that frictional heating is correctly described by the equation originally presented. We made an effort, however, to point out that the equation in general describes the loss of potential energy as a mass increment or finite volume descends a slope. Taking this into consideration, the drop height had to be adapted in the refined calculations, confirming however that frictional heating is the dominant term. The refined calculations now show explicitly the contribution of entrained snow to frictional heating. The contribution that entrained snow has because of its different temperature is small for our cases but is also explicitly calculated in the revised version.

In the following I will focus on the questions, if the results support the conclusions, if the aim to investigate the usability of an infrared camera is met, and if the methods used are appropriately described. I am convinced the authors are able to address my specific comments below. Thus I recommend to publish this manuscript in The Cryosphere after Major Revisions.

2 Specific comments 2.1 Do the results support the conclusions? RC: I understand what it means to gather data from real avalanche experiments, which prevents to in-

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clude several boundary conditions. But the main conclusions of the manuscript are formulated in a too general way, which is not supported by the data collected from two (three) studied avalanches under quite similar conditions. Other important conclusions are rather supported my common knowledge than by own data. I suggest a more careful wording of the following conclusions: 2.1.1 Friction is mainly dependent on the elevation drop “Our results confirm that for the investigated avalanches the thermal energy increase due to friction is mainly depending on the elevation drop of the avalanche and thus a rather constant value (for a specific avalanche path and typology)” (in the Abstract). The authors could not investigate avalanches with several different elevation drops to justify this statement. Only an oversimplified friction calculation supports the statement that friction is mainly dependent on elevation drop. This generalization is especially problematic since the authors were not able to separate the sources of warming. I suggest a less generalizing formulation.

AC: We agree with the reviewer that the main conclusions were formulated too general. We therefore now provide a more detailed analysis of the friction and entrainment contribution to the overall warming (see our detailed response to comment 1.2 of Reviewer 1 above). As suggested we will reformulate the conclusions to clearer distinguish results that can be transferred to other situations and results that are only valid for the specific avalanches investigated in this study.

2.1.2 Entrainment is very specific RC: “... warming due to entrainment was very specific to the individual avalanche and depended on the temperature of the snow along the path and the erosion depth ...” (in the abstract). Similarly, the authors could not investigate avalanches with different entrainment characteristics and their impact on warming. AC: We will change this formulation in the abstract. In the paper we now describe the mass balance in more detail. Furthermore, we now clearly separate the contributions due to friction and entrainment. We now can show that indeed there is a difference in the entrainment characteristics of the investigated avalanches. However the reviewer is correct in observing that the entrainment is not so “important” for the thermal balance in these two cases. In fact, while the entrainment characteristics of the two avalanches are quite different (avalanche 2 is characterized by a growth index Ig=me/mr=of 3.7 while avalanche 3 as Ig=1.6) the temperature difference between release and entrained snow is almost negligible for avalanche #2. This makes friction the main contribution for the warming of the deposit, in this specific case. This is now better described in the paper.

RC: The conclusion is based on general knowledge of a temperature gradient in the snowpack (p. 5806, line 5ff) and the observation of different surface temperatures on the bed surface of two avalanches.

AC: The reviewer is correct that this conclusion cannot be based on the specific measurements we have analyzed since in our case entrainment has a small contribution in the overall thermal balance. We will reformulate this specific conclusion but discuss what we may expect if the entrainment goes into deeper and warmer layers. Yet, we are not sure whether the reviewer misunderstood our method to calculate the temperature of the entrained snow. Since the thermal balance for the two avalanches is based on measurements, combining TLS data and manual snow profiles to assess the depth of the entrained layer and the respective temperatures, our results are based on measurements and not on general knowledge of a temperature gradient in the snow cover.

RC: The authors should consider rewording which indicates that this statement is rather based on reasonable theoretical considerations and common knowledge (temperature gradient in snowpack and irregular entrainment and erosion depth) than from own collected data.

AC: We reworded our statements to be clear where we refer to our measurements and where to literature.

2.1.3 Relation between flow regime and thermal signal RC: Throughout the manuscript the authors relate the thermal signal with different flow regimes, e.g. in Figure 7 and in

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the text: “Lateral profiles ... allowed to differentiate between undisturbed snow cover, the deposits of a fluidized layer ... and the dense core.” (p. 5802 line 22ff). The authors observed differences of surface temperatures obtained by an infrared camera (Fig 7). The authors could hypothesize that these observed differences coincides with observations typical for different flow regimes. I do not think that the presented results are indeed allowing to identify deposits of different flow regimes. The authors did not describe in the methods that they systematically observed deposits indicating different flow regimes (Issler et al., 2008). This was much later added in the Discussion section. I suggest a more careful wording when relating thermal observations with assumed flow regimes. In the current way of writing I am not convinced that the authors knew exactly where the depositions of different flow regimes were located. Moreover, I doubt that the authors can be completely sure that mentioned flow regimes were existent in the studied avalanches. Cited authors only “believe” that a fluidized flow exists in dry snow avalanches (Gauer et al., 2008). I think the authors need to be clearer that they have a strong indication for this fluidized flow for their studied avalanches and know where the depositions were located (and communicate this accordingly), before they can be sure that the thermal signal allows to detect related deposits. This issue can also be solved with a more careful wording and a clearer communication of how the avalanche depositions were observed in the methods section.

