Dear Anonymous Referee #1 (corresponding to https://doi.org/10.5194/tc-2021-182-RC1)

We thank you for your valuable comments. Here we address how we revise the manuscript corresponding to your specific comments. The comments are in black words, which are followed by our responses in red words.

Comment

This paper describes an interesting and well-conducted study on very small glaciers in the Northern Japanese Alps. Although these tiny snow/ice patches probably have a very limited relevance, their monitoring allows new insights into glaciological processes determining this glacier size class. The evaluation of data specific to these miniature glaciers in comparison with worldwide observations results in valuable conclusions. The paper is mostly clearly written and illustrated and fits well into The Cryosphere. However, when reading the manuscript, a relatively large amount of minor issues and questions came up. At several instances, wrong units and (maybe) wrongly stated results are present that require careful re-reading by the authors. I have two more important conceptual comments that should be addressed during the revision:

Response

The following are responses to the substantive and detail comments.

Substantive comments:

1：Computation of mass balance profiles: The authors present mass balance profiles and elevation gradients for both winter and annual balance. Although they correctly mention and that the comparison of digital elevation models does not deliver local mass balance (allowing the computation of gradients etc.) and discuss emergence velocities of the ice, the conceptual approach remains partly vague and might be questionable for some situations: In fact, only for one of the five investigated glaciers, emergence velocities have been determined in the field. Moreover, the investigated locations only cover a limited elevation range, about at the median glacier elevation. More justification should be provided for the assumption that the measured emergence velocities are directly transferable to the other glaciers, and that the values measured are representative for the entire glacier. Conceptually, for typical alpine glaciers, emergence velocities are small around the median elevation, but increase in magnitude towards the top and the snout. If this also applies to the present situation on glaciers in the Japanese Alps, it might be that the measurements just captured the low emergence velocities at median elevation but the signal across the entire elevation range has been missed and mass balance profiles derived from surface elevation changes are thus biased. I do not consider this possibility as likely as the measured emergence velocities are much smaller than the mass balance rates, and the dynamics on the snow/ice patches is certainly different than on a standard glacier. Nevertheless, the issue needs to be looked at more closely, providing more justification for the assumptions.

Response

We change a profile of altitude changes instead of a mass balance profile in Figure 13, because accurate estimates of emergence velocities are difficult. In the Changri Nup glacier in Nepal, the emergence velocity of the glacier terminus is about 0.37 m a^{-1} against an average horizontal velocity of 9.7 m a^{-1} (Vincent et al., 2016). In the Argentière Glacier in the French Alps, the emergence velocity of the glacier terminus is 3-6 m a^{-1} against a horizontal velocity of 35-60 m a^{-1} (Vincent et al., 2021). As shown in these references, the emergence velocity is less than a tenth of the horizontal velocity on the glacier surface. In the case of VSGs in the Japanese Alps, the horizontal velocity is less than 4 m a^{-1}, and we consider that the emergence velocity is extremely small. In our revised paper, we add the above explanation with some references, to explain that the emergence velocity of VSGs in the Japanese Alps is extremely small. We discuss the characteristics of the mass balance profile using the profile of the altitude change as an estimation that the emergence velocity is low.

2：Converting snow/ice volume changes to mass change: The study is based on surface elevation changes that are subsequently converted to a mass change using a density assumption, both for winter and annual balance. In my opinion, there is a high potential for uncertainty that is barely described in the paper so far. The authors rely on some local observations of snow/firn/ice density and consider these values as universal, both regarding all glaciers and all years. This is certainly too much simplified. A variability in densities in the spatio-temporal domain is certain. It is clear that this cannot be measured but an additional uncertainty component that should be estimated based on as much evidence as possible is clearly needed. Furthermore, I am also partly doubtful regarding the chosen numbers and mentioned processes: (1) The density of winter snow corresponds to the observations at an off-glacier snow observatory. The authors mention that snow depth is half that on the glaciers, most likely resulting in smaller densities. Moreover, I would expect the winter snow depth on the glaciers to be particularly high because of a significant portion of wind drifted and avalanche snow. (2) The density of volume change is actually not only composed of the first annual layer's density, as suggested by the authors, but strongly depends on compaction dynamics in older layers. Although it is argued that transition from snow to ice is occurring during a single year under this climate, more evidence supporting this claim is necessary in my opinion. If incompressible glacier ice (900 kg m-3) is formed during a single year, this should become evident in field observations and images after a year with mass loss, which has not been demonstrated. Figure 2 seems to indicate snow surface almost everywhere. Also, even if compaction of older layers was zero, the strong variability in annual mass balance should lead to differences in the density of volume change between negative (high density of lost material) and positive (lower density of gained material) years – an effect that is not considered at present. I realize that it will be difficult to resolve all these processes without any further observations (that would be difficult if not impossible to acquire). But the problem should be carefully discussed and be incorporated in a better uncertainty estimate of seasonal/annual mass balances. Uncertainties presently are too small in my opinion.

