

Interactive comment on “Snowmobile Impacts on the Physical and Mechanical Properties of Different Snowpacks in Colorado, U.S.A.” by Jared T. Heath et al.

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Received and published: 1 September 2017

We want to thank the reviewers for good insight to help clarify many of the points that we were trying to make in this paper. We have changed the SWE time series to a more appropriate depth times series (as more data are now available), added a schematic of the control and treatment plots, and added a plot summarizing the data as a time series. The Introduction and Conclusions, as well as part of the Discussion have been rewritten to clarify the text. Below is how the reviewer comments were addressed.

REVIEWER 1 > The paper describes snow cover measurements to quantify the impact of snow mobile travel. Specifically, differences in density, hardness and temperature

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between an undisturbed snow cover and a snow cover subjected to various degrees of snowmobile usage are presented. The authors describe partly novel and thorough field experiments which were used to investigate these changes in detail. However, the results remain very qualitative and not very new.

We disagree that the Results are qualitative – See Figures 4 through 9, and Tables 1 and 2. We also disagree that the Results are not new. As Review 2 states, there is only one similar paper in the literature (Thumlert and Jamieson, 2015).

> Furthermore, since the goal of the study is not clearly defined in the introduction and the presentation and discussion of the results is rather poor, major revisions are required before the paper can be accepted for publication.

We disagree. At the end of the Introduction, we clearly state the purpose and then the objectives of the paper: “We examined the effect of snowmobile use on the physical and material properties of the snowpack. The objectives of this research were: (1) quantify changes to physical snowpack properties due to compaction by snowmobiles; and (2) evaluate these changes based on the amount of use, depth of snow when snowmobile use begins, and the snowfall environment where snowmobiles operate.”

>Overall, there are three main issues with the paper: 1. After reading the introduction it does not become clear why this study is needed and why changes in snow properties due to snow mobile usage should be quantified. Indeed, the first paragraph deals with the economic importance of snowmobiling. It is completely unclear how this is at all relevant to the measurements presented in this paper. The second paragraph then lists several studies before stating the goals of this study. As such, there is no clear context, no knowledge gap is identified and it remains unclear why the authors performed these measurements.

The first paragraph has been rewritten to be more succinct and use the economic and user data to set the stage for the work. Some of the specific details have been moved to an appendix. The second paragraph has also been rewritten to set the context

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and clearly state that no other papers have examined how snowmobiles influence the physical and material properties of the snowpack.

>2. The presentation of the results is rather poor and the broader relevance remains unclear. In the results section the authors show vertical density, temperature, hardness and ramm hardness profiles for all sampling dates. However, they mainly discuss mean (bulk) properties or the properties of the basal layer. As such, it would be better to show plots of the temporal evolution of the mean properties (e.g. mean density with time for the control, low use and high use) and the basal layer properties.

We have added a set of figures summarizing the temporal evolution of the mean properties.

>Furthermore, the authors essentially list the results and the writing is very dry. I would suggest that the authors use the figures and tables more actively in their writing and focus on the main results. Many parts of the text have been rewritten.

>Finally, a more in-depth analysis is required to gain new insights into the effects of snow mobile travel on changes in snow cover properties and make the results more broadly relevant. Specifically, the authors could develop a simple model (e.g. linear regression) to predict snow densification after snow mobile usage.

While this is an interesting idea, we feel that this would yield a qualitative model. As this is an interactive discussion, I am eager to hear what this could be.

> and they should investigate how snow layering affects densification. This is beyond the scope of the paper.

>3. The discussion and conclusion sections need to be rewritten. The lack of a clear objective in the introduction translates to a very scattered discussion. Vague and out of context statements are made which do not really relate to the work presented in this paper. For instance, the third paragraph of the discussion deals with snow metamorphism. Some very general statement on the influence of ground and air temperature

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are made and then related to very specific increases in density observed in the measurements (lines 332 to 334). The line of thought is very hard to follow. Similarly, there are vague statements about the transferability of the results to snow grooming (lines 306-316), minimum snow depth for skiing (lines 405-409) and snow making (lines 426-433) which seem completely out of context. The authors need to do a much better job at putting their results into context, discuss the limitations of their findings and highlight new insights.

The vague statements have been removed or significantly rewritten, as per the specific comments. With changes to the Introduction, we feel the paper is put better into context.