AC: We added a description of the criteria for the identification of Issler et al., 2008 in the Methods section. In agreement with the reviewer comment we cannot be 100% sure that the observed lateral changes in the IRT measurements can be related to a fluidized layer (the same restriction applies to Issler et al., 2008 since direct in-flow measurements are lacking there as well). Yet, we think the IRT measurements are in agreement with observations in the deposits and the criteria formulated by Issler et al., 2008. They are therefore an informative addition to the other field observations. Regardless of whether the IRT signal differences arise from actual temperature differences or varying roughness, it appears that different areas in the deposits of an avalanche can be distinguished.

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The reviewer is correct that the association of these regions to specific flow regimes is questionable, as questionable as the existence of a fluidized layer in avalanches is in the first place! As a compromise, in agreement with our own field observations we will replace “fluidized layer” with “thin deposit area” in a revised paper version. We will discuss in the discussion section possible reasons for these differences. It is clear that to finally answer this question, further field and laboratory studies will be necessary (see answer to comment 2.2 of reviewer 1).

The authors are convinced that the identified boundaries in the IRT data, the TLS and the measurements in the field match. This could be verified with GPS measurements, geo-referencing and matching persons visible in the IRT data with their GPS position. This also allowed us to locate the transects in Figure 6 and 7 as well as the locations of the point measurements in Figure 4. As suggested by the reviewer this will communicated more clearly in the revised paper.

2.2 Investigate the potential of the infrared camera RC: As a minor aim the authors wanted to investigate the application potential of an infrared camera for measuring the spatial temperature distribution (Introduction p. 5795 line 18ff; Conclusion p. 5810 line 1f). The authors only presented three data points, which is – as the authors stated – very basic. The authors used the camera in Figure 7 for a quantitative assessment of the surface temperatures. Additionally, they assigned different flow regimes to slight differences in this temperature, much less than 1 deg between an assumed fluidized layer and a dense core (Figure 7b). The cited literature (Schirmer and Jamieson, 2014) suggested that snow temperatures of a snow pit adapt fast and irregularly on a pit wall based on roughness after being exposed to the atmosphere. This case might be comparable: Avalanche deposits are similarly not in equilibrium with the atmosphere (as recognized by the authors, page 5809 line 6ff), and the deposits of assumed flow regimes may have a different surface roughness. This indicates that the situation described in the literature of a fast adaption of snow temperatures due to roughness differences may apply here as well.

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AC: In contrast to Schirmer and Jamieson (2014), we did not investigate single roughness elements of convex or concave shape on the centimeter scale. With the used IRT cameras and objectives the pixel size of the footprint is approximately 1 m with the old camera model and 0.5 m with the new model (this information is now added to the methods sections). In our case we therefore receive an IRT signal which represents an average over the footprint area and has a more isotropic signal character than from a single roughness element. As shown in Figure 4, initially no large difference in cooling rates between relatively smooth bed surface of the release (IRT_release) and the rough debris areas (IRT_depo) could be measured. We agree with the reviewer that the effect of different roughness surfaces needs to be investigated further, preferentially with controlled laboratory experiments. We do not think that our data allows for more detailed conclusions due to limitations in control of atmospheric conditions, viewing angle, footprint size and distance between sensor and object (as discussed in the method section). Even if we cannot exclude possible absolute temperature errors due to superficial roughness, the different deposition areas can be clearly distinguished by the IRT camera. Since we only interpret these relative values in a qualitative way our conclusions are not affected by imprecisions in absolute temperatures.

RC: For the drawn conclusions (2.1.2 and 2.1.3) and for the proposed aim of investigating the potential of an infrared camera a more thorough validation is needed. My experience with measuring surface temperatures with metal thermometers is that they can be wrong by several degrees due to heating of diffuse solar radiation after shading the sensor. This means that the reference temperature used to validate the thermal camera is questionable. The comparison with a snow layer in a fracture line profile seems more promising.

AC: We agree with the reviewer that the measurements in the fracture line profile might be more reliable. The comparison measurements with the digital metal thermometers were conducted according to current snow profile standards. To answer this in more detail, laboratory experiments, similar to Schirmer and Jamieson (2014), should be conducted.

RC: However, a potentially fast cooling of the fracture line was not investigated with the infrared camera.

