Review of „Geodetic point surface mass balances: A new approach to determine point surface mass balances from remote sensing measurements" by C. Vincent et al.

This manuscript presents a method to derive glacier point surface mass balances from vertical velocities and surface elevation changes. In contrast to similar techniques using the emergence velocity, this method avoids the problem of determining the representative surface slope. In this respect, this new approach circumvents a considerable error source, because surface roughness and medium scale undulations obstruct the effective glacier surface slope. However, the problem remains to accurately determine the vertical velocity at the glacier surface, while the horizontal surface velocity can be more easily derived from remote sensing information. The presented method has a high potential for enabling large scale surface mass balance surveys and the manuscript clearly presents this potential also with respect to the usage of remote sensing information. However, the main difficulty of the validity of the vertical velocity in space and time is not fully investigated so far. It should be possible to use sensitivity experiments based on interpolated parameter fields, to demonstrate the potential errors, which are unavoidably introduced by relying on spatially and temporally discrete remote sensing data. This would allow to evaluate the feasibility of this method in a much better way. In the following, some improvement are suggested, which very likely are rather easily implemented.

Structure: The numerical analysis should be included main analysis, not in the discussion, as this an important component of the overall concept.

Title: It is not obvious that the paper deals with glacier mass balance, even though it is submitted to a cryosphere related journal.

Given the multitude of available data, I am missing a more rigorous analysis of the possibilities by using the parameter fields. All of the parameters show homogeneous spatial fields, even though the local gradients might be large. Therefore, missing point data could also be derived from the spatially interpolated data, which might enable a larger flexibility. This is also true for the temporal evolution. It is stated that the method works with observations of the vertical velocity during periods previous of the elevation and velocity change determination. But there is no analysis, how the temporal change in vertical velocity (and there is a non-negligible trend observed) might impact on the results. The numerical analysis might provide very valuable insight how temporal trends could even be anticipated for certain geometric conditions.

Abstract:

L. 57-64: It is correct that point mass balance measurements represent the balance between local accumulation and ablation and thus a resultant climate information. However, this part of the manuscript provides a rather simplistic description of the situation. Point mass balances need to be measured at constant locations, not along moving stakes for multiple years. What about long term accumulation observations at stable locations? Do we need many distributed measurements across a glacier for resolving climatic information? I know that some of the questions are partly answered in the earlier publications of C. Vincent, but a short summarizing discussion might be helpful.

L. 66-68: There is an objective and an aim of the manuscript. These two sentences point in the same direction and could be combined.

L. 77: The area determination is 17 years old. Is there a newer area estimate?

L. 79: The tributaries are facing SW.

L. 88: It is more likely 2350-2400 m?

L. 97: What does “accurately” mean here: accurately at the end of the ablations season, or accurately like “highly precise”?

L. 100ff: Even though the strategy is explained later, it should be made clear here that the stakes are re-drilled each year at the original position in order to maintain the local reference system. What happens to the remaining stakes (10 m will not melt out at each location every year)? Are they also measured in the following year?

L. 108: 0.01 m accuracy is rather optimistic, given the short occupation times (what about resolving multipath uncertainties from 60 observations?).

L. 110 ff: This paragraph starts with vertical velocities, without introducing the requirements of using velocities at all. Maybe it is better to insert a sentence that both velocity components are required. The “bottom tip” of the stake means the “real bottom” where the lowermost stake segment touches the ice? “Emergence measurements” mean ablation measurements? How can you be sure about the tilt of the stakes in the borehole? Does this tilt change over time? How do you obtain the same level of accuracy for horizontal and vertical velocities, while the z-component of GNSS measurements are usually not as precise as the horizontal ones?

L. 120: This is probably “focal length”, not “focal lens”.

L. 123: These are probably “resulting” and not “original” ground resolutions. The original ground resolution of the photographs might be higher (smaller dimension).

