Dear Reviewer 1,

Thank you very much for providing your valuable comments that helped us to significantly improve our manuscript. Below we provide our detailed responses to your questions in italic font. Here is a quick summary of changes:

- *Addressed reviewers' comments to the best of our knowledge;*
- *Added recent Sentinel-1 data, mainly to investigate what is happening at region P1 at the Malaspina Glacier;*
- *Recomputed offset maps using smaller 128x128 window and the Gaussian filter with 1.3 km 6-sigma width;*
- *Detected another surging Kluane Glacier and analyzed it in-detail;*
- *Used OGGM software to extract flow lines and performed all analysis for selected flow lines;*
- *Simplified interpretation by removing reference to kinematic waves, which require more attention and, which possibly will be addressed in a separate publication.*
- *Provided animations for four AOIs.*

Best regards,
Sergey Samsonov, Kristy Tiampo, and Ryan Cassotto

Review of Measuring the state and temporal evolution of glaciers using SAR-derived 3D time series of glacier surface flow

By Sergey Samsonov, Kristy Tiampo, and Ryan Cassotto

This paper present a technique for producing times series of the 3D glacier motion using ascending/descending data.

Overall it's a good paper, and the technique seems sound. That said, though I would like to see a better technical discussion with respect to errors and temporal/spatial resolution as noted below. The discussion needs some work make it clear what the data is actually showing, especially with respect to what is the source of the vertical displacement (see below).

Reply: Thank you. We addressed these issues.

General points I don't see much discussion of errors. Typically, with 6-day sampling and azimuth offsets, you are going to get about errors of about 20-m/yr. The vertical velocities are more driven by range offsets, but the time everything is solved for, some of those errors are going to fold into the vertical solution.

Reply: We utilize a standard speckle tracking technique, which sources of errors are well understood from previous studies. Note that while individual offset maps have large errors (30 m/year in range and 120 m/year in azimuth, see discussion), the derived mean velocities have significantly smaller errors (0.7, 0.3, and 0.2 m/year, while the maximum values are 21, 18 and 7 m/year, for northern, eastward and vertical components, respectively). This is similar to reconstructing GNSS velocities from long time series of GNNS observations.

Vertical motion is a combination of the vertical motion due to surface parallel flow other factors (e.g., submergence/emergence velocity or subsidence/inflation due to subglacial water flow). In general, the surface parallel vertical displacement will be the dominant term and it will vary with the horizontal speed, especially for mountain glaciers with relatively steep slopes. The other forms of vertical displacement are the more interesting terms though. So, it would make sense to compute the surface parallel component and remove it to isolate the other types of vertical motion. Otherwise statements like “The predominately downward flow of ice observed throughout the Malaspina Glacier’s massive lobe (Figure 4b,c) indicates that ablation rates have exceeded emergence velocities during our 4 year study period, implying” are potentially incorrect or at least presented without proper context. I have no reason not to believe the glacier is wasting down faster than the emergence velocity, but in general glaciers flow downhill, so that could largely due to the surface parallel flow component. More-over, from image to image, the glacier is largely measuring the same coherent patch of speckle, which would record motion of the surface due to ice dynamics. It should not be measuring downward motion of the surface due to direct ablation (unless you phase of the ablated layer is accounted for, which does not seem to be happening here). In other words, this is like measuring the downward motion of a GPS on a pole sunk in the ice, which will not measure ablation, vs a GPS placed on the surface, which will measure ablation. Distinctions such as these need to made in the discussion.

Reply: We have differentiated between surface parallel flow (SPF) and non-SPF in another paper that was recently published (https://doi.org/10.1016/j.rse.2021.112343). Here we can refer to panel “c” in Figures 6-9, where it can be seen that the slope of flow vectors differs from the slope of the topography. If motion was parallel to the surface then flow vectors would also be parallel to the slope. Also, please see four animations that show temporal variability of the flow lines relative to the topographic slope. We addressed the difference between Eulerian (SAR) and Lagrangian (GNSS) representations in the Discussion section.

There is some discussion about penetration effects, but it is import to note you can get some really strange offset patterns when you have soaked firn, which can be spatially coherent over large distances (can map into errors of several hundred m/yr). I am not sure that some of what’s being seen is this kind of effect (e.g. blue patches Fig. 4b).

Reply: That is quite possible. It is, however, not specific to our processing technique. Our final maps of vertical flow are based on averages of four full years of observations; thus, have an equal number of melt and accumulation seasons that should account for differences in penetration depth.

This is how we addressed this issue in the previous manuscript (https://doi.org/10.1016/j.rse.2021.112343):
“The penetration depth likely changes throughout the year due to seasonal temperature changes but this change is small over 6 to 12 days, over which the individual offset maps are computed. The error due to the seasonal variability in penetration depth is removed by differencing primary and secondary observations. This can be deduced from observing seasonally correlated signals only in a few regions (at low and high elevations). Higher temperatures during summer would result in higher water content and less penetration depth, which would appear as an upward movement in the flow displacement time series. We, however, observe downward motion during summer. The radar penetration depth could also change throughout the day due to surface melt and meltwater percolation variations. Ascending and descending data is acquired at different times; it is processed separately and combined only during MSBAS analysis. Any error due to diurnal variations in penetration depth is also removed by differencing primary and secondary observations since both images are acquired at precisely the same time of day.”

Point, P2, seems to exhibit a seasonal cycle not seen in the other data. It also happens to be near a marginal lake. I wonder if pressure variations as the lake fills and drains are contributing to the seasonal up and down motion. Some discussion as to why this point has a strong seasonal signal would be good.

Reply: Note that point numbering has changed, P2 is now closer to current P4. Actually, many of the glaciers in our study area show similar behaviour (hence this point is chosen as a characteristic). This is evident in the time-series shown in the supplemental movies. The motion is believed to be caused by melt-induced seasonal variations and it was modelled in (<u>https://doi.org/10.1016/j.rse.2021.112343</u>). Note that the magnitude of vertical motion caused by the melting process is small and nearly constant. It is hard to see in plots that have a different vertical scale (e.g. P1).

Specific points Line 48: "However, the SPF constraint is only applicable to glaciers in steady state." This statement is not correct, the SPF assumption ignore the submer-gence and emergence velocity and other vertical motion, which is true whether or not the glacier is in steady state. And if the glacier is not in steady state, it will still measure vertical velocity variations that are parallel to the surface.

Reply: That is correct, we now understand these processes better. We revised the manuscript accordingly.

Line 84: A 256x256 sampling window (both patches???) will provides about 3.5km resolution (256 * 13.9 m azimuth resolution), which is further degraded by a 2-km median filter. In addition to the lack of resolution, this can cause problems where the matcher will lock on stationary rock areas more easily than the glacier to report zero velocities a km or 2 inboard of the margin. How is this dealt with. There should be some discussion of what the spatial resolution is. Certainly, the ground resolution is not 200-m as stated, even if the data posted are at 200 m.

Reply: In the revised version we reduced the correlation window to 128x128 pixels and used a Gaussian filter with a width of 1.3 km (6-sigma). We recognize the benefits of having high-resolution results. Unfortunately, in this area, the application of a small window produces measurements that are too noisy, and if we only select pixels with high SNR the spatial coverage reduces to nothing. Therefore, we are limited to using a larger window.

We consulted the developers of the GAMMA processing software that is used to compute speckle offsets. We were advised that the window that is used to compute the offsets is not uniform, pixels in the centre have larger weights than those pixels on edges. The effective resolution is about four times higher than the window size; thus, the effective resolution is X by Y. The process of the extraction of offsets, as it is implemented in the software, is not linear. We acknowledge that the spatial resolution is reduced by using such a large window. However, this is necessary for extracting temporal information. Note that the computation of offsets, in general, is not specific in any way to the technique presented here.

To confirm this we computed offsets for a single pair using 64x64, 128x128, and 256x256 correlation windows. In figure 1, below, we present these results before and after filtering. As you can see, while there are differences, overall the signal is consistent. Note that filtering does not reduce the resolution significantly. Again, we found that these processing parameters are optimal for our purposes in this region; however, it does not mean that they would be optimal in other areas.

