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

Anonymous Referee #1

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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 tem-

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Interactive Discussion

Discussion Paper

peratures 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

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. 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 implementation (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. 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?

I understand what it means to gather data from real avalanche experiments, which prevents to include 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.

2.1.2 Entrainment is very specific

“. . . 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. The conclusion is based on general knowledge of a temperature gradient in the snowpack (p. 5806, line 5ff) and the observation

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Discussion Paper

of different surface temperatures on the bed surface of two avalanches. 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.

2.1.3 Relation between flow regime and thermal signal

Throughout the manuscript the authors relate the thermal signal with different flow regimes, e.g. in Figure 7 and in 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). They 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.

2.2 Investigate the potential of the infrared camera

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

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.

2.3 Description of the methods

2.3.1 Infrared camera

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. 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?

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 georeferencing. Please provide the resolution, source and if available, error of the used DEM.

2.3.2 Temperature probes

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

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Interactive Discussion

Discussion Paper

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).

2.3.3 Calculating mass

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

3 Technical comments

Fig 1c: step down not visible.

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

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”, p 5862 line 6), or a change in the temperature scale.

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

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

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.

Fig. 8: Please enlarge the axes labels.

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

P. 5802, line 15ff: The reasonable relation between temperature and erosion depth

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Interactive Discussion

Discussion Paper

could be investigated with laser scanning data or measurements of the crown height.

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?

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

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.

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

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.

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

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