

Response to the anonymous referee #2

#### 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 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 Interactive the resultant combined expanded uncertainty as well as its impact on the main results comment of the paper calculated.

We restructured the manuscript and added thorough calculation of measurement uncertainties as well as a better description of other uncertainties.

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.

The discussion of errors and uncertainties was shifted to the section on Data and methods and expanded, discussing systematic and measurement errors separately.

- (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.

The criteria for the selection of the test sites are now part of the paragraph on test sites. The locations have been clearly indicated by previous (own) GPS records and documents of the pre-projects. Of course only own DGPS measurements are presented here.

(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).

Done – we changed the number to the original 6% and added the information that this is close to the measurement accuracy.

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 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.

We changed the wording to distinguish measurements and unknowns.

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

The formula is replaced.

(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).

We agree with that points (-cold content can increase only if small amounts of water are injected and cold air can penetrate the snow cover through the holes for a longer time; no skiing and no grooming and no snow falls after the injection). As injection is not used as mass balance management method, but mainly for ski racing, we decided for shortening that part rather than going to deep into that topic.

#### Technical corrections

p1 (1) I30: “:::depth height:::??”

height removed

(2) l4: Fischer et al., 2011 a or b?

a, J Glac

p2 (3) l6: please explain the first occurrence of the shortcut "GI" (4)

done

l9: 1987 in the manuscript, 1986 in Tab.1 ??

1986

(5) l13: They noticed:::

Section rephrased

(6) l18 and others: I would prefer "t-bar lift" instead of "tow lift" throughout the paper

replaced

p3 (7) l19: increase or decrease cold content? you could use the absolute value to clarify:::

Water injection is not discussed any more

p4 (8) l19: please use consistent naming for "Austrian glacier inventory" (GI?)

ok

p5 (9) please add units to all variables (SI)

The section is removed

p6 (10) For DGPS (?) profiles:::

The section is removed

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.

The section was rewritten. In any case, we can not separate the effects of various measures in this spatially distributed study.

(12) L27: (Tab.3)

changed

P8 (13) L29-30: "On average" instead of "In mean"

changed

p12 (14) l5:::ski tourism in the year 2100::.

changed

(15) L12: "Fujita and Ageta, 2000" is missing in the references

added

(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 interactive energy balance being dominated by sensible/latent heat fluxes at lower altitudes.

changed

(17) L18: at the end of this sentence you could again cite the work of Skogsberg as well as Grünwald and Wolfsperger).

done

#### References

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

Done

## Figures

Fig.1: In the caption please specify whether DGPS measurements are indicated by the red lines.

Not all the red lines show DGPS data, in case recent LiDAR DEMs have been available, these have been analyzed. The red lines thus denote survey profiles (either DGPS/DEM or DEM/DEM comparison). This note was added to the caption.

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.

The Figures have been shifted to the supplement. Y axis has not been changed for the various surveys, every point is displayed with its elevation in the year 2007. Otherwise, if every point had been displayed with the surface elevation during the surveys, we would compare different locations.

Fig3.: It should be 25th /75th percentile (and not %!)

Changed

The captions of all following figures could be reduced:::there is a lot of redundant information.

As this is part of the supplementary material now, we decided to keep the full caption, as the main manuscript is much shorter and straight forward.

Fig.13 and 18: on the right subplot "mbm" and "ref" is missing as label

The indices "mbm" and "ref" are added to the corresponding profiles (Figures shifted to supplementary material and numbers changed).

**Formatiert:** Schriftart: Nicht Kursiv, Nicht unterstrichen

## References

Joint Committee for Guides in Measurements (JCGM): Evaluation of measurement data – Guide to expression of uncertainty in measurement,

**Kommentar [MST1]:** Wurde das hinzugefügt?

JCGM 100:2008, GUM 1995 with minor corrections, available at:

[http://www.bipm.org/utis/common/documents/jcgm/JCGM\\_100\\_2008\\_E.pdf](http://www.bipm.org/utis/common/documents/jcgm/JCGM_100_2008_E.pdf), 2008.