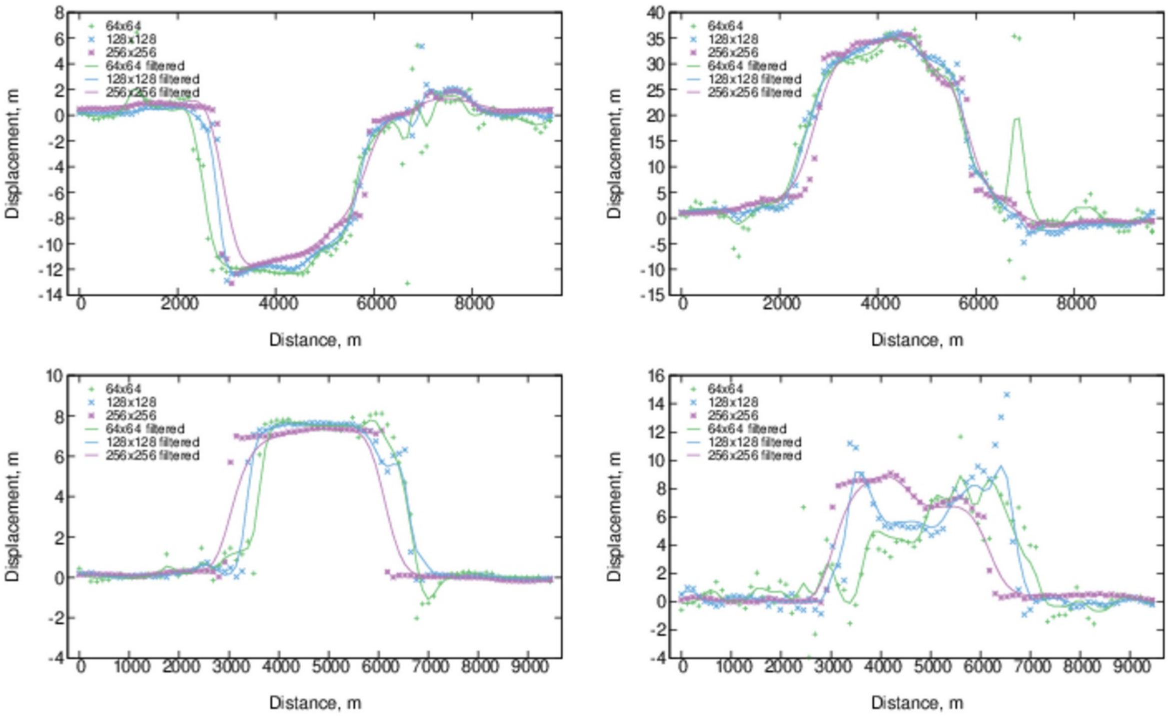

Figure 1: (top-left) Seward range, (top-right) Seward azimuth, (bottom-left) Klutlan range, (bottom-right) Klutlan azimuth.

Line 85: What corrections were applied for baseline and to calibrate the data (e.g., were control points used to remove biases).

Reply: We use precise orbits downloaded from the ESA website. We calibrate the offsets by fitting and removing the polynomial model. This approach works well in this region where most areas do not show any motion. The entire Sentinel-1 scene is processed as a whole, and it is cut into small sub-regions only for visualization in the manuscript. Note that the entire Sentinel-1 scene extends far beyond the area shown in the manuscript. The software provides alternative methods of calibration that can be employed in other, more complex, regions (e.g. calibration against multiple reference regions, Z-score).

Equation 1 – please break separate into two equations (put the cumulative as a different equation).

Reply: Done. Now we have two equation 1a and 1b.

117 “temporal smoothing” What is the temporal resolution after regularization.

Reply: The effective temporal resolution decreases by about a factor of two. This depends on the strength of the regularization that is controlled by a regularization parameter lambda. For some studies, the decrease of a temporal resolution would be unacceptable (e.g. timing of the particular events). In that case, a small lambda can be chosen. In this study, we believe, it is not particularly important, because trends are overall smooth. The selection of the optimal lambda can be performed using the L-curve method, which has been explicitly shown in our previous manuscripts (https://doi.org/10.1016/j.rse.2013.12.017, https://doi.org/10.1080/07038992.2017.1344926).

Line 224-226. I would like to see the surface parallel flow components removed before seeing discussion about kinematic waves.

Reply: This issue was raised by multiple reviewers but it was not given a proper detail in our manuscript. Since it is beyond of the original scope of the manuscript we decided to remove the discussion on kinematic waves entirely.

Figures. The x-axis of the vector profile plots could be lined up with the color times series plots. The vector plots while pretty, don't really give a good idea of the magnitudes of the vertical velocity. Please show the profiles also on the b panels since that actually shows magnitudes of vertical motion. Also use some kind of symbol on the c and d plots to indicate where the points PX are.

Reply: To address this issue we introduced distance markers in green in panels "a" in Figures 6-9. These distance markers correspond to distance along profile in panels "c"-"e" in Figures 6-9.

Would be helpful to see the color panels broken out separately as horizontal and vertical magnitudes. The alternating patches of slow and fast flow are strange.

Reply: We have done it – panels "d" and "e" in Figures 6-9. The alternating patches along the y-axis show seasonal variations in flow while the same alternating pattern along the x-axis shows spatial variability along flow. Note that in the revised manuscript we utilize an OGGM software (https://docs.oggm.org) for selecting profiles along flow lines to improve sampling along flow, which has improved the results.

Dear Reviewer 2,

Thank you very much for providing your valuable comments that helped us to significantly improve our manuscript. Below we provide our detailed responses to your questions in italic font. Here is a quick summary of changes:

- *Addressed reviewers' comments to the best of our knowledge;*
- *Added recent Sentinel-1 data, mainly to investigate what is happening at region P1 at the Malaspina Glacier;*
- *Recomputed offset maps using smaller 128x128 window and the Gaussian filter with 1.3 km 6-sigma width;*
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Best regards,
Sergey Samsonov, Kristy Tiampo, and Ryan Cassotto

Review of "Measuring the state and temporal evolution of glaciers using SAR-derived 3D time series of glacier surface flow" by Samsonov, Tiampo, and Cassotto

Summary

The authors discuss a method for inferring time-dependent 3D surface velocity fields from synthetic aperture radar (SAR) data and apply this method to data collected from Sentinel I in 2016-2020 over five outlet glaciers in Alaska: Agassiz, Seward, Malaspina, Klutlan, and Walsh. Their results show complex glacier flow fields and temporal variations, and capture a host of interesting phenomena, including seasonal variations in ice flow, a surge, and dynamical glacier states. The authors present a series of figures showing the resulting velocity fields and report on their results and a few possible implications.

The manuscript is in line with a growing area of research that has great promise to advance our understanding of the cryosphere due to the volume of information available from modern remote sensing platforms. The authors have described a potentially useful method and chosen an interesting study area. As such, this work may be of interest to the TC readership.

However, as discussed below, the paper needs a lot of work in terms of organization, presentation of the methods, and analysis of the results before it can be considered acceptable for publication in the scientific literature. Indeed, I found the paper to be frustrating to read for a variety of reasons, perhaps the main reason being that the authors seem to be trying to claim levels of success, novelty, and generality that their study does not merit rather than taking a measured approach to demonstrate that their method works, inform readers of the merits of their method/study, and to connect their work to the scholarly literature. The authors almost completely ignore the existing body of work on time-dependent 3D surface velocity fields (which their citations suggest they are aware of), with the exception of references to their own papers and a couple of passing references to a paper (Guo et al., 2020) that presents what appears to be the exact same method the authors are presenting here. As a result, the authors do not place their study into the proper context, nor do they give readers the ability to compare the strengths and weaknesses of the authors' method and those of other methods. But most importantly, the authors do not demonstrate an awareness of the documented challenges of inferring 3D time-dependent velocity fields from satellite data and thus they ignore any consideration of accuracy and precision in the measurements, mixing between the inferred velocity components, and propagation of measurement errors. Perhaps because of this oversight, the authors make claims about the viability of their method that are unsupported by the work presented in the manuscript. Nowhere do the authors test their technique nor make any meaningful attempt to show that their method actually works; rather, we get a few basic equations, some results from actual SAR data (which can only show that the matrix in the authors' method is invertible but cannot show that there is sufficient information to make the inferences the authors are trying to make), and then unsupported claims like (line 164) "[t]he technique in this study is a viable solution for computing 3D flow displacement time series…" The authors never discuss errors nor the limitations and challenges of their method, inferences of multidimensional flow velocities and other such matters that one would expect to find in a scientific publication. In addition, the presented analysis of the results lacks the depth and detail needed to provide new insight into the glaciers being studied or glacier dynamics in general. The current manuscript is very short compared with the vastness and richness of the material the authors are trying to cover, and the supplementary material contains only a single figure, so the authors have plenty of space in the main text and supplement to expand on their methods and findings. More details are provided below.

Reply: Thank you for sharing your opinion. We answer your questions below.