Response

As you pointed out, the winter snow density includes uncertainty. Therefore, the annual mass balance does not coincide with the sum of the winter balance and summer balance. In our revised paper, we exclude the winter and summer balances in Table 4, Figures 9 and 13, and instead shown as accumulation depth and ablation depth. However, to compare with the mass balance amplitude of glaciers which is recorded in WGMS, we need to calculate the winter and summer balances of the northern Japanese Alps. In Japanese mountains, the snow density of avalanche deposits is larger than that of snowfall snow layers (Naruse et al., 1986; Abe et al., 2016). In the northern Japanese Alps, the density of avalanche deposits is 590 kg/m^3 (Shimizu et al., 1974). Therefore, we calculate the mass balance amplitude using the winter snow density in the range of 431 to 590 kg/m^3. Figures 11 and 12 and the appendix are also revised accordingly.

The transition from snow to ice occurred during a single year. We already confirmed an exposed ice layer on the glacier surface at the end of the snowmelt season of negative balance year based on aerial images and field (in Fig 4a of Arie et al., 2019). Therefore, we use 695 kg m^{-3} for positive balance years and 860 kg m^{-3} for negative balance years as the snow density. These densities are derived from measurement data by Kawashima (1997) and Fukui et al. (2018).

Detailed: comments:

Line 23: Why no equilibrium line? The ELA was just either above or below the glacier.

Response

We change to 'VSGs in Japan are not divided by a distinct glacier ELA into an upstream accumulation zone and a downstream ablation zone'

Line 31: only define acronyms if they are also used later in the paper.

Response

We remove 'GPR' as we do not use any more.

Line 58: "volume" and not "mass" change results from the geodetic method.

Response

We change it.

Line 59: Given the multitude of recent studies relying on the geodetic method for glacier change assessment, I would suggest selecting newer and more appropriate references on the methodology.

Response

We add some references.

Line 64: It would make sense to mention already here that the geodetic method does not provide local mass balance, and thus mass balance profiles, but only elevation change. Given an estimate of spatially distributed (!) emergence velocities AND local density, this elevation change can be converted into a mass balance profile (see major comment above).

Response

As responded second substantive comment above, in our revised paper, we show it as a profile of relative altitude change.

Figure 1: Inset in map is not very clear. It would help to show entire Japan and use colour/shading for the sea.

Response

We change it.

Figure 2: mention the year of the images in the caption.

Response

The year of shooting is already listed in 2016.

Table 1: The area stated in the last column is wrong (typo).

Response

We change your point in Table 1. For Karamatsuzawa Glacier, the correct value is 0.103 km^2.

Line 92: At least somewhere in a table, the exact dates of the measurements need to be given. Considering the high mass balance amplitudes, daily ablation/accumulation rates are important, thus also time differences of a few days will be affecting the stated seasonal/annual mass balances.

Response

We add the date of the aerial photography.

Figure 3: Would be easier to read with a larger contour line spacing.

Response

Although we created 15m and 20m contour lines, the topography of the surrounding mountains has become difficult to see in detail. We do not change the figure.

Line 135: rho is not the ice density but the average density of the lost or gained material, including the effects of the compaction of lower firn/ice layers.

