The Cryosphere Discuss.,
doi:10.5194/tc-2016-194-RC1, 2016

The Cryosphere Discussions
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Interactive comment on "Seasonal evolution of a ski slope under natural and artificial snow: detailed observations and modelling" *by* Pierre Spandre et al.

Anonymous Referee #1

Received and published: 13 October 2016

General Comments

The authors present a quantitative assessment of the water mass that is lost during production of man-made (MM) snow within a French skiing area using two distinct methods: (i) differential GPS surveys together with snow coring on freshly produced MM snow piles and on a section of a skiing slope and (ii) physically based snow modeling adapted to skiing slope conditions (e.g. accounting for grooming) with and without production of MM snow. The authors conclude that under (the observed) ideal production conditions around 40 % of the water used for MM snow is lost during the production of which 10 % are attributed to thermodynamic effects and 30 % to mechanical and/or technical effects (wind, settings of the snowguns). The quantification of the water mass

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lost during MM snow production is not only important for the skiing industry itself (optimizing water and energy costs) but also to raise general awareness for a more responsible use of water and energy resources. For the scientific community this study is highly valuable for two reasons: (i) To my knowledge it is only the second quantitative study after the work of Eisel (1988) which was based on a now outdated technology of snow production, (ii) It allows to correctly quantify the effect of MM snow production in assessments of historical (observed) or future (projected) snow conditions within skiing resorts (related to climate change) by giving the "real" amount of artificial snow present on the slopes instead of an estimated potential production. So far, such loss effects have been very broadly assumed.

In general the manuscript is well structured but partly poorly written and suffers from too much detailed information at some places (please see my detailed comments below) which distracts from the important findings. I congratulate the authors for the serious and detailed treatment of uncertainty concerning their measurement methods and results, which is still too rarely done in geosciences. But I also strongly recommend to use a more rigorous, exact and commonly defined uncertainty terminology (see my detailed comments below). Regarding the role of the thermodynamic effect, the authors should add more details on the method of Eisel (1988) used to estimate sublimation/evaporation of the MM snow and discuss the related uncertainty with regard to the final conclusions of the paper.

Finally, I very much encourage the authors to continue the work in future by investigating in more detail what the influence of some of the questioned points on the total mass loss is (e.g. technical setting of the snow gun ("quality parameter"), detailed thermodynamic effects). Such could e.g. be done by operating a snow gun in a more controlled environment (e.g. parking lot or indoors). This would be very valuable for the scientific community because chances are that you could derive parameterizations that would be transferable in space to be directly used in other related studies.

Therefore I suggest accepting the paper after the points listed in the specific comments

and some minor ones in the technical corrections have been properly addressed by the authors.

Specific Comments (in decreasing order of importance)

(1) Please improve general readability (e.g. English language editor service)

(2) Uncertainty methodology: For clarity and consistency in the scientific community, I very much encourage the Authors to study, use and apply the Guide to the Expression of Uncertainty in Measurement (GUM; JCGM, 2008)) as well as the terminology that is defined therein. In detail it is not clear to the reader what confidence interval the given (combined) uncertainty values you are referring to, what coverage factor you are using etc. (e.g. standard (66% level) vs. expanded (e.g. 95% level) uncertainty).

(3) The authors should provide more details about the Eisel (1988) methodology they use to calculate sublimation/evaporation in Sect. 4.1 and include an estimation of the related uncertainty, also in relation to the main conclusions. Given that this is the main source of potential mass loss beside mechanical effects, this component should be treated with more care in the manuscript!

(4) To my mind, the manuscript would very much profit from being more concise at some places:

a. Sect. 2.2.3 (Evaluation of related uncertainties on snow depth) should be strongly shortened. I would suggest to move everything from p6 to p8 (including) to an appendix or additional material (also Fig. 2a,b and 3 and Table 2). The essential information is written on p9.

b. Most of the information of Sect.3.3.2 (Water recovery rate from observations of the ski slope) could be put in a table, this would be much clearer.

c. The abstract and the conclusions could be shortened with the same essential information content.

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(5) It would be interesting to have some photos of the produced and measured snow piles and the snow guns and where the produced snow is found (toe/back of the snowgun,. . .) as e.g. additional material.

(6) More technical details about the snow guns used for MM snow production would be helpful as well (e.g. air-water gun or fan gun) and an estimation whether your results are applicable for other types of snow guns. In order of appearance

(7) P 4 L 21-22: "We expected. . .." I do not understand this sentence.

(8) P10: please add more details about the weighting method to measure snow density on MM snow piles and the related uncertainty.

(9) P11 L4: it is not clear to me how the uncertainty on the density is derived.

(10) P12 L20: please describe the snow gun sensor (type of sensor, accuracy,. . .)

(11) P15: The "quality" parameter should be clearer introduced and explained in the methods section.

(12) P21 L8: what about the wind erosion of already deposited MM snow? could this be an additional source of mass loss ? If not, please explain here why not. And what about melting? If you can exclude it add it here for completeness as well and argue why.

Technical corrections (general)

Whenever referring to a date in your manuscript it should read "on 4 December" and NOT "on the 4 December". . . I would prefer "equation" instead of "relation".

Technical corrections (in order of appearance)

P1 L20: is snow a "material"?

P1 L20-21: ". . .encouraging ski lifts operators to an increasing amount of technical methods of snow.. "

P2 L1: “ski resort stakeholders”

P2 L6: “. . .Machine Made (MM) snow mass on ski slopes. . .”

P2 L11: “They found an average of 6% of water loss. . .”

P2 L15: “. . .a maximum of 4 m^3. . .”

P2 L16: “. . .and with a MM snow technology”. Abbreviation MM was already introduced.

P2 L25: “. . .the efficiency of MM snow production. . .”

P2 L29-30: Please clarify what you mean by “dedicated sessions” as this terminus is used several times afterwards.

P4 L6: “(wet-bulb temperature of the air and wind conditions)”

P6 L23: “. . .interpolated GPS points with the TLS points. . .”

P6 L31: There is something wrong with “. . .determined help to a total station”

P7 L2: “. . . An average elevation difference of -0.0012 m was measured. . .”

P13 L8: I would prefer to call it a “threshold wet-bulb temperature for snowmaking”

P16 L12-17: I would replace “on the natural/groomed snowpack” with e.g. “concerning the natural/groomed snowpack. . .”

P22 L8-9: “. . .not be possible to be completely assessed due to complex. . .”

Figures and Tables

Figure 2. The light brown vertical lines are hardly visible

Figure 4a. Think of adding more color to the colormap, values > 0.2 m are not visible, the position of the snow gun is not visible.

Figure 4b. Please use SI units (left ordinate), the average snow depths (x) are hardly

visible.

Figure 5. The position of the snowgun is hardly visible. Caption: add the information what area this data shows (skiing slope!?).

References

Eisel, L. M., Mills, K. D., and Leaf, C. F.: Estimated consumptive loss from man made snow, JAWRA Journal of the American Water Resources Association, 24, 815–820, doi:10.1111/j.1752-1688.1988.tb00932.x, 1988

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