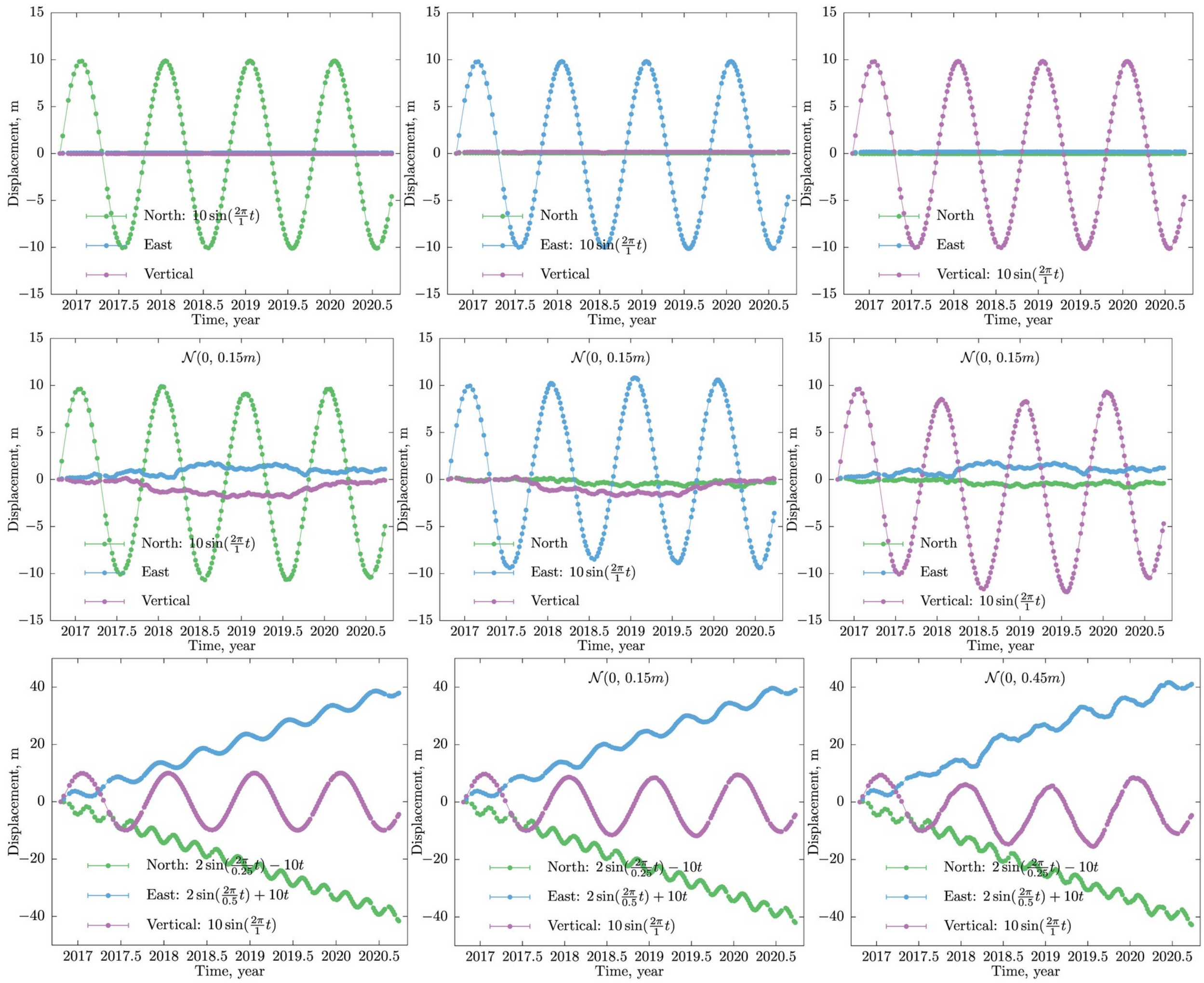
Major points:

- The title and abstract of the paper suggest the main goal of this manuscript is to present a general method for inferring time-dependent 3D surface velocity fields of glaciers. But the methods are presented as an incremental step from previous work and not described in sufficient detail to merit publication based on the methods alone. Key information that readers need to understand the method and reproduce the results are missing. A few specific points:

o No meaningful validation of the method is provided. In my reading, I did not find any evidence that the method produces accurate 3D velocity fields. I only found reference to one comparison between the authors' results and independent measurements taken over vastly different time scales (Gardner et al., 2018, 2019). This comparison is given in the figure in the supplement (Fig. S1). I would argue that the authors' results differ markedly in all cases from the measurements of Gardner et al., 2018, 2019. Even on the glaciers where the authors claim 'nearly identical results' there are clearly significant disparities

(100s of meters per year, or roughly a factor of 2) between the data sets. The authors provide some plausible suppositions to explain these disparities but never explore these possibilities. Rather, the authors give us offhand references to filtering and other technical matters that can and should be tested and some discussion of how the flow of glaciers is expected to differ between the more recent (2016-2020) observations made by the authors and the multi-decadal average of Gardner et al., 2018, 2019. Indeed, the comparisons between the authors' velocity fields and those of Gardner et al., 2018, 2019, especially over surging glaciers, are scientifically interesting but are not viable tests of the methods presented here. If the authors wish to present a new method, especially one that is as generally applicable as they claim, they need to conduct multiple appropriate tests on synthetic data to test their method under the conditions expected in the natural environment and to convincingly show readers that their method reliably produces accurate results. I cannot stress enough that the authors do none of this work in the current manuscript.

Reply: The technique presented here is based on 10 years of research and is incremental in nature. Many issues raised by the reviewer have been discussed in previous publications. In the revised version we provide synthetic tests that demonstrate that 3D flow displacement time series can be reconstructed from ascending/descending range/azimuth data very well. See figures below for a preview, figures are numbered 1-9 from top left to bottom right corners, the actual geometry matrix from the manuscript is used: figures 1-3 individual components are reconstructed from a harmonic input without noise (see legend for input signal); 4-6 same as 1-3 but with 10% noise; 7-9 all three components are reconstructed with zero, 10% and 30% noise. As you can see the reconstruction is very good. Similar plots were produced for the 2D tectonic time series in the first MSBAS paper https://doi.org/10.1111/j.1365-246X.2012.05669.x.



o The authors need to provide some discussion of the effects of the viewing geometry on the inferred results. While range and azimuth offsets are orthogonal to one another in existing SAR systems (where the radar line of sight is orthogonal to the platform velocity vector by design), ascending and descending orbits are not orthogonal (as shown in Fig. 1). Thus, the viewing geometries are nonideal and the relative orientation of the orbits and the flow direction of the glacier influences the precision of the inferred velocity components. This geometric effect is amplified by noise in the measurements, particularly the disparity in noise between range and azimuth offsets (where range offsets generally have higher signal-to-noise ratios than azimuth offsets). These effects are discussed to some extent in Minchew et al., 2015, 2017, though the basic ideas are well known from GPS and should be given ample consideration in work of the type the authors are presenting.

Reply: The effect of viewing geometry has also been discussed in previous papers and are cited here. We also show that the rank of the geometry matrix for a case with one set of velocities is 3. This means that the solution exists, and is unique and stable. See also the synthetic tests above that fully support this statement. We are happy to refer the readers to Minchew et al., 2015 paper for additional discussion by citing it in our manuscript.

1. Given the orientation of satellite orbits, I expect that there are strong covariances between the inferred vertical and horizontal components of flow. This effect is likely to be most pronounced on Klutlan Glacier, whose flow direction is close to the line-of-sight direction of the radar, meaning that the range offsets pick up most of the horizontal motion and all of the vertical motion while the azimuth offsets provide relatively little constraints. The covariances between horizontal and vertical velocity components are likely to be lowest on Malaspina Glacier, which is flowing more or less south, in a direction that is close to the azimuth direction of the SAR data. This orientation is favorable to inferring 3D velocity fields as the azimuth offsets (which are purely horizontal) are doing most of the work to constrain the horizontal velocity while the range offsets provide information on vertical velocity with little direct influence from the horizontal components. More generally, it is worth noting that the authors' results seem to show that the vertical component of velocity is largest in areas of the glacier that are flowing more along latitude (i.e., east/west flow direction, which is close to alignment with the radar line of sight) than along longitude (which is close to being aligned with the orbits, or azimuth direction), which suggests covariance between the horizontal and vertical components of velocity in the east/west trending flow. Again, all of these topics are discussed in some detail in Minchew et al., 2015, 2017. The take-away is that the authors need to quantify and discuss the geometric and measurement errors for their methods to be publishable and to support any claim of generality.

Reply: The synthetic tests and the properties of the geometry matrix show that viewing geometry is sufficient for reconstructing any 3D flow displacements time series. As a result, there should be no need to present the covariance matrix for synthetic tests since the results in the figure are very clear, covariance terms are zero.

1. There is no discussion of errors. The authors provide error bars on the results they present in Fig. 7 but these are merely spatial variances. I would expect a modern paper on geodetic methods to discuss formal errors in the SAR offset fields, how formal errors in the SAR measurements are propagated to the inferred 3D fields, how viewing geometry impacts the results (as just discussed), and any sources of additional error that may not be accounted for in the formal error (e.g., the influences of radar penetration depth and surface moisture, as discussed by Minchew et al., 2015). The lack of treatment of errors is a considerable omission from a methods-focused paper that should be rectified before publication.

Reply: We discuss errors in the revised manuscript. In the original version we omitted this because at the level that we wanted to present the precision and accuracy is sufficiently high and does not raise any concerns, as it can be seen from time series. All the errors that the reviewer mentioned are not unique to this technique and have been previously discussed by the authors, and others, in earlier work. However, we agree that this is the body of work that is not likely to be familiar to many in this audience. We now provide a synopsis and appropriate references.

1. The authors need to provide a reference to the form of the Tikhonov regularization matrix of various orders, some convincing evidence that regularization “is not critical” in this case (cf. line 119), and some discussion of why regularization doesn’t make a difference in this case and the conditions under which regularization should matter. Finally, the authors need to be clear how regularization is being applied: are the authors regularizing in space or time or both? Eq. 3 suggests that regularization is only applied in time but this should be clarified in the paper. It would seem that the authors are filtering in space using median filter with a window size larger than the width of some of the glaciers (line 85). A discussion of how this filter and window size were chosen and the effects of a nonlinear (median) filter and such a heavy filter on the final results would be useful.

Reply: This has been discussed and explicitly shown for the 2D case of tectonic deformation (mathematically identical) in https://doi.org/10.1080/07038992.2017.1344926, and referenced here. Theoretically, it is possible to regularize in time and space but the resulting matrices become so big that they cannot be handled by modern computers. We also provided a discussion on the filter selection.

