

The authors have made substantial changes to the original paper and addressed some of the concerns raised by the reviewers. While this has somewhat improved the quality of the work, the manuscript still requires major revisions to improve the presentation of the results and the discussion of their limitations and implications.

The introduction has in part been rewritten. Nevertheless, it still does not provide a clear context to explain why this study is required and why one should be interested in the influence of snowmobile use on snow properties. Does it affect the underlying vegetation, is it relevant for snow melt in the spring, does it stabilize the snow cover to reduce the avalanche danger? I suspect the last topic was what motivated the authors to perform these measurements. If so, this should clearly be stated in the introduction, and relevant studies which have investigated these effects should be discussed.

The presentation of the results has not improved much and still remains rather poor. Most figures show vertical density, temperature, hardness and ramm hardness profiles for all sampling dates. These figures are illustrative but not easy to interpret. Furthermore, the authors mainly discuss mean (bulk) properties or the properties of the basal layer. While the authors have now included a Figure showing the evolution of mean density, basal density, mean temperature gradient and mean hardness (Figure 8), this figure is only briefly mentioned at the end of the results section in a separate subsection (4.6). Furthermore, many of the results shown in this figure are repeatedly discussed before. For instance, in lines 208 to 220 the authors discuss changes in bulk density and constantly refer to Figure 4, which shows the vertical density profiles. While reading this passage, I found myself repeatedly looking at Figure 8, and it would be much more efficient and intuitive for the reader to show the plots of the mean and basal properties in each respective subsection.

Finally, the discussion and conclusion sections still need to be largely rewritten as it remains very scattered. Indeed, the authors need to do a much better job at putting their results into context, discuss the limitations of their methodologies and findings and highlight new insights. For instance, the hardness and ramm measurements have some peculiarities. In some pits specific layers sometimes have very high values which then disappear in subsequent pits. This is not observed in the control pits and highlight the difficulties in obtaining reliable hardness measurements in snow disturbed by snowmobiles. Such problems are not discussed at all by the authors even though they clearly highlight some of the limitations of this study. Similarly, the authors put a lot of weight on a 9 mm grain size measurement in one pit (section 4.5 and line 399 in the discussion) to discuss the influence of snow mobile travel on grain size. I have dug many snow pits and have looked at countless layers of depth hoar in various snow climates (from coastal to continental), and have seldom seen depth hoar crystals of that size. This particular measurement is therefore rather surprising to me and could very well be an outlier, and the authors should be more cautious with their interpretation.

Specific comments:

lines 37-38: it is not clear to me why I should be interested in changes in snow properties due to snowmobile travel. The context is missing.

lines 47-48: 'had a highly significant effect' In what way did this effect manifest itself?

line 57-58: 'land managers need to make decisions'. What kind of decisions do they need to make that this study will help improve?

line 147: 'where the temperature gradient was linear' it is not very clear what the authors mean here. The temperature gradient between two temperature measurements is always 'linear'.

line 157: 'fresh' is not an official crystal form. 'Precipitation particles' should be used.

line 168: 'for each stratigraphic layer'. I assume that for thin layers this was not possible. Please state the minimum layer thickness where these hardness measurements could be made.

line 175: what do the authors mean by 'relative hardness'?

lines 195-197: I would say that even for REP the snow depth was somewhat below average.

Section 4.1: include a figure showing the temporal evolution of the mean and basal layer density over the season (from Figure 8), as most of the discussion centers around bulk and basal layer density and not the vertical profiles.

