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Interactive comment on "Snow farming: Conserving snow over the summer season" *by* Thomas Grünewald et al.

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We are grateful to S.R. Fassnacht for his very positive and constructive review. We are answering his comments in the following, for clarity we repeat the original comment (C) and answer (A) afterwards:

General Comments

C: This is a somewhat novel idea and I applaud the authors for using a modeling approach with field data to assess the utility of snow farming. There are no major problems with this paper and with some clarification, it will make a good contribution. A: Thank you for this positive feedback.

C: The differences in ablation between the two sites is attributed to the "potential warm-

ing effect of the black paved road at Martell resulting in lateral advection of heat (page 21 line 1)." Can this be quantified at all? This is an important point. A: Together with the good suggestion to do full three-dimensional (Alpine3D) simulations also for the snow heap, it will be interesting to see (probably in a future study), in how far the road may contribute to stronger melt. However, it will not be adequate to present a calculation of the effect in the current paper, which presents a simplified analysis based on one-point simulations at the top of the pile. A "back of the envelope calculation", which could be done based on an assumed road temperature could be done but is considered not to be very useful as we don't have time-resolved estimates of surface temperatures for both the side of the pile and the road. Thus, at least rudimentary radiative transfer modelling in combination with local energy balance modelling would be required to quantitatively estimate the effect. For this, we do not have sufficient input data and it is beyond the scope of the analysis as presented in this paper.

C: The writing is good, with a few instances of paragraphs that seem to short. There are a few words uses that are somewhat subjective, such as "huge" on page 20 line 31. These can be distracting. The figures are good, but could be slightly improved, such as adding section letters (e.g., Figure 11, use a. depth of covering layer) and increase the font size on axes. A: We are carefully revising these points and implementing changes where required (see detailed comments)

Specific Comments

- page 1, line 11: "a factor of 12" instead off "or" A: changed

- page 1, line 15: this sentence is confusing "switching of precipitation of completely would strongly increase melt" A: Sentence was changed to "No significant effect of additional precipitation could be found as the sawdust remained wet during the entire summer, already with the measured quantity of rain. Setting precipitation amounts to zero, however, strongly increased melt."

- page 1, lines 17-21: another citation could be the pozo de nieve (snow wells) that

were extensively used in Spain well into the 19th century A: Thank you for this hint, we have added a citation (Morley 1942)

- p2, l 2-5: While it is an old citation, it is an interesting approach to reduce mass loss of small glaciers/snowfield due to sublimation - Slaughter (1970 US Army CRREL Special Report 130) A: It is indeed an interesting paper. As it is, however, not directly linked to snowfarming we believe that it is not meaningful to discuss in the context of our study.

- p3, l 27: it may be intuitive to you, but add direction to the location, i.e., lat: 46.808°N, long: 9.868°E (I assume). A: We have added directions for both study sites.

- p3, l 29-30: could you simulate how different natural snow in piles would be? "A large snow pile is formed by machine made snow produced during the winter months." A: This would be possible by changing the initial properties of the snow heap in the input file. However, differences between well settled, aged natural and technical snow are marginal and simulation results with old natural snow would therefore not reveal significant differences.

- p4, l 3-5 and 12-13: did you compare the met station on top of the building to the met station on top of the snow heap? A: Yes we did for TA and VW and developed a correction for VW. This is described in Sect. 3.1

- p4, l 4-7: you use new symbols that do not seem common – air temperature (TA), relative humidity (RH), wind speed (VW), direction (DW), incoming shortwave radiation (ISWR), incoming longwave radiation (ILWR). Are this necessary, or can you use more common symbols? A: We changed symbols for wind speed (WS) and wind direction (WD).

p5, l 14: you "calculate snow volumes." What about mass? Snow mass or SWE cannot be directly calculated from TLS measurements. It also requires snow density. Only few density measurements were available, it would therefor add uncertainty to results to use SWE instead of HS (which was measured highly accurate). We therefore focus

on volume for the measurement but also address SWE when analysing model results (also see reply to review by J. Garvelmann).

p5, l 17: state the wavelength of the TLS "near-infrared spectral range" A: the wavelength is 1064 we added it in brackets.

p7, l 10: is the word "extremely" necessary? A: removed

p7, l 19: do you mean "crown" instead of "crone?" A: yes that's right.

p7, l 19: be specific about the type of "linear interpolation" A: We changed to ". . .by triangulation with the nearest points".

p7, l 31: the "grain size of 1mm" seems quite small A: This should be "grain radius" instead of grain size. 2mm is a typical grain size for well settled technical snow.

Table 2: is a spectral albedo of sawdust of 0.5% correct? This seems low, or explain what this is. A: yes, this is a mistake. It should be 50%ÂÍ

p8, l 13-14: data were "resampled to 30 min time steps" but the "modeling time step was set to 15 min." Please rectify or discuss this discrepancy A: We changed to "All input data were filtered, quality checked and resampled to the modeling time step of 15 min using the meteorological input-output library MeteoIO"

p8, l 18 and 21: should this read "Table 3-5", instead of Table 3.5? A: it should be Table 3.

p9, l 15: change "Lower temperatures and irradiation is mainly A: changed

Figure 3c: maybe use two axes the same as Figure 3a and b, with net SWR in red on the left and net LWR in blue on the right. A: I think that the figure is quite clear as it is. I see no reason for adding a second axes with the same units and dimensions.