- The authors need to discuss the existing literature and place their methods into proper context. Methods for inferring 3D, time-dependent surface velocity fields of glaciers (e.g., Minchew et al., 2015, 2017; Milillo et al., 2017; Guo et al., 2020) and generalized frameworks for inferring multi-dimensional surface velocity time-series (e.g., Greene et al, 2020; Riel et al., 2020) have been published. Only two of these papers are cited in the current manuscript and neither of these are discussed in any meaningful detail. In particular, it appears as though the methods of Milillo et al., 2017, and Guo et al., 2020, are strikingly similar to those presented by the authors in this work. Certainly, they all take the same basic approach of using the small-baseline subset (SBAS) method extended to multiple dimensions.

*Reply: We provide additional background information on the history of multi-dimensional time-dependent surface flow in the revised manuscript. The SBAS is a general linear inversion technique, simply speaking it is V*t=D.*

This similarity between Milillo et al. (2017), Guo et al. (2020), and the current work needs to be properly explored in the manuscript. In my own reading of Guo et al., I cannot find any real difference between their approaches and those presented here. Indeed, the current authors state on line 170 “[o]ur approach is conceptually similar to the technique of Guo et al. (2020)…[h]owever, our software can additionally compute 1D, 2D…, and 3D flow velocities and displacement time-series and linear rates.” It is important here to distinguish methods (and the ideas behind them) from implementation (software and tools); it appears that the authors’ claim to novelty is in the implementation and the additional features of their software rather than differences in methodology. In other words, their software bundles several methods that are discussed in previous publications into a single user interface. I find this confusing because the paper seems to be about methodology, which is separate from implementation. I respect the fact that the authors have been working independently on this method for some time and that they should have their efforts rewarded by having the opportunity to publish their work in the scholarly literature. But more needs to be done to clarify the differences (if any) between the method presented here and those in the existing literature. If there are no meaningful differences, the authors should make that clear and emphasize the unique aspects of their work and results. Thorough testing of the method, exploration of precision and geometric effects, and robust uncertainty quantification (all discussed above) would make this paper unique from Guo et al. and would add value to the authors' methods.

In contrast to the studies mentioned in the previous paragraph, Minchew et al., (2017), take a markedly different approach to inferring time-dependent 3D velocity fields. I note that this paper is cited in the current manuscript but simply in a list of other papers that apply InSAR to glacier flow; the fact that Minchew et al. (2017) present a method for inferring 3D, time-dependent velocity fields is ignored by the authors, as are the lessons learned about the challenges of accurately inferring 3D, time-dependent velocity fields from SAR data (as discussed above). It would be useful to compare the approach of Minchew et al. (2017) to the authors' approach in an insightful way as the two approaches are quite different. For example, the authors could note that Minchew et al. assume a form for the temporal basis functions based on prior knowledge of the study area, while the current authors invert a matrix for displacement at a given time. The need for prior knowledge in the Minchew et al approach means that this method is not general and so its application is limited to areas where the assumed basis functions should be valid. But the advantage of the Minchew et al approach is interpretability of the results, straightforward connection of the results to the physics of the systems being observed, and robust uncertainly quantification, all things that are lacking in SBAS-based methods like those presented here.

A recent improvement to the work of Minchew et al. (2017) is Riel et al., 2020. I won't fault the authors for not discussing Riel et al. (2020) in the current manuscript as this is very recent work, but it would be good (though, not required) to include a brief discussion of Riel et al. (2020) in the revised draft to add context to the authors' work. Riel et al., 2020, adopt some of the methods of Riel et al., 2014, 2018, and apply them to remote sensing observations of glaciers. From a methodological perspective, this has the effect of generalizing the approach of Minchew et al. (2017) to allow for a generic set of temporal basis functions, from which a sparsity-inducing optimization is used to identify the simplest set of basis functions that describe the data. Here again, the main advantage of this approach is interpretability of the results (and robust uncertainty quantification), which provides the ability to decompose the observed signal into short and long-term variations, and features to ability to constrain transients, secular, and periodic signals. There are certainly limitations to the Riel et al (2020) method, namely that it still requires some level of prior knowledge to provide confidence in the resulting basis functions. The authors' methods may be complementary in the sense that they do not rely on basis functions. Again, the authors' method provides flexibility at the expense of interpretability of the results, where was the Minchew et al. and Riel et al. approaches sacrifice flexibility in the method for enhanced interpretability of the results. An appropriate exploration of these differences in approaches will provide readers with insight into the respective strengths and weaknesses so that they can make informed decisions about which methods may best suit their needs and where improvements can be made.

Riel et al (2020) take the time-dependent methodology further by introducing methods to quantify the propagation of waves through glaciers in the case of generalized methods (Minchew et al., 2017, quantify wave propagation but this is implicit and straightforward in the periodic basis functions).

Importantly, Riel et al (2020) are able to track waves of different frequencies (because separation of frequencies is inherent in the time-series methods) and are able to show that waves with seasonal and annual periods on Jakobshavn Isbræ, Greenland, are dispersive (phase velocity varies with frequency). These waves are likely to be kinematic waves due to the long periods and contemporaneous changes in ice thickness (though there are caveats to this hypothesis and it remains to be tested), so the findings of Riel et al

(2020) are also applicable to the interpretation of the results presented in the current manuscript because the authors report observations of kinematic waves.

The work of Greene et al., 2020, should also be referenced and discussed in the revised manuscript. This is a conceptually different approach to all of those mentioned above and warrants comparison to the method being proposed by the authors. In this work, the authors apply a generalized method that disentangles periodic variations from non-periodic variations.

Reply: The data contains some fixed amount of information, you can use it to extract N parameters with high precision or M parameters with less precision, assuming N<M. Minchew et al. (2017) and Riel et al. (2020) chose to solve for a small number of parameters that can be resolved with high precision. Here we choose to solve for the maximum possible number of parameters, again that are naturally resolved with less precision. Our technique can be used in any environment; the quality of the results is, of course, dependent on the input data. Other techniques are limited in scope as the reviewer said himself in his comment. Based on our reading, the technique of Milillo et al. (2017) is close to Minchew et al. (2017) while the technique of Guo et al. (2020) is closer to ours; we detail those differences in the revision.

- Without some quantification of the errors and analysis of the covariances between horizontal and vertical velocity components, I am skeptical of the results because it's not clear that they are valid. Indeed, some results strain my physical intuition (which it is essential to note, does not mean the results are wrong and could be my own failing). For example, 600 meters of downward (vertical) displacement at a point (P7, Figure 7g) in 1.5 years seems rather extreme, even during a surge, especially for a glacier with a total flow speed of only ~1000 m/yr. How does this displacement compare with the local ice thickness and are there any observations of dramatic surface lowering of 100s of meters to validate this observation? As discussed above, I am willing to bet that there is a strong tradeoff between the inferred horizontal and vertical components that results in unrealistically large vertical displacements (i.e., horizontal displacement bleeding into the inferred vertical displacement due to the fact that the flow is close to being in line with the radar line of sight, meaning that there is not enough information in the SAR offsets to allow for accurate inferences of the 3D velocity vector). Some validation of these and other results needs to be done as does some attempt to connect the results to physical models (e.g., given mass conservation, is the horizontal flow speed consistent with the inferred vertical speeds?).

Reply: We understand the reviewer's confusion. It took us some time to comprehend this phenomenon. The flow displacement time series in Figure 10 show the cumulative displacement measured at the *surface within an Eulerian pixel. It demonstrates how surface flow has varied over time in 3 dimensions and is not indicative of the surface elevation changes.*

A good analogy is river rapids where there are areas where water flows almost vertical. There, the vertical flow velocity is fast (kms/hour) and the river depth is less than one meter (can be just a few cm for small streams). Similarly, for glaciers, it is just a matter of inflow and outflow, it has little to do with glacier thickness. We return to this in a later reply statement.

Some more specific points:

o It is hard to glean insight into the data plotted along the transects because the transects are crudely drawn and undoubtedly cross flowlines. This is likely the source of confounding results like those shown in Figure 5 (Klutlan Glacier) where the plots indicate that there are areas (~20 km, 30 km, and 36 km along the transect) that show very slow velocities (<200 m/yr) throughout the observation period but are surrounded upstream and downstream by fast-flowing regions (velocity > 1200 m/yr). A transect drawn along a flowline shouldn't show such behavior, which is the reason it's a good idea to be careful about where one draws profiles. The authors could provide a clearer view of their results by extracting data along profiles that are consistent with the dynamics of the fluid.

Reply: We agree, however, the manual extraction of flow lines is subjected to error, we improved this by using flow lines provide by OGGM software (https://docs.oggm.org/en/latest/flowlines.html). While we observe some discrepancies, we believe it is of a sufficient quality and also reproducible.

o The possibility that the authors captured a kinematic wave is truly exciting but not very convincing due to the issues discussed above and the qualitative discussion surrounding the observation. Again, it would be useful for the authors to attempt some validation of this observation and some in-depth discussion of the implications. How fast is the wave propagating? Over what distances does it attenuate? Does the wave deform as it travels? Are the amplitude and speed of the wave related to ice thickness, surface slope, or other observables? Is it really a kinematic wave or do longitudinal stress gradients matter? Is it a monochromatic wave or are there multiple frequencies? What is the period (are the periods) of the wave? Even if the authors can't sort out these details, some discussion of basic wave propagation would be valuable, if for no other reason than to point others toward the possibility of using SAR to observe traveling waves of various sorts. Again, I'll point to Riel et al (2020) as an example ofthinking about observing wave propagation with SAR. (And while I'm on this point, the authors need to provide citations to support the idea that one should expect kinematic waves to accompany surges.)

