Dear Dr. MacGregor

Thank you very much for the opportunity to revise our manuscript. We successfully addressed all the comments. Below, please find our response to your comments in italic font.

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

Editor Decision: Publish subject to revisions (further review by editor and referees) (10 Jun 2021) by Joseph MacGregor
Comments to the Author:
Dear Dr. Samsonov et al.,

I've now received and reviewed three thorough reports from referees concerning your revised MS on a new derivation of 3-D glacier flow in Alaska from SAR from the same three referees. This is unusual and indicates the broad interest in the topic you've presented. However, all three indicate that major revisions are required, and no referee felt that any element of the manuscript could yet be rated excellent, so further revision is clearly still needed.
Reply: We also feel that the reviewers' reports are especially thorough. We are very thankful to the reviewers for the effort and time.

I agree that the manuscript is currently a bit dissonant with unusual complexity in the SAR methods and relatively little detailed discussion of the sometimes large signals observed.
Reply: We disagree to some extent about the complexity of the technique because all the equations are presented explicitly and can be programmed directly from the manuscript. The real challenge was the huge amount of data and various programming challenges that we successfully solved (for example, the matrices are huge). Our original intention was to introduce the software and let the community use it, we did not want to go into the details, because from the theoretical point of view the problem is very simple, it is the computational aspects that are very complicated. Nevertheless, we followed the reviewer's guidance and revised accordingly.

While reviewer #1 suggest breaking out the manuscript so that the glacier signals can be explored more fully, I leave it to the authors' discretion as to how to proceed.
Reply: The strength of our technique is in the ability to produce regional maps (again, the amount of processing is huge). While we cover a lot of glaciers, we focused our study on only four glaciers, three of which experienced surge behavior during our observation period. Because of the added benefit of observing vertical flow from 3D SAR observations of surging glaciers, we believe it makes sense to present all of them in the same manuscript.

Please make a clear decision as to whether to highlight some of the unusually large signals observed and explain their possible physical origins more, or clarify whether they may be due to uncertainty in the method and require further future investigation.
Reply: We do not observe unusually large signals. For example, in Figures S7-S14, we compared our SAR-derived results with the Landsat-derived (from ITS_LIVE) results during 2017 and 2018. Both results are in a good agreement and the Landsat-derived results most of the time show slightly larger velocities, which we explained (Landsat data is acquired mainly during the sunny (summer) days when the surface flow is above the average rate). The largest velocity that we observe here is about 3000 m/year or less than 10 m/day. These are very typical maximum velocities for glaciers.

A particular concern of referees #2 and #3 is the lack of a formal uncertainty analysis, which I agree is required for this relatively new combination of method + application.
Reply: All velocities are now provided with standard deviations (Figures S1-S3) and the coefficients of determination R2, Figures (S4-S6). Previously this information was not shown in the manuscript but was included in the supplementary files (to be submitted to the data repository). You can also see error bars in time series (Figures 11-12). To the best of our knowledge, it is very unusual to have an analytical representation of the uncertainty analysis in numerical computations. We have not seen anything like this for SBAS-derived manuscripts, and, even if it was available it would not be of any benefit, because the precision of the input data is the same for all pixels, the transform is also the same, therefore, the output data will also have the same precision. Clearly, one precision value for all pixels does not provide much value. Instead, we use statistical analysis to estimate precision. The average and maximum values of standard deviations are also reported in the discussion section. Additionally, we use two independent datasets - from ascending and descending orbits, which cross-validate each other. We also provide a more detailed analysis in the synthetic data section and believe that the resulting discussion is sufficient. We now also compare our velocities ts with the Landsat-derived velocities (Figures S7-18) during 2017, 2018 and the entire period.

This also dovetails into reviewer #1's concern indicating that a simple smoothing might be just as good as the regularization.
Reply: As noted in our reply, the truncated SVD method is the standard method employed for inversion of DInSAR pixels with uneven time intervals today, and has been so for many years, because of the ill-posed nature of the problem combined with the very large date set. To the best of our knowledge regularization is the only available mathematical technique for solving ill-posed problems (Truncated SVD is a particular case of regularization).