Figures 3 and 4: use Oct rather than Okt. Think about putting these two sets of figure beside one Another A: labelling has been changed.

p11, l 3-4: delete the first two sentences: “This section presents results obtained from the TLS surveys. The focus of the analysis is on the Flüela data set. Values for Martell are provided in brackets.” A: Sentences have been removed.

p11, l 5 and beyond: use a period as the decimal place “8.99m” rather than a comma “8,99m” A: changed

Figure 5: consider changing the color ramp so that white is no change, blue is an addition and red is a less. At present it is confusing as blue can be a small gain or loss. A: We changed the colour map according to the suggestion.

Figure 5: there is a scale bar. Think about adding dimensions (in x and y) to one of the figures instead so we know how big it is. A: We removed the scalebar and indicate scale in the x-axes now

p13, l 12: perhaps show this “respective coordinate” on Figure 5a and 6a A: We are showing the coordinate in Fig 5c, 5d, 6c and 6d now

Figure 7: do you mean “aspect” for “exposition?” The x-axis for Figures 7b and 7d are unclear A: yes it should be aspect. We changed it and reworked the axis.

p16, l 4: what is meant by “in dependence of the different settings?”?” Also, add a number to “(Table)” A: Deleted. See below.

p16, l 4-6: I would delete these sentences. They are not necessary. What is meant by “The difference pictures densification?” A: Deleted. It was meant to explain the difference in relative losses in SWE and HS; this is attributed to an additional effect of densification on HS (losses in HS are larger than in SWE).

Figure 10d. I am surprised that precipitation does not appear to change the results at al. A: The reason is that the cover remains wet even for the case of no additional rain. Additional wetness seems to have not much additional cooling effect. This is described in the text.

Table 5: how is the albedo of the snow heap modelled over time, as this influence net SWR.

A: Albedo of saw dust albedo is modelled with a constant value of 0.5; We cannot quantify temporal change of saw dust albedo but is probably limited over a single summer. Albedo of snow (only relevant for simulation without sawdust cover) chages in time dependent of snow properties as described in Schmucki et al. 2014

Figure 11: would this be clearer is there were log scales (both positive and negative), as some of the bars are difficult to see. A: We have tested this suggestion. As expected, log scale improves readability of the small fluxes. However, the large difference in contribution of the single fluxes to melt is then less pronounced. We think that emphasising this relative difference is more important than readability of the smaller, relatively unimportant fluxes and therefore remain with a linear scale.

p20, l 9-10: can you quantify “total mass balance can be rated as marginal?” A: yes of course; let’s assume a total size of the gaps of 10m2 (which is large) and a mean error introduced by interpolation of 0.5 m (which is also large). This results in a volume uncertainty of 5m3; relating this to snow volume of the entire heap (5000-7000 m3) is below 0.1%

p20, l 31: is the word “huge’ necessary? A: removed

p20, l 31-31: “Possible explanations are different properties of the covering materials.” I assume that you did not model Martell? Can you do some simple calculations to describe the differences between saw dust and wood chips. While you subsequently say that it is likely not important, we don’t know this.

A: We also modelled Martell, but with the same properties of the covering layer. We believe that the general properties of saw dust and the mixture of saw dust and wood chips in Martell are similar. To test this we did some modelling (see our reply to J. Garvelmanns review below). However, the large heterogeneity of the surface, espe-

cially in Martell, with very dark areas consisting of older cover material and with areas of fresh, brighter material might well have effects, e.g. on albedo.

Reply to J. Garvelmann: From our investigations we think that the difference between the materials is much smaller than the uncertainty in the estimations of these properties and the very small-scale spatial variability of the cover material (which is especially large for the mixture of chipped wood and saw dust). To test the effect of porosity (and therefore water storing capacity) we performed some model runs with varying grain sizes of the covering layer. Increasing the grain radius (from 0.1 mm to 1 mm) by a factor of 10 did not reveal any significant differences in the final mass loss. Only much larger grain sizes (3 mm) increased mass loss slightly. Wood chips of that size (or even bigger) exist in the Martell covering Material, but the finer particles clearly dominated. Moreover, from laboratory measurements of small samples from both heaps we found nearly identical dry densities. We therefore believe that the assumption of same properties of the covering material is appropriate. We include a corresponding sentence in the text.

p21, l 1: can you try to quantify the “potential warming effect of the black paved road at Martell resulting in lateral advection of heat?” A: See detailed explanation above

p21, l 14: delete “such” A: done

p22, l 28: I think you mean “proved” rather than “proofed.” I think that this word is too strong as you only modeled one point at the top of the pile. A: we changed to “proved”

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