Reply: This issue was raised by multiple reviewers but it is not given a proper amount of attention in our manuscript. Since it is beyond of the original scope of the manuscript, we decided to remove the discussion about kinematic waves entirely.

Minor comments:

Title: Given the lack of details and analysis of the method (e.g., lack of synthetic tests showing the validity of the method, lack of analysis of errors and limitations of the method, etc), the title seems overly generalized. What the authors have done here is slightly modify existing methods and applied them to a particular case study. Thus, the title needs to be narrowed and case study mentioned.

Reply: We addressed all these issues in the revised manuscript.

- Abstract: Similar comment as the previous one about the title. The authors are seriously overselling the generality and novelty of their work. Other authors have published strikingly similar methods as well as markedly different methods that aim to do the same things the authors are attempting. I think the method the authors are presenting is worth publishing eventually, but it is not as novel as the authors seem to be insisting and careful rewording is warranted. Furthermore, without extensive testing of the method (which, as detailed above, has not been done in this manuscript), the argument that the proposed method is generally applicable to the global catalog of SAR data is unsupported.

Reply: We provided synthetic tests (figure above) that demonstrates that the inversion always produces unique and stable solution, therefore the technique is general. The ability to compute the individual offset maps depends on the region, data availability, and is outside of the scope of the study.

Line 1: It's unusual to refer to the components of a vector as direction and "intensity." It's more comment to reference direction and magnitude, or in the case of velocity, direction and speed. The authors could clean up the description by simply saying "Glacier velocities adjust to a warming climate…"

Reply: Corrected.

Lines 5-6: "We observe seasonal and interannual variations and the maximum horizontal and vertical flow velocity in excess of 1000 and 200 m/yr, respectfully." I don't know what to take from this sentence as there are 4 quantities mentioned (seasonal and interannual, horizontal and vertical velocities) and 5 glaciers in the case study, yet only two speeds mentioned in the sentence. These variations should be given context by comparing them with the mean flow speeds.

Reply: Corrected.

Lines 20-22: A couple of comments: 1) The sentence starts with the phrase "Modern remote sensing techniques …include…" but then lists GNSS. I would not consider GNSS to be remote sensing since the quantity being measured requires a station placed on the glacier. 2) UAV is a curious addition to this list as it is not a method for measuring surface velocity. The first three items in the list are specific techniques, whereas UAVs are merely platforms that collect various types of data, all of which could be described as examples of one of the first three items in the list.

Reply: Corrected.

Line 32: “non-tidewater” needs to be defined.

Reply: Corrected.

Line 35: The authors need to tone down their language and to make a more objective point about the need for studies in Alaska. It is not useful or interesting to say that more studies have been conducted in the ice sheets.

Reply: Corrected.

Line 40: The authors need to elucidate why they think the vertical component of velocity is important, not simply assert it. To a good approximation, ice flows along the surfaceslope, so velocity fields given in two horizontal dimensions should capture the vast majority of information about the flow.

Reply: Corrected.

Line 44: Rignot et al., 2011, is a 2D, not a 3D, map.

Reply: Corrected.

Lines 44-46: One cannot validate remotely sensed maps of velocity using MAI as it is simply another remote sensing method.

Reply: Corrected.

Section 2: It seems to me that the order of this section is backward relative to the authors’ apparent desire to introduce a general method. If I understand their motivation correctly, the method should be described and evaluated first and separately from the data being used in the case study.

Reply: Corrected. The method now is presented first.

Line 79: Give the SLC resolution of Sentinel-1 somewhere in this paragraph. Also worth mentioning that SLCs were collected from both Sentinel-1 satellites with a nominal repeat time of 6 days.

Reply: Actually, it is mainly 12 days for this region. We provided SLC resolution.

Line 84: “Such a large window size…” Why? Is it because of the flow speed or something else?

Reply: We consulted the developers of GAMMA processing software that is used to compute speckle offsets. We were advised that the window that is used to compute the offsets is not uniform, pixels in the centre have larger weights that pixels on edges. The effective resolution is about four times higher than *the window size. The process of extraction of offsets, as it is implemented in the software, is not linear. We acknowledge that the spatial resolution is reduced by using such large window. However, this is necessary for extracting the temporal information. Note that the computation of offsets, in general, is not specific in any way to the presented technique here. We also reduced the window to 128x128 pixels in the revised manuscript.*

Line 85: What do the authors mean by "distinct peak?" What do the uncertainties look like (curvature of the correlation surface, difference between peak values, etc.)? It would be useful if the authors would give a few examples of the range and azimuth offsets and associated uncertainties for the study areas in the supplement.

Reply: To address this we provided signal-to-noise ratio (SNR) maps for all deformation products in the supplementary files. We also computed average uncertainties for our range and azimuth offset maps and for computed velocities. We believe it is not worth providing examples of SNR function for particular pixels because there are so many pixels and images (~10^8), selecting a few will not be representative. However, they look like a figure below.

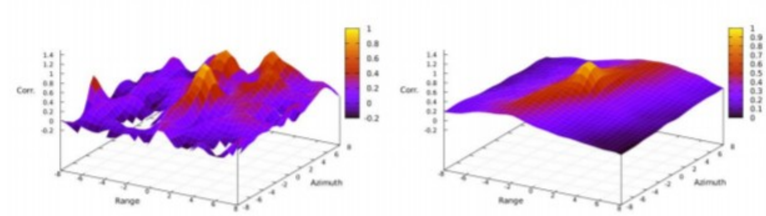


Figure S1. 2D cross-correlation plots calculated with a small window (left) and a large window (right)

Line 89: The term 'resolution' appears to be improperly used here. Resolution is a statement of information content but what the authors appear to be referring to is grid spacing.

Reply: That is correct, we now use grid spacing.

Eqs. 1 and 2: It's useful to bold variables that indicate vectors and matrices for clarity.

Reply: We rewrote these equations using capital letters for matrices.

Line 90: RO and AO should represent sets of range and azimuth offsets. (In other words, it's useful to connect RO and AO to the rho and alpha designation for individual displacements used below.)

Reply: Corrected.

Lines 90-91: What are the sizes of matrices L and A in relation to the number of observations?

Reply: This information is now provided.

Line 91: It's useful to point out that lambda is a scalar (if, in fact, it is).

Reply: It is, corrected.

Line 95: azimuth and incidence angles need to be defined for the non-SAR-expert.

Reply: Corrected. "The azimuth angle is the compass heading of the satellite, measured from the north; it discerns ascending vs descending orbits. The incidence angle is the angle between the ground normal and the look direction from the satellite; it is one of the acquisition parameters of the side-looking SAR sensor."

Eq 3: There are several differences between the variables used in the equation and those described above (e.g., upper case to lower case 's', different subscripts on rho and alpha, etc.). These need to be consistent. I don't see where the Delta t variables are defined; they need to be defined for completeness.

Reply: Corrected. Thank you for checking these fine details.

Lines 111-112: "Since this method does not make any assumptions about the direction of motion, it provides the optimal solution applicable to any phenomenon." As I mention ad nauseam above, the authors provide absolutely no support for this statement. This is merely an assertion as there is no attempt to show that this method produces accurate or optimal solutions for glacier flow or any other phenomena. Unless the authors can provide proof through rigorous testing of the method, this and other statements need to be removed or clearly qualified with statements like "we hypothesize that…"

Reply: We believe that the synthetic test does this job. Also, the technique is a basic transformation of a coordinate system.

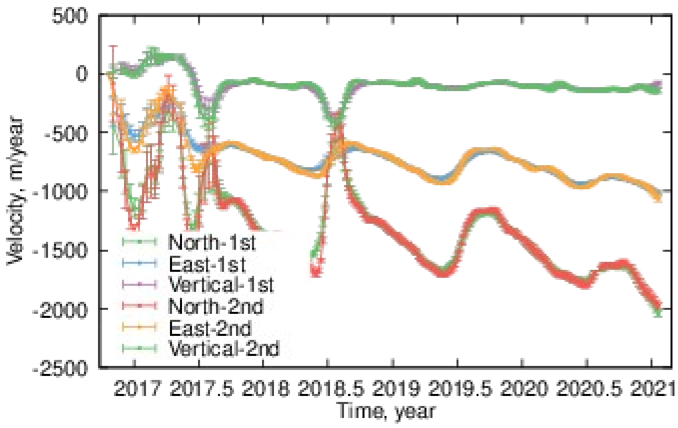
Line 113: In my reading, it seems that this is the first mention of coherence in the SAR images. I think a more expanded discussion of noise and errors is required. But at the very least, the authors should make clear to the non-expert reader what they mean by 'coherent pixel'.

Reply: We removed the word "coherent" to reduce confusion. In the previous version it was used in a broader sense meaning "of a good quality", rather than in a specific sense used in DInSAR (the magnitude of correlation coefficient).

Line 120: 'visually indistinguishable' from what? Could the authors show us some examples of the effects of regularization in the supplement.

Reply: In this case, visually indistinguishable from each other. 'Virtually indistinguishable' might be a better term, we used new phrasing in the revision. In the figure below you can see results for P2 (Figure

11b) computed using first and second order regularization. We believe it is not worth providing this figure in the supplementary files because it does not convey new information.