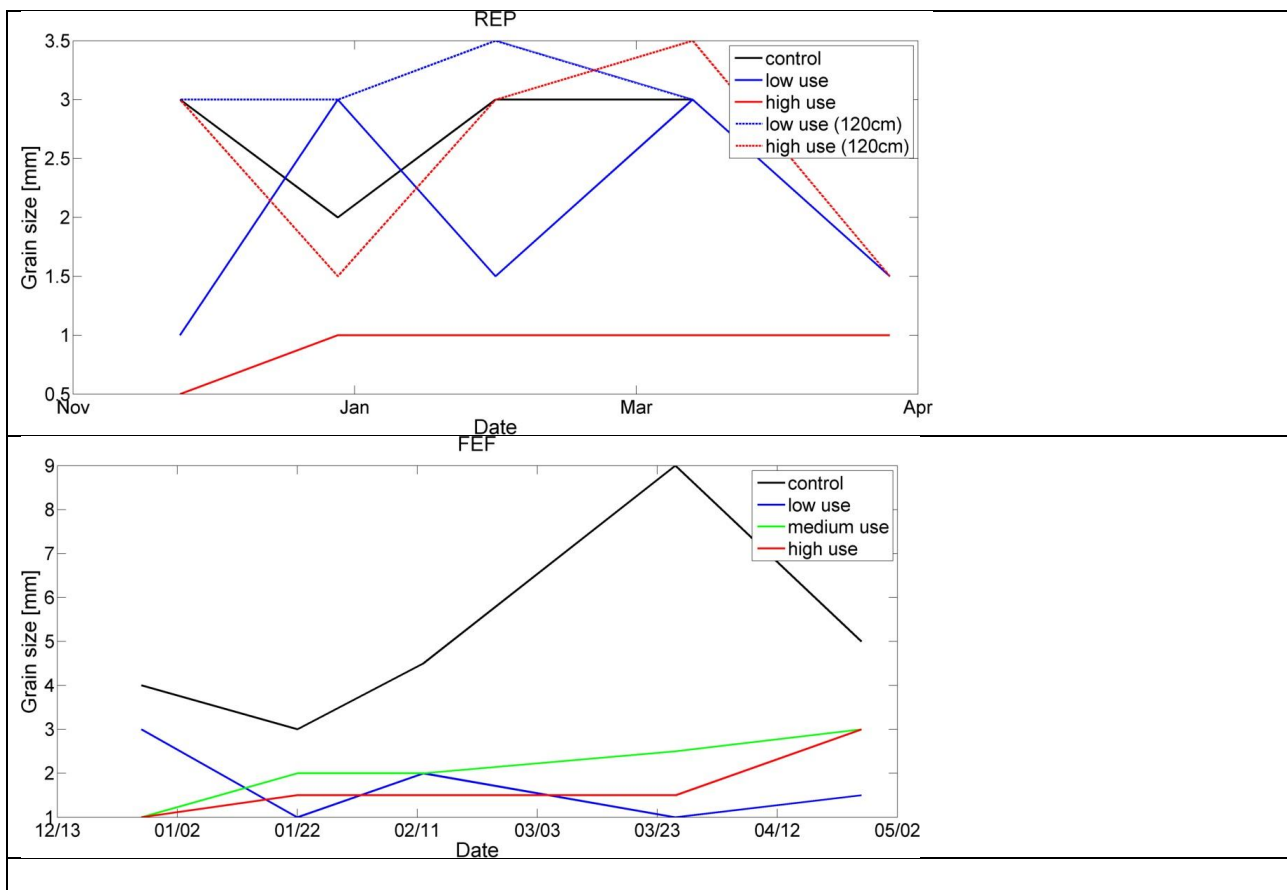
Section 4.2: include a figure showing the temporal evolution of the temperature gradient and the basal layer temperature.

line 255: 'by April 26 (Figure 5b)': this figure only shows values for 26 March.

Section 4.3: include a figure showing the temporal evolution of the mean and basal layer hardness over the season.

Section 4.4: include a figure showing the temporal evolution of the mean and basal layer ram resistance over the season.

Section 4.5: include a figure showing the temporal evolution of grain size over the season. This is much more illustrative than a table. It also more clearly shows that the 9 mm measurement is likely an outlier, and that the most marked differences in grain size were at the FEF site and for the high use site at REP (see figures below)



Section 4.5: this section seems redundant as all these results were already addressed in the sections above.

line 331: 'were similar' in what way? Describe the similarities and differences more precisely.

Section 5: The discussion requires extensive rewriting to more clearly discuss some of the limitations of the employed methodology, highlight the main findings and discuss the results in context with other studies.

lines 339-345 Here you provide a general statement on observed densification and compare it with results from another study. In lines 355-361 you again discuss the observed densification more quantitatively. Clearly, these two sections should be combined.

lines 348-352: I don't think that compacting the snow with a snowmobile alters the snow microstructure, unless you are compacting new snow. What was the snow type when you first compacted the snow in December? Also, snow hardness is predominantly determined by density, and not grain characteristics.

lines 352-354: 'such changes' unclear what this refers to. Be more specific.

lines 361-365: unclear what the point is here.

lines 373-374: I don't agree with this statement. Your results show that for the FEF site there were very little differences between the amount of use as the densification and grain size changes were similar for low, medium and high use. For the REP site, on the other hand, the differences were more pronounced. This is one of the main findings of your work which should be highlighted and discussed much more clearly.

lines 384-386: provide an explanation why the effect of snowmobile travel is less for deeper snow covers. To me, this would mean that the initial impact of snowmobile travel, when the snow cover is still very shallow, is decisive.

lines 387-388: I do not believe that compaction impeded faceting. However, the resulting faceted snow is likely stronger (better bonded). Did you observe differences in grain *type* at the base of the snow cover?

lines 390-393: it is unclear to me how less dense snow at the base of the control plots indicates that more metamorphism took place. You can still have kinetic growth in denser snow.

lines 401-403: 'results may be transferable': what results do you mean?

lines 404-407: I do not follow your reasoning here. The results clearly show that there was no significant difference in temperature gradient. You can therefore not conclude that the vapor pressure gradients and depth hoar growth was slower since you did not measure those. All you can say is that the densification at the start led to a decrease in grain size throughout the season.

lines 408-424: The point of this section is not clear to me. Suggest rewriting.

lines 425-431: This is the first time where a context for the measurements is given. This should also be mentioned in the introduction, as this seems the main reason why these measurements were performed.

lines 440-453: this last section does not seem very relevant to me.

Section 6: The conclusions have to be rewritten to better highlight the main findings and their implications.