Response

We change to 'snow and ice density'

Line 137: It is not strictly speaking the "stratigraphic system"! This system refers to the absolute maximum (winter) and minimum (autumn) of glacier mass. This date is normally unknown and can rarely exactly be met by monitoring programmes. I am quite sure that the surveys conducted do not correspond to this stratigraphic system (which is not a problem but needs to be stated). Regarding the stratigraphic system, referring to the much newer Cogley et al. 2011 publication would probably be more appropriate.

Response

We add Cogley et al. (2011) as a new reference. It is not easy to take aerial images before a snowy day, because Cessna flies on clear fine days. As you pointed out, our flight does not perfectly match the stratigraphic system. In line 138, we add the sentence as "The aerial images were taken just before the snow falls at the end of snowmelt season based on the stratigraphic system. However, the date has a gap of several days from a day before the snow falls, due to flight schedule and weather conditions."

Line 154: The choice of using the smallest glacier area over the study period for inferring total mass balance is interesting and should be discussed in more depth. In fact, reference-surface mass balances are then computed that may diverge quite a bit from the conventional mass balance, typically reported in glacier monitoring programmes, related to the actual glacier surface area. Both systems have their pro and cons. The mass gain or loss occurring beyond the minimum perimeter of the glacier could however also be determined and included in the computations. Or is there a reason that glacier extent has not been re-mapped every year?

Response

The annual glacier area is confirmed by the change of glacier terminus. However, we could not confirm the annual glacier area in the Japanese Alps, because snow cover around glaciers does not disappear on a positive balance year. In this case, we could not know the annual glacier outline in positive balance year. Therefore, we used the smallest glacier area for the calculation of glacier mass balance during the observation period. This area is composed of bare ice on the surface of the whole area. Therefore, this area is definitely a glacier area.

Line 159: Why is there only a 10m buffer around the glacier extent? The current glacier extent, or the minimum one? Chances are pretty high that there is remnant some snow close to the glacier in either the first or the second terrain model that would completely distort the correction process. Given that a terrain model can be generated for a bigger perimeter around the glaciers, why did the authors not use all of the stable terrain for this assessment? I would actually EXCLUDE the buffer zone. The text however clearly indicates that the comparison was done WITHIN the buffer zone. Furthermore, in Figure 4 it seems that the buffer is much bigger than the 10m stated. More explanation is needed here.

Response

In the case that we set a buffer zone around the glacier area, the DSM comparison includes the change of snow depth, because snow cover around glacier does not disappear on positive balance year. To avoid this, we took a 10 m buffer zone from the outline of the remaining snow area around the glacier in the positive balance year of 2017. The entire area within this buffer zone is the bedrocks. We compared the DSM differences at the same location through the observation period.

As the reason we do not calculate on all stable terrain, the accuracy of DSM depends on the slope (Fig. 7). The rock wall surrounding the glacier in the Japanese Alps is a steep slope. Therefore, the steep slope was excluded, to verify the accuracy in the slope zone of the same degree as the glacier area.

Line 161: probably “winter balance error” is meant here.

Response

We change it.

Line 169: Please use subscript for w and s after mass balance B (and not just Bw, Bs), always following the terminology proposed in Cogley et al., 2011.

Response

We change it.

Line 208: Do you mean "in elevation" instead of "in slope"?

Response

This is "in slope". The slope is steeper, the accuracy of the DSM becomes poor.

Line 222: This uncertainty refers to the digital elevation models but does not account for the (important) uncertainty due to unknown density of volume change. This aspect also needs to be considered.

Response

As responded second substantive comment above, in revise, we exclude the winter and summer balances in Table 4, Figures 9 and 13, and instead shown as accumulation depth and ablation depth. However, to compare with the mass balance amplitude of glaciers which is recorded in WGMS, we need to calculate the winter and summer balances of the northern Japanese Alps. In Japanese mountains, the snow density of avalanche deposits is larger than that of snowfall snow layers (Naruse et al., 1986; Abe et al., 2016). In the northern Japanese Alps, the density of avalanche deposits is 590 kg/m^3 (Shimizu et al., 1973). Therefore, we calculate the mass balance amplitude using the winter snow density in the range of 431 to 590 kg/m^3. Figures 11 and 12 and the appendix are also revised accordingly.