AC: In all cases the (vertical) fracture line itself was too far away to allow for detailed investigations of its cooling. Yet, we investigated the surface temperature at the release area and but not the fracture line.

RC: Also, the authors have not stated how many minutes or seconds passed between the avalanche release and the infrared image used for this validation. The authors have taken videos of the release which provides data to address this known issue in more detail.

AC: The first images were acquired right after the release. For avalanche #1 and #2 only the older camera model was available which did not allow to record raw data with high, e.g. video, frame rates. Therefore the acquired videos are compressed files and do not allow to access the raw data directly. The new camera model, e.g. used for avalanche #3, would allow to record raw data at high frame rates and enable such analysis in future investigations.

RC: I suggest more careful wording when data of the IR camera is used and the potential is discussed. I would prefer if the authors add a more thorough validation of the infrared camera after an avalanche release, which can partly be done with existing data (how fast is the cooling, is the cooling speed different over different surface roughness, between the three avalanches representing different atmospheric conditions) or with new data.

AC: Figure 4 illustrates the available verification data. We now discuss this in more detail. A more elaborate verification requires laboratory setup and was out of scope for this paper.

2.3 Description of the methods 2.3.1 Infrared camera RC: Please provide necessary information what the chosen emissivity value was, how the apparent temperature was determined, how the relative humidity was determined, how large the distance was from the camera to the avalanche (specifically to the transects in Figure 7), how large distance differences were from the constant value chosen for the whole image. I assume these values can be changed a posteriori and thus the sensibility of these values can be tested. Some minor changes may have large impacts on results caused by the rather large distances in this application. Please mention for Figure 7 how the pixel size was converted in meters.

AC: During all our experiments cold and dry snow conditions prevailed. We therefore chose an emissivity coefficient of 1 for the processing of our data. Again we want to point out here that our main aim was not to achieve highly accurate absolute temperature values and we did not go beyond an accuracy level as demonstrated in Figure 4. We also did not assign different emissivity values to different areas in the avalanche, which would technically be possible in post processing. Due to the dry snow conditions and the cold temperatures with low relative humidity a basic sensitivity study did not show drastic effects on the results, which have to remain mostly qualitative at present. In general, a validation of the IRT camera was out of scope of this paper. In this first approach the camera is used in a qualitative way to assess the spatial differences in temperatures. A nearby automatic weather station allowed to measure the relevant atmospheric parameters during the experiments. Information on the distance between the IRT camera and the release area below the rocks, which was around 800 m, will be added (also see comment 2.2. of reviewer 1 on pixel size of the footprint of the measurements). As the reviewer states, these values can be changed if raw data was recorded. Unfortunately this is not possible for the videos acquired during the release since this data is compressed. For the raw images that where acquired at regular intervals after the avalanche release a post-processing is possible.

RC: Much later was mentioned (for the determination of transported mass) that the image was georeferenced with a digital elevation model (DEM). Was the same method applied for Figure 7?

AC: Information on geo-referencing and pixel to width conversion has already been provided in the response to comment 2.1.3. above.

RC: In the abstract it was mentioned that “this data set” was used to calculate the thermal balance, just after mentioning the infrared camera. This is confusing since only snow profiles were used for this purpose. For the mass assessment I assume these calculations are dependent on the quality of the DEM used for geo-referencing. Please provide the resolution, source and if available, error of the used DEM.

AC: We add this information to the revised paper.

2.3.2 Temperature probes RC: Please provide information of the vertical and horizontal resolution of the probing for Figure 8. For snow temperature measurements in snow profiles, the thermometers are regularly placed for a couple minutes to allow the sensor to adapt to the temperatures. Please provide the Time Constant for this sensor (and own experience when used in snow), your measuring time at one location, and the time used for one transect. Please also mention how the depth of the debris was determined (Fig. 8).

AC: For avalanche #2 a lateral trench was dug through the deposits which allowed to perform regular digital thermometer measurements, as well to measure the depth of the deposit. The measurements were performed well behind the trench wall, in order to avoid exposure of the deposited snow to air. For avalanche #3 the temperature was measured with the vertical probes and measurements were started simultaneously from both sides of the avalanche. Additionally the center profile (Pdepo) was performed at the same time. This took about 1-2 hours in total. A single measurement with the temperature probes took about 3-5 minutes. The depth of the debris could be well observed as a sharp temperature gradient in the temperature measurements itself. To verify this, a trench or regularly spaced pits were dug and the interface between debris and undisturbed snow cover was identified.

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2.3.3 Calculating mass RC: It is not clear to me how the mass was calculated for avalanche #2 since the laser scan is only partly available (Table 1).

AC: Unfortunately the laser scan before the release did not cover the entire avalanche area. Nevertheless, field observations (Figure 1b) showed that the avalanche entrained the same snow layers along the path. We therefore extrapolated the TLS measurements from the upper part of the entrainment area of TLS data to the lower part until the elevation where the deposits started.

