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Interactive comment on "Local reduction of decadal glacier thickness loss through mass balance management in ski resorts" *by* A. Fischer et al.

Anonymous Referee #2

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General Comments

The paper presents a valuable, comprehensive and comprehensible overview about the medium-term (decadal) effect of technical modifications of the glacier surface mass balance within Austrian Skiing resorts. The application of these measures started around the year 2004 and the related physical processes and short-term effects were already investigated in detail in a number of earlier studies. The authors analyze digital elevation model differences as well as DGPS measurements at selected spots of different glaciers with and without application of such measures between multiple years in order to quantify the effect of these intentional modifications on surface elevation changes within this timescale. Results indicate the clear medium-term benefit as well

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as the limitations of these technical measures on a larger scale in terms of costs and efforts. Although the uncertainty of their method is discussed in the manuscript, the latter should be done in a more thorough, quantitative way, thereby also using an appropriate and exact terminology. In a revised version of the manuscript, the individual uncertainty sources should not only be named but all of them also be estimated and the resultant combined expanded uncertainty as well as its impact on the main results of the paper calculated. Therefore I suggest accepting the paper after the points listed in the specific comments and some minor ones in the technical corrections have been implemented by the authors.

Specific Comments (in decreasing order of importance)

(1) In the discussion section (p10. Lines 5-15) the Authors indicate a maximum uncertainty for their method of 1.1 m for both the DGPS and the DEM differences. It is not clear a) how this number is calculated exactly (uncertainty components), b) what confidence interval it is referred to (e.g. standard (66%) or expanded (95% level) uncertainty), c) what the impact of the combined expanded uncertainty is on the main results of the paper. For clarity and consistency, I very much encourage the Authors to study and use the Guide to the Expression of Uncertainty in Measurement (GUM; JCGM, 2008)) as well as the terminology that is defined therein.

(2) It is not clear how areas with long-term mass balance management were exactly identified (onsite location) in the study (own (GPS) records or data from skiing resorts?,. . .). Please add this information.

(3) Concerning the single effect of grooming on snow and ice ablation, the authors should add that the observed effect was in the order of only 5 % rather than 10% and that this number was very close to the measurement uncertainty (Olefs and Fischer, 2008; Fischer et al., 2011; ;Olefs, 2005;Olefs and Obleitner, 2007). It is also worth to clarify the following in the paper: Based on previous studies, it is still not clear what exact physical mechanism(s) leads to the observed effect. Beside the reduction of

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surface layer erodibility through compaction (stronger bonding of the snow crystals), there may be other effects, e.g. a modification of surface albedo due to a reduction of average grain size of the surface snow layers induced by the snow-cat or a modification of snow thermal conductivity (Olefs and Obleitner, 2007). If there are new studies that separate those exact effects on the ablation reduction known to the authors, they should cite them.

(4) I strongly suggest adding units (SI) to all variables whenever formulas or variables are used in the manuscript (e.g. p.5).

(5) The physical effect of water injection in the snow cover is mainly to add mass to the existing seasonal snow (if there is enough cold content in the snow to refreeze the injected water). After injection, the release of latent heat due to refreezing of the water decreases the absolute value of the cold content of the existing snow cover (as e.g. shown in Fig.7 of Olefs and Fischer, 2008). Firstly, I do not understand why the cold content should be increased by this method (p.3, line 19). Secondly, the authors could also add the two main resulting limitations of this method apart from the large effort: enough cold content before injection and timing problem (enough time between applications).

Technical corrections

p1 (1) l30: "...depth height...??" (2) l4: Fischer et al., 2011 a or b? p2 (3) l6: please explain the first occurrence of the shortcut "GI" (4) l9: 1987 in the manuscript, 1986 in Tab.1 ?? (5) l13: They noticed... (6) l18 and others: I would prefer "t-bar lift" instead of "tow lift" throughout the paper p3 (7) l19: increase or decrease cold content? you could use the absolute value to clarify... p4 (8) l19: please use consistent naming for "Austrian glacier inventory" (GI?) p5 (9) please add units to all variables (SI) p6 (10) For DGPS (?) profiles... P7 (11) L20: here the single effect of grooming (compaction of the surface layers) is mixed with the potential effect of snow farming (lateral transport of snow mass by snow cats), please clarify. (12) L27: (Tab.3) P8 (13) L29-30: "On

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average" instead of "In mean" P12 (14) l5:. . .ski tourism in the year 2100. . . (15) L12: "Fujita and Ageta, 2000" is missing in the references (16) L16: I would suggest to write "(e.g. without glacier cover)" as a) in other regions of the world glaciers do exist at low altitudes and b) the fact that the effectiveness of surface textiles to reduce ablation decreases with altitude is not tied to the surface type (glacier or not) but it is due to the energy balance being dominated by sensible/latent heat fluxes at lower altitudes. (17) L18: at the end of this sentence you could again cite the work of Skogsberg as well as Grünewald and Wolfsperger).

References

P15, l21: The year of publication should be placed at the end.

Figures

Fig.1: In the caption please specify whether DGPS measurements are indicated by the red lines. Fig.3 and following: it is not clear what you mean with "surface elevation changes plotted for surface elevation in 2007" ? Do you mean the difference 2007 – 1999 and 2015-2007 ? Please clarify in the captions and also in the ordinate label. Fig3.: It shoud be 25th /75th percentile (and not %!)

The captions of all following figures could be reduced. . .there is a lot of redundant information. Fig.13 and 18: on the right subplot "mbm" and "ref" is missing as label

References

Joint Committee for Guides in Measurements (JCGM): Evaluation of measurement data – Guide to expression of uncertainty in measurement, JCGM 100:2008, GUM 1995 with minor corrections, available at: http://www.bipm.org/utils/common/documents/jcgm/JCGM_100_2008_E.pdf, 2008.

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