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The Cryosphere Discussions

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

Anonymous Referee #1

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General comments :

This paper deals with the impact of mass balance management on glacier thickness changes in ski resorts. From photogrammetry, laser scanning and GPS measurements, the authors compared the thickness changes on profiles with and without mass balance measurements over the last 20 years. The authors conclude that thickness changes could be reduced by 35-65% thanks to the mass balance management. This paper shows a large dataset given that 16 profiles on 5 glaciers have been measured since 1997 or 1999. These comparisons are rare on the alpine glaciers and these results certainly deserve to be published. It does not concern the scientific community only but also many people involved in the mass balance management.

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Discussion paper

However, this manuscript has large weaknesses and did not reach a sufficient maturity. This manuscript is difficult to read and confusing.

First, the authors should revise the structure: - Data: a lot of information should be included in Data and not elsewhere in the paper: for instance, the information related to the uncertainties on photogrammetry, GPS.., measurements given in Discussion (lines 5-15 p 10) should be reported in Data section. The authors should check that, everywhere in the manuscript. Seven lines in “Surface elevation data” are not sufficient to describe the measurements given these data are the basis of the paper. The authors should explain here clearly that DGPS measurements of 2014/2015 are compared to DEM from 1997/1999 and 2006/2007. It is not obvious at this stage of the mansucript. - The techniques of management on each glaciers should be summarized in a Table (maybe in the Table 2). - In Data and Methods section, the explanations about the emergence velocities (p.5, lines 1-20) should be removed from Data and Methods: first, the authors do not provide any explanations here why and how they used these equations. At this stage, the reader wonders why the authors introduce these Equations relative to the emergence/submergence velocities. These equations should be moved to the Discussion (lines 16-29, p10) where the authors provide a discussion about the relationship between the surface mass balance and the elevation changes. However, I am not sure these Equations are helpful given the authors do not use them. In any case, the authors should use the classical way to present the equation related to emergence velocity (Cuffey and Paterson, 2010, equation 8.65). Equations 2 and 3 are not necessary in any case, given these equations are not used for calculations in this paper. - Study sites: the authors should replace the long (and indigestible) description by a Table. - Results: this section is indigestible. The reader does not need the full and detailed description of elevation changes at each pylons, skilifts, pistes. . .. Here, the description seems to come directly from a technical report. It is not useful for the scientific community. The number of Figures which show the elevation changes (Fig 3, 4, 56,7,8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20, 21, 22, 23,24 and 25 !!) should be considerably reduced and most of them should be moved in a supplementary material. For this section, the authors should make a strong effort to sum up the results, to analyze them and to make a new Figure to show the summarized results. From my point of view, it is absolutely necessary and this kind of Figure would be useful for the scientific community.

Second, the analysis of results is poor. I am aware of the difficulties given that the data come from different techniques and different areas. In this way, it is very difficult to compare elevation changes for areas with different altitudes and different aspect. However, despite on Table 3, there is a lack of quantitative results. I think that Table 3 is not sufficient to analyze the results. In addition, I am not sure that results given in relative reduction % are relevant. Some figures in Table 3 seem to me strange or wrong: for instance, at ST2, the authors reported a relative reduction of -396% despite on the fact that the elevation change is +0.2 m for the reference profile and -0.7 m for the profile with mass balance management. Did I miss something? If not, the authors should check the whole results. Again, I do not think the relative reduction in % is meaningfull. When the value of elevation changes are close to zero (close to ELA), the relative reduction can reach very large values but it does not mean that the impact is more important. This way of presenting the results is not convincing. I believe that the percentages given in the manuscript (and in Abstract) are easy to understand for the general public but are probably no relevant.

Third, I am not convinced by the conclusions relative to the impacts of mbm. For instance, the authors claimed that "the submergence and emergence should be similar…so that a large impact resulting from different or changing ice flow regimes is unlikely". It can be questioned from the results shown in this study. For instance, it seems very difficult to make conclusions about the impact of mass balance management when the measurements have been done at very different altitudes (Fig. 3, Fig 5, Fig. 7….) and for different aspects. Moreover, I do not understand how the submergence/emergence velocities spatial distribution can be neglected in this study. The authors wrote that "Interannual differences in emergence/submergence velocities are

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less than 0.5 m a-1 at Kesselwandferner", but, here, this is the spatial distribution of emergence velocities which is questioned. Or I missed something. The results shown in Fig 3 to 25 are confusing and again, a thorough analysis and a synthesis are missing to provide relevant results and to convince the reader.

Detailed comments:

Many things should be improved but I think it is not necessary to make a list at this stage given that the structure of the manuscript and the analysis of the results should be strongly revised first. Detail information should be removed from the manuscript when there are not used in the manuscript (GPR data, history of ski tourism…). The authors should check that carefully.

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