Line 121: Presumably the authors mean that a line was fit in time at each pixel. It is not clear if the authors are average the speed in time (keeping the unit velocity vector fixed) or each velocity component individually, or some other combination. This should be clarified.

Reply: With the technique presented here, we compute velocities between consecutive SAR acquisitions. Sentinel-1 data is acquired with either a six or 12 day revisit cycle, and velocities are computed for every revisit cycle interval (so-called instantaneous velocities). The flow displacement time series are then reconstructed from these instantaneous velocities. Assuming a 12 day Sentinel-1 revisit cycle, our technique produces 365/12 = ~30 3D velocities per year. Since all these data cannot be presented in a single publication (30 velocities per year x 3D x 4 years ~ 360 figures), as a simplified representation of our results that require only four figures, we choose to compute mean velocities by fitting a line to the flow displacement time series. Along with the mean velocities for each of the four components, we compute their standard deviations and coefficients of determination (R2), which help us understand if the linear model provides a good approximation. For some regions, a linear approximation cannot capture all the complexity of the motion. For these regions, we plot flow displacement time series, which describe instantaneous velocity at each moment in time. Annual or any other duration (monthly, quarterly) velocities can also be computed from our flow displacement time series by aggregating time series at different intervals. The linear rate of each velocity component is computed individually.

Line 124: I'm not sure what the authors are referring to here, but it sounds like the kind of statement that would be more useful in a figure caption.

Reply: Corrected.

Line 133: Does 'mean' mean time-averaged or is there some spatial averaging?

Reply: Linear model fitted to the entire time series – time-averaged, see comment above.

Line 145: Report the window size in meters; the number of pixels can be given as a parenthetical, but what matters is how large the window size is in geographic space.

Reply: Corrected.

Line 164: See above comments about unsupported claims.

Reply: Corrected.

Line 166: What do the authors mean by "with different scales?"

Reply: At different spatial scales. InSAR is limited to measuring cm-scale displacements, while SPO can measure m-scale displacements. But this statement was deleted for simplicity.

Line 168: Citation is needed.

Reply: Corrected.

Lines 168-170: Unclear what method the authors are referring to.

Reply: Corrected.

Lines 170-171: The authors state (emphasis mine) "[o]ur approach is conceptually similar to the technique of Guo et al. (2020) that was built on our previous work." As written, the (italicized) second half of this sentence comes across as petty and unprofessional. All science is built on the work of others. That is its nature and strength. The authors are duly cited by Guo et al. (2020), so phrases like the one italicized above are unnecessary. If the authors decide to keep such statements, it would be best to reword.

Reply: Our objective was to emphasize that the first version of this manuscript was submitted before Guo et al. (2020) was published. Therefore, it is not based on Guo et al. (2020) work but is similar to it. This statement was deleted.

Line 181: Again, why is it the case that large correlation windows and "strong filtering" are needed? The authors need to provide some reasoning that his related to the physical characteristics of the area of interest to support and provide insight into this statement. This is especially true given the claims of general applicability of the methods. If readers were to use this method somewhere else, is it necessarily true that large correlation windows and extensive filtering is needed? How will they know? Additionally, the term "strong filtering" needs to be quantified here.

Reply: Computation of offset maps is not a part of the method that we present. We chose to start from SLC data, but we can also start from the velocity maps produced by someone else. We provide additional guidelines for users who may want to use this technique in other regions.

Lines 181-182: The fact that surges do affect the results in this study but not those of Gardner et al. means that the comparison is useful for scientific study but not useful for comparing the results between the studies or validating the method.

Reply: There are no other 3D ice surface velocities from this region or period to compare against. Gardner et al results were provided because they provide 2D resolved velocities from this region, as recommended by the editor during submission.

Line 184: The fact that glacier flow can deviate significantly from mean flow speeds is well known and documented. This is not something that the authors have demonstrated. So, the statement needs to be reworded or removed.

Reply: Corrected.

Line 185: "SAR-derived time series are often compared with GNSS-derived time series…" Some citations are needed.

Reply: Added.

Lines 185-186: "…and both techniques are considered conceptually similar; however, there is an important difference…" This looks like a strawman argument and hearsay. The authors need to provide a citation to show that someone thinks that these methods are similar. Otherwise, they should just make their point without acting as if they are addressing some controversy that does not exist.

Reply: Added references.

Lines 185-198: I do not see why this discussion is necessary or useful and think it should be removed entirely. Eulerian and Lagrangian coordinates are well understood (as the authors point out in line 191) and well established. Converting between Eulerian and Lagrangian coordinates is also well understood. This paragraph does not add anything new to the topic.

Reply: We believe this is an important discussion, as we have interacted with other reviewers who are not as astute regarding this issue, or the cryosphere community in general.

Lines 199-210: I do not understand what the authors are trying to say here. So far as I can tell, they are trying to elucidate the distinction between cumulative displacement and instantaneous velocity. I don't see why that is necessary and think that this entire portion of the discussion should be overhauled or removed.

Reply: Removed.

Lines 204-207: I do not understand the point of this discussion and find it very confusing. It seems that the point being made is that flow direction might change in time in unconfined glaciers but probably not in glaciers whose flow is confined by bedrock. That's a pretty basic point that is not advanced by the current study. The connection the authors are trying make to ice streams is tenuous at best because again, this behavior is well known, methods have existed for quite some time to measure the horizontal flow speed and direction, and ice streams are defined as fast-flowing regions that lack strong lateral confinement from the bed topography.

Reply: We removed this text.

Lines 207-208: "Moreover, such direction changes are not easily discerned in 2D or 3D resolved velocity fields." This is a completely and utterly false statement that is both unsupported by this work and contradicted by numerous published studies. Plenty of work has shown that SAR-derived velocity unit vectors are accurate to within a couple of degrees in the horizontal (see stacks of papers by Rignot, Joughin, Rott, and other pioneers of this field). I argue that such accuracy is more than sufficient to quantify changes in flow direction.

Reply: We removed this text as well.

Lines 219-220: I don't fully understand why this interpretation is correct. In an idealized sense, SAR observations should be sensitive to the motion of the scatterers, not necessarily the surface. Indeed, it is critical when talking about SAR-derived vertical velocities to distinguish vertical velocity from changes in surface elevation. When one is discussing vertical velocities in glaciers that experience surface melt, as all glaciers in this study do, there is an additional complexity due to the dielectric influence of surface melt on the radar signal (penetration depth, or phase center). This is discussed to some extent by Rignot, Echelmeyer, and Krabill (2001) and Minchew et al. (2015). As mentioned above, some careful exploration of this component of the signal is needed here along with some qualification of these findings.

Reply: We addressed this issue in a separate manuscript. Here, we simply want to point out that the flow of ice occurs at a steeper angle than the surface topography (in the absence of structural boundaries that may force flow in that direction), therefore signifying ice loss. We rewrote this text.

Line 220: My previous comment notwithstanding, the dynamic state of Malaspina has been reported previously (e.g., Larsen et al., 2015; Muskett et al., 2003) and appropriate citations are needed.

Reply: Corrected.

Line 224: Such large changes in vertical velocity over such short distances should be manifest in the surface topography and thus should be tested against surface elevation time-series where available.

ArcticDEM would be a good place to look and would require very little effort on the part of the authors. This could go a long way to showing that the inferred vertical velocities are accurate.

Reply: Elevation data for our period is limited. Operation IceBridge data, publicly available through the NSIDC, is only current through 2012 and ArcticDEM has only a few points. However, we refer again to the analogy between ice flow and river rapids where vertical and horizontal velocities fluctuate but no change in surface elevation occurs over time, it is all about inflow vs outflow rate change. Another consideration, as our reflective surface remains at some depth the localized changes in elevation rapidly are filled with dry snow that remains transparent for SAR but is not transparent for LIDAR and LANDSAT. Such snow can be moved, for example, by the wind. There is a lot of uncertainty in what SAR-derived results represent. With our technique it is possible to see previously unseen effects of a secondary magnitude, some of them are likely due to SAR-ground interaction. These are the reasons we are cautious about providing in-depth interpretation and instead concentrate on a technique.

Line 235: The authors need to qualify such statements: "We hypothesize that the downward vertical motion…represents a kinematic wave…" More needs to be done to verify that this is what is recorded in the data (see above discussion).

Reply: As mentioned above, this issue was raised by multiple reviewers but it is not given a proper amount of attention in our manuscript. Since it is beyond of the original scope of the manuscript, we decided to remove the discussion about kinematic waves entirely.

Line 242: I'm not sure I agree with the statement that vertical and horizontal motion can only be derived from SAR. I do agree that nadir optical measurements (like Landsat) are insensitive to vertical, but I see no reason why time-dependent altimetry could not be fused into the data to give vertical and horizontal motion (again, the distinction would need to be made between vertical velocity of the ice and vertical motion of the surface). I think it's best for the authors to soften this statement, perhaps saying that SAR is currently the most viable way to infer 3D velocity.

Reply: We slightly rewrote this statement.

Line 245: As noted several times above, this qualification needs to be infused throughout the discussion section to provide a more nuanced discussion.

Reply: Corrected.

Where can readers download the software?

Reply: We will provide a link to the repository with software after the manuscript is accepted, in case changes are needed to be made. In any case the software can be accessed by contacting the first author. Mendeley Data repository does not allow modification after submission.