3 Technical comments RC: Fig 1c: step down not visible.

AC: Changed terminology to secondary release.

RC: Fig. 2: avalanche #4 is #3, I assume?

AC: The legend has been changed accordingly

RC: Fig. 3 and 5: Please export the infrared data into an external program to avoid plotting unnecessary information and to provide more temperature values in the color bar. This also helps to avoid using colors in the text instead of values ("from pink to orange", p5862 line 6), or a change in the temperature scale. AC: Figure 3 and 5 are compressed videos and therefore do not allow to be processed as raw data in an external program. As indicated in the caption of the figures the presented images are screenshots from the videos.

RC: Fig. 4: Please mention in the caption and in the text to which avalanche this data belongs

AC: We have added the requested info.

RC: Fig. 6a: The transects appear to me that they did not extend into the undisturbed snow, while this is indicated in Figure 7. AC: This is now better illustrated in Figure 6.

RC: Fig. 6c: The transect appear to be not on the avalanche (bad angle). This transect was not used in Fig. 7, so this figure could be deleted.

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AC: This is correct. The figure was removed.

RC: Fig. 8: Please enlarge the axes labels.

AC: Adapted as suggested.

RC: P. 5799, line 8ff. The authors could verify with the available laser scanning data if the entrainment in the gullies was indeed negligible.

AC: TLS measurements were not performed for the upper part of the rock face since the gullies would cause a lot of areas which are shaded from the measurements. Due to the steepness of the terrain (all above 45°) which causes constant avalanching and the very small potential entrainment area in the gullies the mass contribution can be assumed to be small compared to the snow in the slope below the rock face.

RC: P. 5802, line 15ff: The reasonable relation between temperature and erosion depth could be investigated with laser scanning data or measurements of the crown height.

AC: In fact this can exactly be observed for the release area of avalanche #2 when comparing the TLS data in Figure 2 (larger release depth) with Figure 6b (warmer snow surface temperatures).

RC: P. 5805, line 9ff: Please discuss why you think the 1.5 deg is an upper limit. If additional mass is entrained, more potential energy is available, which will increase deltaT, while early deposition of mass on the track does the opposite. Do the authors think early deposition dominates?

AC: We have now corrected this conclusion. See also answer to comment "1.2 Evaluation of the overall quality". Our initial calculation assumed that the entire mass, i.e. the released and entrained mass, is concentrated at the elevation of the release area. This is an extreme case in the sense that, normally, (large) parts of the mass is entrained along the path, lowering the center of mass, and thus providing less potential energy that can be transformed into heat. We now stick more to our avalanche situation and we calculate the friction contribution using the real centre of mass, providing a more detailed mass- and elevation-dependent calculation as discussed in detail above. Early deposition does for sure not dominate for the observed avalanches. Both avalanches exhibit a pronounced growth index.

RC: P. 5806, line 10ff: Do the authors mean “altitudinal changes” (gradients) are variable, or temperatures are variable due to an altitudinal gradient.

AC: We intended to indicate that snow temperatures can be variable due to an elevation gradient, with warmer snow temperatures typically at lower elevations. This has been shown in the cited paper of Steinkogler et. al, 2013). We reformulate this in the paper to be more understandable.

RC: P. 5806, line 19ff: I suggest to consider that warmer temperatures at the bottom of the flow could be explained with upper layers were cooling faster due to enhanced air contact. The authors have used similar arguments at the lowest deposition layers which cooled to the temperature of subjacent snow.

AC: Cooling in the uppermost areas of the deposits has to be expected and was accounted for. Gray areas in Figure 9 indicate the areas of the snow profiles which where neglected (both on the top and at the bottom of the deposits) to account for temperature changes. Yet, we do not expect that the snow at depths of approximately more than 30cm below the surface experienced a significant cooling the relatively short time between the avalanche stopping and the measurements.

RC: P. 5806, line 24: Please explain why this shape of the temperature profile indicate plug like flow.

AC: We have used the wrong terminology. We did not mean to extrapolate the flow regime from the temperature profile but we wanted only to recall that the shape of the temperature profile resemble the one of a plug flow. This is not the correct word in this context. We will change the text accordingly and we will suggest that the constant temperature in depth may rather indicate a good mixing of snow in the avalanche core.

It is not possible to draw conclusions on the temperature profile shape at the boundary conditions (top/bottom) of profile, since cooling/warming effects from air and basal snow layer (gray areas in Fig. 9) may have corrupt the measurements.

RC: P. 5809, line 1: “Deposits from the powder cloud have consistently lower temperatures ...” Consider more careful wording, since only three avalanches were studied and it is not clear if the powder cloud and the dense core can easily be discriminated in the infrared images.

AC: As suggested we reword this statement.

Interactive comment on The Cryosphere Discuss., 8, 5793, 2014.