L. 130: Accuracy information for the location and elevation of the ortho-mosaics and the DEMs are missing.

L. 134: I am confused. In line 123, the dimension of the ortho-mosaic is given with 0.1 m. There is no need for resampling then.

L. 141: But this also depends on the quality of the ortho-images and their co-registration, which is not provided.

Fig. 1: It would be instructive, to have isohypses across the glacier, in order to see the exact location, as the numbers only indicate a broad region. Also for Mer de Glace, some isohypses would be helpful.

Fig. 2: The colour coding should also be included as legend in the figure itself.

L. 159: space between “framework” and “used”.

L. 170: the surface mass balance needs to be expressed in the same dimension/material as the other components.

L. 173: the downslope direction is a bit misleading, as local slope patterns might show different directions as the main flow. The statement is only true for the mean slope over a certain distance.

L. 179/180: Well, slope can be calculated along any distance. But this is a critical point of the entire theory: which is the appropriate scale of surface slope for such analysis. I am not sure that the annual displacement is the correct scale. This requires some elaboration.

L. 186: This conclusion tells us that the determination of the emergency velocities has rather large errors.

L. 203: It should be noted that all vectors in this diagram have the unit of velocity: m/yr.

L. 217ff: It took me quite a while to digest this statement. Finally, I think the strong point of this formulation is that measurements are taken at the annual displacement distance. In consequence, the relative thickness change is based on identical geometric and surface conditions. A small scale surface undulation is detected at exactly the same relative location and therefore does not influence the elevation change. Also surface conditions, like patches of lower albedo, are advected and do not alter the ablation conditions. Maybe this should be elaborated.

L. 229-240: The description of velocity measurements and interpolation of the velocity field is not fully clear. First, it seems to me that a larger number of stakes were not drilled at the last-years location in 2018 (stakes 12 and 14-19). Even if this is mentioned later, it should also noted here, because these are not negligible deviations. As the surface velocity field of a glacier is rather homogeneous (which is also documented in Fig. 5), the measurement location has no large influence on the interpolated velocity field, as long as the measurement density is sufficient. However, the exact location of the stake is important for the application of the presented theory.

L. 243: Which two periods do you refer to?

Fig. 5b: In my opinion, the larger differences of the stakes with offsets in the relocation are only due to larger uncertainties in the velocity determination. In principle, temporal deviations in the vertical velocity field (not in the point measurements) should be expressed in an analog manner as in the horizontal velocity field due to the incompressibility condition of ice.

L. 281ff: Here you use the slope along the 1-year displacement vector, correct? This is probably not an appropriate choice, even for a smooth glacier section.

L. 313: You should provide a reasoning, why you use the vertical velocities of the previous year, instead for the year of the mass balance measurements.

L. 317-324: This observation reflects the situation that the vertical velocity field shows considerable spatial gradients. It would be interesting to see how the results change if you use the values from the interpolated field at the exact measurement locations.

L. 328: This 15 m is probably site related and should be discussed.

Fig. 9: As far as I can see, the vertical velocities are measured at the midpoints of the annual displacement vectors. This is different from the method described in Fig. 3, where the vertical velocity is determined for the downstream displacement vector. How does this influence the resuts?

Fig. 10: The isohypses are very thin and hard to see. I am not sure what additional information is provided by this figure. It is also not referenced in the text.

L. 404f: I do not understand this remark, as it is stated in the introduction that a noticeable debris cover is only observed below the ice fall.

L. 407f: Does this infer that the vertical velocity is determined for each single year from GNSS measurements and then the mean value for 2001-2018 is used, based on the fact that the stakes were replaced regularly within a distance of 35m?

L. 461f: This argument is not correct, as can be seen in Fig. 11b. But the changes are rather smooth and comparably small, but definitely not negligible.

L. 480: Again, small is a rather relative condition. Chages from 0.2 to -0.5 m/yr within one year (Fig. 11b, stake 2) are hardly small.

L. 483 onward: In my opinion, this section belongs to methods and results, respectively, as this is an essential part of the paper and should not be presented in the discussion.