Specific comments: >line 33: It is unclear to me why climate change will affect the amount of land available for snowmobiling. We think that this is self-evident.

> line 36-39: How can there be old snow below a shallow snow cover? This sentence is very unclear and should be rewritten. We don't understand why this sentence is confusing. However, this sentence has been rewritten.

> line 55: remove imperial units here and throughout the paper Removed here and through, except imperial units are left in the section that discusses the initiation of snowmobile use (12" and 48") as those are the standard in the U.S.

>line 58-61: it is not clear to me why this section on conflicts among different user groups is relevant to the paper. This is setting the context for the study site. A sentence has been added to clarify this.

>line 67: The authors should describe what a SNOTEL station is and what they measure. A sentence and weblink have been added.

>line 68: “: : : was used to characterize the 2009-2010 winter on REP”. Characterize is not very specific. This sentence has been changed. The point is to show “how the 2009-2010 winter compared to other winters.”

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>line 69: it is unclear what is meant by operational sites. This only became clear after reading the results. These are “not experimentally controlled.” This has been added to the sentence.

>line 92-100: a sketch of the experimental setup would make this description more easy to follow. A figure has been added.

>line 107: remove “and continued through the duration of the winter season”. Removed.

>line 110-113: rewrite to “Vertical snow profiles were observed to record snowpack properties including snow density, temperature, stratigraphy hardness and ramm resistance.” We use the word “ram”, rather than “ramm” throughout. This sentence has been rewritten as two sentences.

>line 118: mL should be ml Either of these version are SI, so mL is maintained.

>line 118: mention the thickness of the density cutters I am not sure what the reviewer is asking for here. We measured snow density as a continuous profile of discrete 10cm measurements.

>line 119-121: remove the sentences “The density of snow : : . and bulk snowpack density were compared.” The later part of this sentence was removed and the former part was rewritten.

>line 123-125: Unclear how a mean over 10 cm can be taken if the measurements are done every 10 cm. Yes, see line 118 above.

>line 127-129: “However, repeatability for any : : :” it is unclear what the authors want to say here. This sentence was rewritten.

>line 131: unclear what is meant by “point of zero”. Do you mean the minimum temperature? This is rewritten as “the snowpack depth where the temperature gradient was linear”

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>line 141-142: remove sentence “The main crystal forms: : :.” This sentence has been rewritten.

>line 148: mention the area of the metal plate attachment. Added.

>line 156-160: ramm and not ram. We disagree. To be consistent we used “ram” throughout.

>Also, better describe how ramm measurements are made. Right now it is not clear that this is a cone penetration test. Provide a reference, e.g. Gubler (1975). Text has been added based on the following citation: American Avalanche Association: Snow, Weather and Avalanches: Observation Guidelines for Avalanche Programs in the United States (3rd ed.). Victor, ID, 104pp, 2016.

>line 162-163: “bottom stratigraphic layer” is not defined. Do you mean basal layer as defined I layer 125? If so, consistently use basal layer. Not necessarily. The bottom layer can be greater than the basal layer, which we define as the bottom 10 cm from the density and temperature measurements.

>line 171: typo “sets samples of samples” changed.

>line 173-174: clearly state what you define as significant and highly significant. Added.

>line 177-185: The definition of a deep and shallow snowpack seems rather arbitrary since the difference in snow depth is not very large. Furthermore, I would not qualify a snow cover of 150 cm as deep. We have changed Figure 2 to a plot of snow depth and chosen a different SNOTEL station that is more representative of the snowpack conditions at FEF. In Colorado a snowpack deeper than 1.5 meters is considered a deeper snowpack, and this was the assumption used in this paper. We changed the text accordingly.

>line 223 changes in temperature gradient changed

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>line 228-229: remove “favoring sintering and bonding of snow crystals” as it is not relevant here. Removed.

>line 229-231: rewrite this sentence deleted

>line 245: unclear what is meant by “the deeper snowack” This is when use starts on a deep snowpack.

>line 266: unclear what “These” refers to. Changed to “hardness.”

>line 267-268: unclear what is meant by “treated transects were approaching control values by the last sampling date” since the colored hardness profiles in bottom of figure 5c were not close to the control profile. By 17 April, hardness values were similar.

