Response to 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.*

*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: -*

We restructured the manuscript as suggested by the reviewer, extended the descriptions of the data, added more details on measurement accuracies and added a more detailed description of the method.

*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).*

done

*-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.*

The equations of Cogley et al. were replaced by the suggested equation in Cuffey and Paterson. As suggested by reviewer #1, this part was shifted to the discussion.

*-Study sites: the authors should replace the long (and indigestible) description by a Table.*

The Table was improved. The detailed description went to the supplement and were described by a shorter and more illustrative descriptions of the sites.

*-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.*

This part was shifted to the supplement.

*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.*

Done

*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.*

We now present three figures summarizing the results of all sites and all periods. Only one figure presents the types of measurements was kept as an example.

*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.*

We revised the presentation of the results to give a better overview by adding mean elevations of the test sites, by calculating annual values with the respective error bars and by separating sites with higher and lower thickness losses.

*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.*

We removed the relative reduction from the Table.

*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.*

The mass balance management mainly influenced surface elevation changes at this location in period 2. We tried to clarify this point in the introduction: during the first period (~1997 to 2006), mass balance management was applied only after 2003. During the second period, mass balance management was applied continuously. This explains why the area mbm in period 1 shows higher thickness losses than the reference area, but lower ones in the period 2. The explanation was in the text, but very well hidden – we apologize and hope that the explanation in the introduction makes it easier to follow the interpretation. We only interpret the differences between period 1 and 2, and the differences between mbm and ref areas in period 2. We removed the relative numbers, as dividing by zero results in odd values.

*Did I miss something? If not, the authors should check the whole results. Again, I do not think the relative reduction in % is meaningfull.*

We removed that.

*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.*

We replaced them by absolute numbers and added mean values of thickness change by Abermann et al 2010 for comparison.

*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.*

Here we have a small misunderstanding, which we hoped to improve by rephrasing the text. We have to assume that submergence and emergence at mbm and ref profiles is similar and not changing too much with time. Otherwise, one could argue that the investigated thickness changes are not resulting from the mass balance management, but from ice flow dynamics. We now show in the study that the shape of the reduced thickness changes exactly fits to the covers for Schaufelferner. We think that this is a good indication that these sharp and rectangular bumps are not caused by changes in ice dynamics.

*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.*

Our approach is to compare areas with mass balance management to areas without mass balance management at various altitudes. High elevations show low thickness changes, low elevations high thickness without mass balance management. With mass balance management, thickness loss is reduced in all elevations, but at different rates. We agree that modeling the effects of mass balance management would be difficult, as the full energy balance is needed and the course of ablation and accumulation during season can result in huge differences. Nevertheless, we think that this is a further step, but not the aim of this study: We wanted to show that there are effects by a relative comparison.

*Moreover, I do not understand how the submergence/emergence velocities spatial distribution can be neglected in this study.*

We investigate thickness changes, not mass balance, as this is the parameter which ski resorts are most sensitive or vulnerable. Horizontal flow velocities on Austrian glaciers are a few meters/year only, so that the differences in vertical flow velocities between the first and the second period and mbm and ref profiles located only few meters apart should be small. At least there is no known proof of rapid velocity changes between the first and the second period, and no indications of extremely changing flux divergence at the profiles. This should be evident from cracks and crevasses. In addition to that, pylons mounted at the glacier surface would have to be repositioned in case of such an event, leading to an official report on that event.

A reduction of ice flow velocity at the glacier tongues lead to increased thickness loss even at constant melt rates. A partially reduction of thickness loss in mbm areas at glacier tongues caused by changes in ice flow would be related to increasing flow velocities, which is not observed.

*The authors wrote that "Interannual differences in emergence/submergence velocities are 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.*

We need booth assumptions for our study. For Kesselwandferner, we actually measured emergence velocity of stakes separated only a few meters. The differences in emergence or submergence are small, unless ice flow velocities are ~100 m/year and the stake is located in a crevasse zone. This type of motion is clearly indicated by surface features as crevasses, and therefore we can exclude that. We did not include that in the discussion, as this topic is far from the main focus of the paper.

*The results shown TCD 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.*

We added the summary Figures and shifted the raw data figures to the supplement.

*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.*

We removed the section on GPR data, but kept the evolution of glacier ski resorts, because it would not be quite straight forward to understand why the infrastructure is located at the current positions now causing the need for adaptation (at least for someone which is not too familiar with length and thickness changes on Eastern Alpine glaciers in last 40 years).