Line 247: The analysis of mass balance amplitudes is interesting. However, the unit of all numbers is wrong! Numbers are given in mm w.e., instead of m w.e. as stated in the text and in the figures. I would consistently convert all numbers to m w.e. (i.e. divide them by 1000). Furthermore, the effect of the partly short series (just four years for the studied glaciers) should be discussed. How strongly do the extreme years (2015-2016, high loss; 2016-2017: high gain) affect the result? Are the four years statistically sufficient to draw a final conclusion?

Response

We changed all units. The negative balance year of 2015-2016 and positive balance year of 2016-2017 are the largest and smallest snow depth respectively in Murodo-daira through the past 20 years. The total mass balance of both years was canceled out. The mass balance of VSGs was first observed in the Japanese Alps in this study. Our data showed that the glacier mass lost largely over a four-year period. The purpose of our paper is to discuss the mass balance changes over a four-year period. The characteristics of the glaciers in the Japanese Alps are well demonstrated even over a four-year period.

Line 261: Although I think the measurements likely do not capture the full range of actual emergence velocities, the analysis is well-conducted given the difficulties of direct field observations. Nevertheless, it remains unclear what has actually been done to derive mass balance profiles from the elevation changes. Which values for the emergence velocity have been applied, and how have they been extrapolated over the entire glacier surface, and to other glaciers? More details are needed.

Response

As responded first substantive comment above, we change a profile of altitude changes instead of a mass balance profile in Figure 13, because accurate estimates of emergence velocities are difficult. In the Changri Nup glacier in Nepal, the emergence velocity of the glacier terminus is about 0.37 m a^{-1} against an average horizontal velocity of 9.7 m a^{-1} (Vincent et al., 2016). In the Argentière Glacier in the French Alps, the emergence velocity of the glacier terminus is 3-6 m a^{-1} against a horizontal velocity of 35-60 m a^{-1} (Vincent et al., 2021). As shown in these references, the emergence velocity is less than a tenth of the horizontal velocity on the glacier surface. In the case of VSGs in the Japanese Alps, the horizontal velocity is less than 4 m a^{-1}, and we consider that the emergence velocity is extremely small. In our revised paper, we add the above explanation with some references, to explain that the emergence velocity of VSGs in the Japanese Alps is extremely small. We discuss the characteristics of the mass balance profile using the profile of the altitude change as an estimation that the emergence velocity is low.

Line 308: Interesting observation. Any possible explanations?

Response

The northern Japanese Alps is a heavily snow-covered area in the world due to the existence of Siberian High and the warm Tsushima current (Kawase et al., 2020). Annual variations of snowfall are quite large, which has a significant impact on the accumulation of avalanches and snowdrifts. The annual change of snow depth also influenced the volume of avalanches and snowdrifts. Therefore, the annual mass balance of VSGs is greatly affected by annual changes in winter mass balance.

Line 313: Well, it is not the mass balance profile that has been measured but elevation change. Actually, typical alpine glaciers would show very similar profiles of elevation change over seasonal and annual periods (the latter only if their mass balance is close to zero)! However, accounting for emergence velocities leads to the reported mass balance profiles that are based on local measurements of mass balance. In that sense, I am still a bit reluctant to accept that emergence velocities are more or less zero throughout the entire elevation range and that mass balance profiles are flat on the investigated Japanese glaciers.

Response

As shown in these references (Vincent et al., 2016; 2021), the emergence velocity is less than a tenth of the horizontal velocity on the glacier surface. In the case of VSGs in the Japanese Alps, the horizontal velocity is less than 4 m a^{-1}, and we consider that the emergence velocity is extremely small. We discuss the characteristics of the mass balance profile using the profile of the altitude change as an estimation that the appearance speed is low.

Line 339: It appears to me that, according to the Abstract of that paper, the stated number is incorrect.

Response

We change it.

Appendix: This very long table should rather go into a Supplementary Material and not an Appendix that is actually coming along in the same pdf as the paper. It is information that does not strictly need to be in paper, i.e. can also directly be obtained from the WGMS. Quickly state how the glaciers are ordered in the table. Units for winter and summer balance, as well as amplitude are wrong (mm w.e. instead of m w.e.).

Response

We change the unit and include to Supplementary Material.

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