For these and other concerns, I expect them all to be addressed fully in a revised version, and I note that reviewer #2 indicated that some of their earlier comments were not fully addressed.
Reply: We double-checked that all comments are addressed.

Figures 6-9 are overall quite effective, but I would like to see P1-9 labeled in the lower panels. In Figures 10 and 11, please indicate which glacier/distance P1-9 are associated with (e.g., Malaspina terminal lobe for P1). As they stand, the signals shown are quite complex and need some context without having to refer back to previous figures.

Reply: We made these changes in the revised manuscript.

Regards,
Joe MacGregor
NASA/GSFC

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

I feel this paper still has a lot of issues.

In particular, the discussion of the glacier behavior is pretty shallow. There are lots of things going on in the data, but the discussion is very weak. Some of it would really benefit from trying to decompose the surface parallel and surface elevation change signals. I have made some comments, but really it just needs some more work to express the main points better.

Reply: The strength of our manuscript is in presenting a new processing technique applied to a new region that produces new and interesting results. We agree that data shows so many interesting signals, which only emphasizes the strength of this technique. However, a detailed description of these signal is beyond the scope of this manuscript. Some of them will be discussed in separate studies that are underway (led by graduate students).

The interesting signals in the data sets are exactly the impression we want to make with our manuscript. Processing SAR data for studying just one or a few small glaciers is how it was done in the past. With the global coverage provided by Sentinel-1 (and forthcoming NISAR), increased availability of inexpensive processing power (e.g. clusters), and software that can handle the processing of these large data sets we can produce deformation products on a regional or even continental scale. Here, we are simply trying to introduce the technique and the results produced on such large spatial scales.

With no context, a lot of the data look wonky. For example, a 200-m displacement of the surface at P9 is a fairly fantastic signal that warrants a lot more explanation. Perhaps not if it's a surge bulge. But how about finding some independent confirmation (Arctic DEM for example).

Reply: As mentioned in the discussion, many of the glaciers in our study area (Kluane, Klutlan, Walsh) were, in fact, surging during part of our observation period (the Sentinel-1 record). And we provided citations from earlier studies that verified the onset of surge activity from 2D flow velocity records using sensors that precede Sentinel-1 measurements. We also searched the ICESat-2, Operation IceBridge (OIB), and ArcticDEM records for surface elevation measurements coincident with surging activity. Unfortunately, records of surface elevation change do not exist during the interesting surge activity as it either precedes the launch of ICESat-2, are not coincident with OIB flight operations, or were not acquired by commercial satellites that form the basis of ArcticDEM time-tagged strip data. Therefore, comparison with surface elevation data from the time periods of *active surge activity is not possible due to a lack of data. In Figures S7-S14, we compared our SAR-derived results with the Landsat-derived (from ITS_LIVE) results during 2017 and 2018. Both results are in a good agreement and the Landsat-derived results most of the time show slightly larger velocities, which we explained (Landsat data is acquired mainly during the sunny (summer) days when the surface flow is above the average rate). The largest velocity that we observe here is about 3000 m/year or less than 10 m/day. These are very typical maximum velocities for glaciers.*

Really this would be a better paper if it stuck to results from one (maybe two glaciers) and explained those well. Then the remaining results could be turned into a second more science rather than technique-focused paper.

Reply: Here the focus is on the design and presentation of the advanced processing techniques. We believe that this is where our contribution to the science community is the strongest.

As a point of curiosity, I really wouldn't mind seeing a discussion of how the results would be superior to taking the staggered ascending/descending offset and interpolating them to common times, and then applying the basic (e.g., Gray) equations at those times to get a solution. Either way, the velocity solution at a point in time is just a linear combination of the offsets that went into it. With no regularization, I didn't go through all of the math, but my intuition says the results shouldn't different greatly (or even be identical) to a simple 2-pt linear interpolator (or perhaps some higher-order interpolation). And it computationally, this latter approach would be far faster. A simple smoothing filter could be applied in place of the regularization. This could be coded up in an afternoon and compared with the results.