Where can the processed data from this study be accessed?

Reply: We will provide a link to the repository with data after the manuscript is accepted, in case changes are needed to be made. Mendeley Data repository does not allow modification after submission.

Table 1: What is the range of incidence angles in the image? The last column should be labeled as the number of SLC scenes.

Reply: Usually SAR beams are identified by the mean incidence angle but in the inversion we use precise values for each pixel. Gamma software provides two quantities for each pixel: SAR look vector elevation angle at each map pixel (lv_theta: PI/2 -> up -PI/2 -> down, the elevation angle is measured between the surface and the look vector pointing at the radar) and SAR look vector orientation angle at each map pixel, 0 -> East PI/2 -> North). These can be converted to azimuth and incidence angles and these two quantities are provided for each orbit with the data. Corrected heading.
Figure 1: Add citation for ASTER DEM.

Reply: Added.

Figure 2 caption: 'octagon' is referenced but no octagons are in the figure. Delta t_i should be defined as Delta t_i = t_{i+1} – t_i

Reply: Corrected.

Figure 3: 'Mean temporal resolution' should be clearly defined and distinguished from the repeat time of a given track.

Reply: Corrected.

Figure 4:
Fix the letter labels on the panels. Panel a is missing its letter. There are two letter b's, one of which looks like it is the label for panel c.

Reply: Corrected. Now figures 6-9.

The abbreviations for the different glaciers should be defined in the caption. o Why put the date in YYYYMMDD format just to have to explain the format in parentheses? Why not just give the name of the month and the day and year? o What is the local time of the acquisition (this matters for surface melt)?

Reply: Abbreviations - corrected. Format of date is preserved for consistency. Time, we believe, is the secondary order effect (compare to the annual cycle) and is not provided.

o Panel a (top left): Add distance markers along the profile for easy reference to the surrounding figures. Make the scale bar legible by changing colors or adding a box behind it.

Reply: Corrected. The intensity image was manipulated to display variability in a range pleasant for human eyes. It does not represent any particular units (such as dB), therefore, scale and units are not provided.

o Panel b: Are these the time-averaged velocities or for a specific time? What data set is used for the contour lines?

Reply: These are time-averaged velocities. For contour lines we used TerraSAR-x 90 m DEM – now mentioned in the caption.

o Panel c: same comment as for panel b about specifying that the velocities are time averaged.

Reply: Corrected.

o Panel d: What are the white gaps?

Reply: These were the values above the scale range. Corrected in the revised version.

Figures 5 and 6: Same comments as for Fig 4.

Reply: Corrected.

Figure 7: It would be much easier to interpret these results if the time-series were merged with Figures 4-6 and/or if the points were labeled with the letter of their respective glacier. The 'P' designation does not provide the reader with any information.

Reply: I guess we continue to use this notation for consistency because it was used in the multiple previous MSBAS-related papers. Since it is not a significant factor and it would require changing most figures, we left the labels unchanged. But we agree with the comment and will be more accurate in our future publications.

Dear Reviewer 3,

Thank you very much for providing your valuable comments that helped us to significantly improve our manuscript. Below we provide our detailed responses to your questions in italic font. Here is a quick summary of changes:

- *Addressed reviewers' comments to the best of our knowledge;*
- *Added recent Sentinel-1 data, mainly to investigate what is happening at region P1 at the Malaspina Glacier;*
- *Recomputed offset maps using smaller 128x128 window and the Gaussian filter with 1.3 km 6-sigma width;*
- *Detected another surging Kluane Glacier and analyzed it in-detail;*
- *Used OGGM software to extract flow lines and performed all analysis for selected flow lines;*
- *Simplified interpretation by removing reference to kinematic waves, which require more attention and, which possibly will be addressed in a separate publication.*
- *Provided animations for four AOIs.*

Best regards,
Sergey Samsonov, Kristy Tiampo, and Ryan Cassotto

This draft proposes a novel method for 3 -D velocity mapping of glaciers using modern spaceborne SAR measurements. Instead of using surface parallel flow constraint, this method combines speckle offset tracking and MSBAS , which is also assisted with regularization. It is further validated with Sentinel-1 data over 5 glaciers in Alaska. The draft is generally well written and the methodology is reasonable. However, there are couple of issues that need to be resolved/expanded in detail.

Major comments:

1. Study area description is better to be extracted from Section I, together with the dataset description in Section 2, to form a separate section, named "Area and Data"

Reply: We followed your advice and created a separate section "Study Area and Data".

1. The model description in Section 2 needs to be clearly rewritten and expanded in detail. If sufficient details do not fit the section, they could be added to an appendix then.

Reply: We rewrote the section "Model" entirely.

Detailed comments:

Line #13: this is the same sentence as included in the abstract, thus redundant

Reply: We rewrote the redundant sentence in the Introduction.

Line #26: SAR-based correlation algorithms not only operate on radar backscatter, but also radar backscatter and phase (complex-valued correlation).

Reply: Corrected.

Section I: you introduced multiple methods for velocity mapping (SPO, DInSAR, MAI), but did not mention what specific one you use in this work and why you chose that one. It is clear later in Section 2 that you used SPO, but would be better to motivate it in Section 1

Reply: We commented in the second paragraph of the Introduction that we use the SPO technique and in the first paragraph of the Model section explained reasons (no need for phase unwrapping, produces range and azimuth results).

Line #74: the last sentence is also the same as that included in the abstract, i.e. redundant

Reply: Corrected.

Line #83-84: the number of pixels also need to be converted to distance in m. I see you want a square sampling interval on the ground by choosing 64 x 16 for Sentinel-1 images.

Reply: This is approximately equal to 200x200m. This information is now provided in the last paragraph of Study area and Data section.

Line #84-86: why isn't the correlation window (256 x 256) a square window on the ground to be consistent with the sampling interval. Also, the numbers you chose are equivalently 1km x 4km on the ground. With the 2km wide median filter, you essentially got a spatial resolution around 2km or at least on the order of km. Even though you resampled the products into 200m, this does not justify the spatial resolution is 200m. That said, the spatial resolution is too coarse over fast-moving glaciers, and the resulting spatial pixels are strongly correlated.

*Reply: Such a large window was required to obtain a distinct, statistically-significant peak of the 2D cross-correlation function; its square shape produced similar precision in range and azimuth directions in radar coordinates, and azimuth precision four times lower than range precision in geocoded products. We found that 128x128 (as in the revised version of the manuscript) is sufficient. If we chose to reduce the number of pixels in the azimuth direction M (to make square window on the ground) we would need to increase the number of pixels in the range direction N to keep M*N=128*128, but that would affect the precision in an unpredictable way.*

In the revised version we reduced the correlation window to 128x128 pixels and used a Gaussian filter with a width to Gaussian 1.3 km (6-sigma). We recognize the benefits of having high-resolution results. Unfortunately, in this area, the application of a small window produces measurements that are too noisy, and if we only select pixels with high SNR the spatial coverage reduces to nothing. Therefore, we are limited to using a larger window.

We consulted the developers of the GAMMA processing software that is used to compute speckle offsets. We were advised that the window that is used to compute the offsets is not uniform, pixels in the centre have larger weights than those pixels on edges. The effective resolution is about four times higher than the window size. The process of the extraction of offsets, as it is implemented in the software, is not linear. We acknowledge that the spatial resolution is reduced by using such a large window. However, this is necessary for extracting temporal information. Note that the computation of offsets, in general, is not specific in any way to the technique presented here.

To confirm this we computed offsets for a single pair using 64x64, 128x128, and 256x256 correlation windows. In figure 1, below, we present these results before and after filtering. As you can see, while there are differences, overall the signal is consistent. Note that filtering does not reduce the resolution significantly. Again, we found that these processing parameters are optimal for our purposes in this region; however, it does not mean that they would be optimal in other areas.