>line 269: change “orders” to “one to two orders”. Changed

>line 309-311: rewrite to clarify rewritten

>line 312: change to “on the underlying snowpack” changed

>line 322: change “also gets more dense” to “increases in density” changed

>line 325: this statement does not fit well with the temperature measurements shown in Figure 4. In particular the measurements in Figure 4b show a temperature of -4 at the base of the snow cover. It is not clear what the authors want to discuss here and this entire paragraph seems out of place. Much of this paragraph has been deleted as it is not necessary.

>line 330-331: not clear what the authors mean by “easily sinter”. Rounded grain do not sinter more readily than faceted grains, as was shown in van Herwijnen and Miller (2013). This has been deleted as it is not necessary.

>line 331-332: “Rounding increases density and snowpack strength” it is not clear what the point of this statement is. This has been deleted as it is not necessary.

>line 340: typo “snowthrough” changed

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>line 360: this is speculation since the authors did not make any observations of grain arrangements. This has been deleted

>line 362: not clear what is meant by “avalanche evaluation” This is meant to imply a simpler method. The text has been changed.

>line 370: how can the precision of the ramm penetrometer be determined?? This is based on measurements and calculated forces.

>line 371: not clear what the authors mean by “undisturbed snowpack” since the ramm penetrometer is widely used to characterize the hardness of undisturbed snowpacks throughout the world. This sentence has been rewritten.

>line 382-383: unclear how the reference to de Quervain is relevant here. This has been removed.

>line 384-387: remove this since the explanation in terms of edge effect and heat transfer from the buffer areas is very speculative and not convincing. This sentence was deleted.

>line 396: “temperature gradients and thus vapour pressure gradients were less” unclear what this statement is based on since there was no significant difference in temperature gradients and vapour pressure gradients were not measured. We can infer vapour pressure gradients from temperature gradients. While there is no significant difference, they were still less and a difference hoar crystal size was seen.

>line 397-399: this sentence is contradictory, is it similar or different? This sentence was reworded.

>line 405-409: unclear how these minimum snow depth guidelines fit in the discussion here. The last sentence has been changed. This is an implication of the findings of this work.

>line 414-415: cooler snowpack at the end of the summer? This is deleted.

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>line 418: snow depth was not less for the disturbed sites in Figure 3! This has been reworded.

>line 431: typo “create surface different conditions” This has been rewritten.

>line 432-433: It is unclear how to consider artificial snow with the present results. This paragraph has been deleted.

>line 442-444: I do not understand how the results presented in this paper can help when modelling the impact of snow grooming or snow making. This paragraph has been deleted, and replaced with one sentence mentioning snowmaking, as there could be cross-over implication. This is not explored herein.

>line 448-449: the authors did not show that the amount of snowfall influenced their results! The point is the difference between the two sites. The sentence has been reworded.

>line 453-454: this statement is incorrect since there were no significant differences between low and high snow mobile usage. This is compared to no use, as shown in Table 1.

>Figure 1: improve the caption and describe what is shown in the figure. More detail is provided.

>Figure 2: It would be better to show snow depth rather than SWE to be consistent with the other figures. Also, there is no need to show data from July to September. Finally, please show the first of each month on the x axis. This figure has been changed.

>Figure 3: it would be better to show the mean snow density with time. Also, the snow depth is sometimes larger for the disturbed sites than for the undisturbed site, which seems counterintuitive. A plot has been added.

>Figure 4: why are there vertical jumps in the temperature profiles? This is not known.

>Also, it would be better to show the mean temperature gradient with time. A plot has

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been added.

>Figure 5: The results shown in this figure are odd. It is not clear to me how and why the hardness of certain layers would decrease in the second half of the season. This is also not in line with the density measurements which show an overall increase over the course of the season. And again, it would be better to show mean hardness with time. A plot has been added.

>Figure 6: better to use a logarithmic x axis. Also, show mean ramm hardness with time. Our intention is show the differences at multiple scales. Some of this may be lost using a logarithmic axis.

Gubler, H., 1975. On the rammsonde hardness equation. IAHS Publication, 114: 110-121. van Herwijnen, A. and Miller, D.A., 2013. Experimental and numerical investigation of the sintering rate of snow. Journal of Glaciology, 59(214): 269-274.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-26>, 2017.

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