Dear Reviewer,

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.

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 are prepared to address 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, in which sources of errors are well understood. We will strengthen the discussion of errors and explain how these errors propagate to the 3D decomposed results. Note that while individual offset maps have large errors, the derived mean velocities have significantly smaller errors. Which 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. Other-wise 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 agree that surface parallel displacement is expected to dominate along mountain glaciers with steep slopes, much like the flow patterns observed along Seward Glacier (Fig 4b). However, although bed elevations along the piedmont lobe are unknown, they are not expected to be steeply dipping, so surface parallel flow here is unlikely to be dominant. Nonetheless, our original statement requires an additional qualifier. We will amend it to read: "Low magnitude horizontal velocities combined with a predominately downward flow throughout Malaspina Glacier's massive lobe (Figure 4b,c) indicates…." Also, we have differentiated between *surface parallel flow (SPF) and non-SPF in another paper that was recently accepted. We will provide a reference to the manuscript that describes this work.*

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. Furthermore, the overall trend in elevation change over these four years (10's meters, Fig 7) exceeds the maximum 10 m depth penetration measured in dry firn in this area in earlier studies (e.g. Rignot et al., 2001); thus, errors due to seasonal variations in depth penetration should be small relative to the true signal. We will discuss this in the revised manuscript as something to watch out for during the interpretation of results.

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: Actually, most of the glaciers at lower elevations show similar behavior (hence this point is chosen as a characteristic). This motion is caused by glacier melting during summer. 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 will address this comment in the revised manuscript.

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: 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. Likely this happens because of the warm regional climate and wet glacier surface (in comparison to Greenland or the Arctic or the Antarctic). Therefore, we are limited to using a larger window of256x256 pixels.

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 center 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 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. We will add this figure to the revised manuscript. Note that filtering does not reduce the resolution significantly. Again, we found that these processing parameters are optimal for our purposes in this region; it does not mean that they would be optimal in other areas.

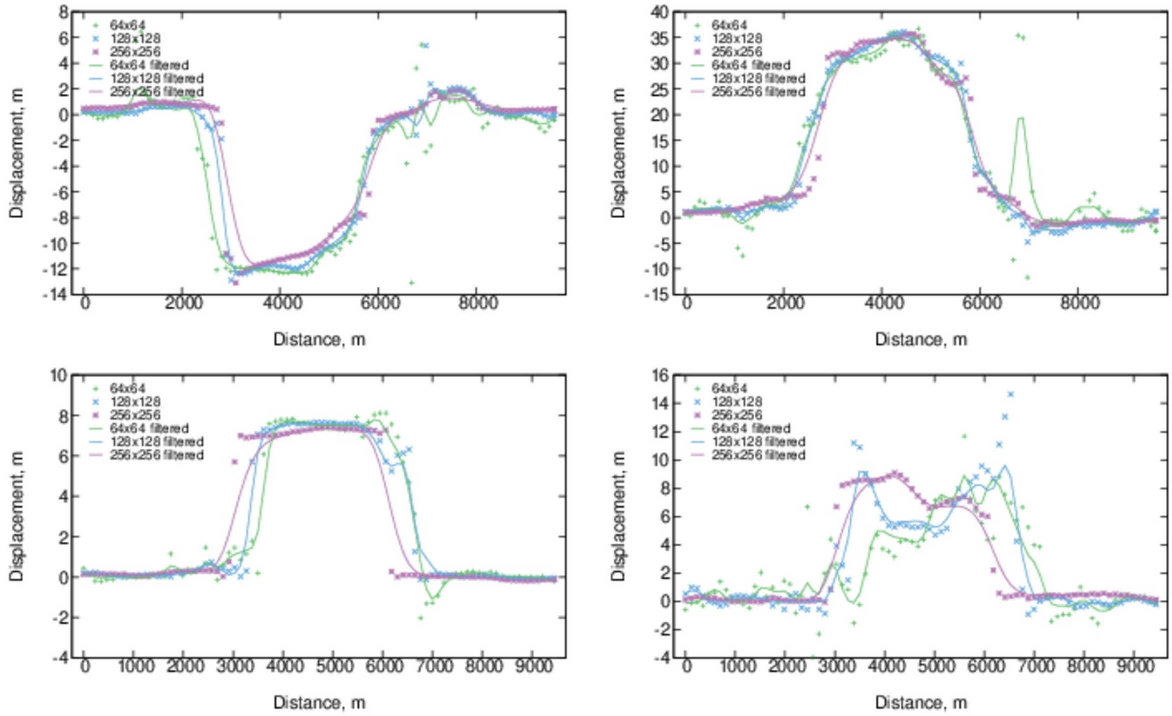

Figure 1: (top-left) Seward range, (top-right) Seward azimuth, (bottom-left) Klutlan range, (bottom-right) Klutlan azimuth.

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: Will do.

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. It has been previously 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 has already been done. We will provide a reference to the manuscript that describes this work.

Figures. The x-axis of the vector profile plots could be lined up with the color times se-ries 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: Will do.

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: Will do. The alternative patches can be due to profiles not well aligned with flow lines. In the revised manuscript we will utilize an OGGM software (https://docs.oggm.org) for selecting profiles along flow lines.