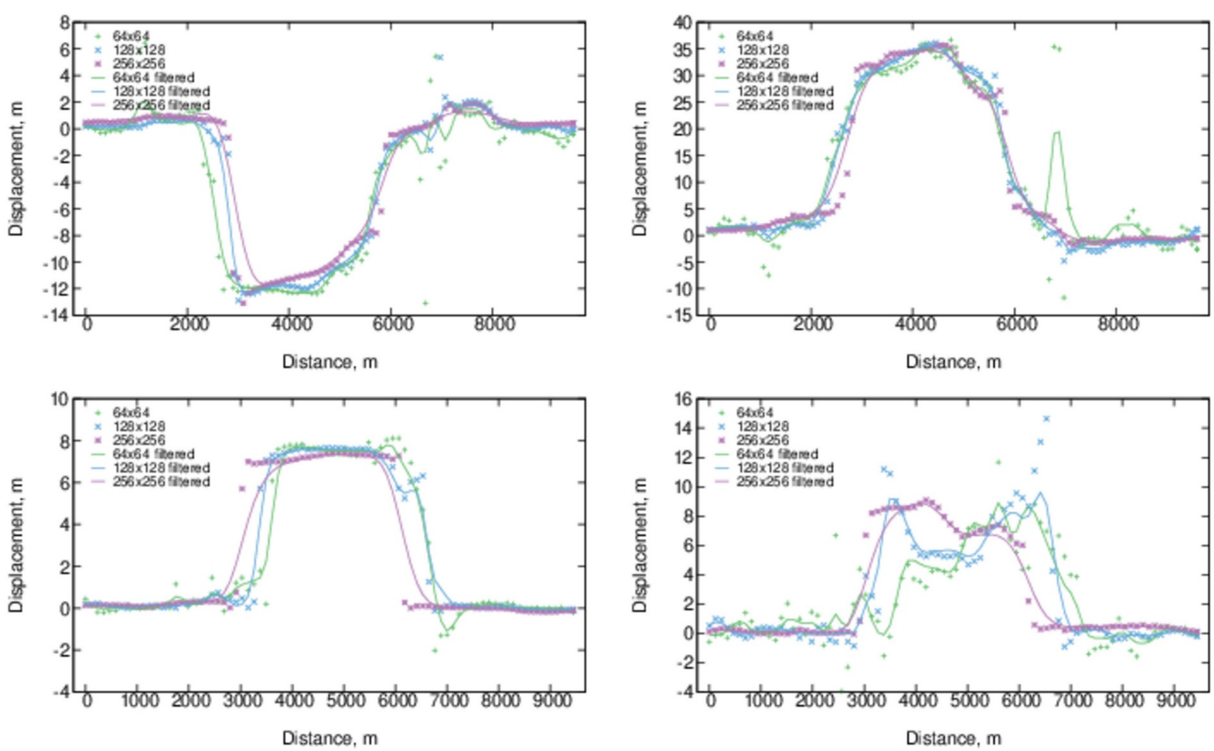


Figure 1: (top-left) Seward range, (top-right) Seward azimuth, (bottom-left) Klutlan range, (bottom-right) Klutlan azimuth.

Eq. 1: you should either cite a reference or explicitly show the proof of this equation. The way it current shows is introducing the equation out of the blue. When details of the proof is involved, you can also put that in an appendix if necessary.

*Reply: While it looks unconventional, it is a basic equation with a meaning similar to V*t =D, that we believe does not require further derivation. It is used in many SBAS and MSBAS publications and its explicit representation can be deduced from the example (equation 3). We provided clarifications about this equation in the third paragraph of the Model section. Also, the Fialko et al., 2001 paper is cited that explains in detail how azimuth and range offsets are used to solve for the 3D deformation.*

Eq. 1: the matrix/vector notation should be clearly defined by providing the dimension, which should then be related to the number of ascending/descending acquisitions.

Reply: We provided the following clarification. "In matrix A the number of columns is equal to the number of available SLC images minus 1 multiplied by three, and the number of rows is equal to the total number of range and azimuth offset maps computed from those SLC images." We also explained the size of the matrix in this particular case.

Eq. 3: this simplified example is not clear. First of all, it is not clear how the Sa and Sr components are coupled in that way. To do so, you probably need a separate graphic illustration besides Fig. 2 or an appendix. If you can find a citation that does exactly the same thing, that would work too. Second, the notation of the rho and alpha elements in the column to the right of the "=" sign were never introduced since they are different from those described in Line #96 -101. Third, the last three elements in the velocity vector only show the northing of velocity at t3 and easting/vertical of velocity at t4. Why is that and what happened to the missing other components at t3 and t4, and what happened to t5?

Reply: This comes from the geodetic analysis of seismic events and it is very well described in (Fialko et al., 2001; Bechor and Zebker, 2006), which are now referenced in our manuscript. We now explicitly show RO and AO in our simplified example (lines 85-90). Each row in A represents one range or one azimuth offset map. We believe it is now clearer.

Line #112: "any phenomenon" This is to vague. You need to be specific what type of phenomenon

Reply: We meant to say any surface motion.

Line #114: the dimension is 609 x 1014 for the matrix to be inverted. As mentiond above, how to relate these numbers to your total ascending/descending acquisitions. After Eq. 1, you should add a symbolic equation that relates the matrix dimension to the number of radar acquisitions

*Reply: After adding the most recent Sentinel-1 data to the revised version of the manuscript (we wanted to see what is happening at Malaspina Glacier at region P1) the dimensions of matrix became 666×1109. This means that we have 223 SLC images (223-1)*3=666 and 108 ascending range and azimuth offset maps and 115 descending range and azimuth offset maps = 108+108+115+115=446 and the regularization rows are (223-2)*3= 663. The total amount of rows is 446+663 = 1109. This now is explained in the Model section.*

Line #116: please report the specific computer setting and runtime for your case

Reply: For us, it takes about 24 hours of processing time on a single node with 44 cores. An Message Passing Inteface (MPI) version of msbas software has also been developed. The processing time in an MPI version is reduced proportionally to the number of nodes.

Line #117-120: add a sentence explaining why regularization is needed, and what happens if not included. Any comparison of the horizontal velocity results derived from the 3-D approach with regularization to those from the 2-D methods? Please add some simple analysis

Reply: It is a somewhat specific and complex issue from the field of linear algebra, which most users probably do not want to know unless they want to develop their own software. There are three theoretically possible cases: the number of equations is less, equal or greater than the number of unknowns. In the equal case, the matrix is square and no regularization is required. In the greater case, the least square solution is found using SVD – this is common in 1D MSBAS (more interferograms than SLCs). In the lesser case (as always in 2D and 3D MSBAS), the solution is found using the truncated-SVD, which is identical to the zeroth-order Tikhonov regularization. If we want to fill the temporal gaps, we need to apply higher order regularization (first and second-orders work equally well in this case). From the computational point of view there is no difference between the 2D and 3D problem. The need for regularization arises because SAR images from different tracks are acquired at different times, which results in more unknowns than equations, producing a rank-deficient, under-determined problem.

Line #121: what do you mean by “mean linear flow velocity” especially the word “linear”? Regarding “mean”, is the 3-year mean value meaningful for those fast-moving glacier terminus? It is expected that such glaciers should have strong seasonal/interannual changes. Probably 1-year mean value is better

Reply: With the technique presented here, we compute velocities between consecutive SAR acquisitions. Sentinel-1 data is acquired with either a six or 12 day revisit cycle, and velocities are computed for every revisit cycle interval (so-called instantaneous velocities). The flow displacement time series are then reconstructed from these instantaneous velocities. Assuming a 12 day Sentinel-1 revisit cycle, our technique produces about 365/12 = ~30 3D velocities per year. Since all these data cannot be presented in a single publication (30 velocities per year x 3D x 4 years ~ 360 figures), as a simplified representation of our results that require only three figures, we choose to compute mean velocities by fitting a line to the flow displacement time series, which we then divide by the length of our record. Along with the mean velocities for each of the four components, we compute their standard deviations and coefficients of *determination (R2), which help us understand if the linear model provides a good approximation. For some regions, a linear approximation cannot capture all the complexity of the motion. For these regions, we plot flow displacement time series, which describe instantaneous velocity at each moment in time. Annual or any other duration (monthly, quarterly) velocities can also be computed from our flow displacement time series by aggregating time series at different intervals.*

Concerning selecting the length of time to estimate mean flow, a shorter period could certainly be used; however, our aim for this manuscript was to demonstrate the technique used and the overall trends that occurred over 4 years. The flow displacement time series (particularly Figure 11) and text in the discussion address the benefits of short term analyses such as seasonal and inter-annual variability. Also four supplementary animations show instantaneous velocities for each of the studied glaciers.

Line #123: how much coarser resolution is the horizontal one resampled to? And also why is <5m/yr removed? Velocity estimates over slow-moving areas (e.g. < 15m/yr) are usually used to tie the products and calibrate the estimation bias. How did you calibrate your Sentinel-1-derived velocity products?

Reply: The resolution and masking out is performed only for improving visualization (after processing is finished), otherwise, images in the figures get oversaturated with details. We use precise orbits downloaded from the ESA website. We calibrate the offsets by fitting and removing the polynomial model. This approach works well in this region where most areas do not show any motion. The entire Sentinel-1 scene is processed as a whole, and it is cut into small sub-regions only for visualization in the manuscript. Note that the entire Sentinel-1 scene extends far beyond the area shown in the manuscript. The software provides alternative methods of calibration that can be employed in other, more complex, regions (e.g. calibration against multiple reference regions, Z-score). You can see an example of the complete data set at the original resolution in Figure 5 and in supplementary files.

Line #180: "every single range and azimuth offset maps must be coherent at every pixel" what does it exactly mean?

Reply: This means that if a pixel is incoherent on one of the offset maps (e.g. 20190201-20190213) it will be excluded from the processing and all results will have NaN value at that pixel. This approach ensures we used only the highest quality results. In general, our processing software can handle partially incoherent pixels (it will be filled by the regularization); however, in this study, we choose to utilize only pixels coherent in all offset maps so their precision is identical. The technique that utilizes partially coherent pixels will be discussed in the follow-up publications.

Line #181: "large correlation window followed by strong filtering" gives you much lower resolution and spatially correlated pixels. Isn't that problematic for fast-moving glacier terminus? Please comment and justify.

Reply: That is correct. However, it is a necessity to use a large window and filtering as processing with a low correlation window produces very noisy results in this region. This has already been discussed above.

In the revised version we use smaller window and a filter with the Gaussian window, we found that it performs better for small and large glaciers. Finally, with the exception of a handful of tidewater glaciers (Hubbard, Tsaa, Guyot, and Taan), the majority of glaciers in our study area are land terminating and thus do not experience the rapid flow that typifies tidewater glacier termini.