Reply: There are a number of issues with implementing offset interpolation to common times approach, below are just a few of them:

- *Measurements contain error – interpolating noisy data is far from obvious;*
- *Some or many measurements can be missed – interpolating over variable time periods (gaps) is far from obvious;*
- *Some reversible signals (e.g. a shift in trend) can be captured by only some data sets – interpolating data that does not capture reversible signals will miss those signals;*
- *Most importantly, the interpolation doubles the amount of input data so the inversion will actually take twice longer. For example, if you have 100 SAR images from one orbit and another 100 SAR images from another orbit, you will get 99 range and azimuth offset maps from each orbit. If you decide to interpolate to the intermediate times you get 199 range and azimuth offset maps from each orbit. That is twice the amount of data. The number of rows in the transform matrix doubles, which is computationally more expensive.*

If one decides to approach this problem by writing equations that govern this process one will end up with the equations that we provided. Then, one can try to solve these equations using unconventional computational methods, e.g. testing various interpolation and smoothing methods, or one can use the conventional, well-established mathematical approach for solving under-determined and over-determined problems, which requires computation of the SVD.

In case of interpolation, how would you choose the optimal parameters from the magnitude of possibilities (e.g. interpolation length, smoothing filters)? It is fast to perform one interpolation, what about interpolating it 250 time periods x four data sets x 10,000,000 pixels ~ 10^10 (10 billion) times? Then you still have to solve the inverse problem with twice the number of rows (<u>which takes as much time as SVD in our case</u>) and then still perform filtering. In addition, truncated SVD is the standard method employed for DInSAR time series inversion today, and has been so for many years, because of the ill-posed nature of the problem combined with the very large date set. While investigation of modifications of the inversion method and interpolation with advanced processing techniques might prove worthwhile in the future, it is outside the scope of this work.

I have made some specific points

Specific Points: Line numbers refer to the marked-up version.

Abstract: “no single technique” I disagree with this statement. Laurence Gray published such ability - 1.Gray, L. Using multiple RADARSAT InSAR pairs to estimate a full three-dimensional solution for glacial ice movement. Geophys Res Lett 38, n/a-n/a (2011). One can certainly argue that this paper improves on the technique, but times series of estimates can be derived using the method he developed. The method presented here improves a bit on lining up things in time, but only to the extent that built-in assumptions about the rate of change apply. I think a fairer statement for the abstract would be “We have improved upon earlier methods to measure the evolution of surface flow …”

Reply: We modified the abstract as recommended.

Abstract: The abstract makes it sounds a bit like 30 years of data have been ignored for want of this technique. There are massive gaps in the record, which is why the measurements have not been made. Please tweak the text to not oversell the available data.

Reply: We corrected an abstract by removing “nearly 30 years of”.

Line 27 “m-scale” is ambiguous as the way it's worded sounds like it's referring to the horizontal scale. Change to “comprise displacement measurements sub-meter to meter-scale precision with … and cm-scale precision with …”. (sensors like TSX do considerably better than m-scale).

Reply: Corrected.

Line 71. How about inserting “open source” before “software” then delete “It is provided to the ….”

Reply: Corrected.

Line 132 remove “compass” as it implies magnetic north when I believe true north is meant. “satellite heading” would also save some words.

Reply: Corrected.

Line 133 “ground normal” not clear if you mean with respect to the DEM or the ellipsoid. Especially since it's said to be a sensor parameter, it sounds like the ellipsoid. Please be clear about what is meant as it’s a rather critical distinction.

Reply: Corrected, replaced with “nadir”.

Equation 3. Please check V column its – not clear why V3 for N, but V4 for e & v.

Reply: Thank you, these were grammatical errors and they are now corrected.

Line 169: given you present a method in Equation 3 with a constraint. Could you be clear about what you mean by “unconstrained”

Reply: Equation 3 uses a numerical constraint (slowly changing velocities). However, we refer to the geometrical constraint – such as “surface-parallel flow” studied in our previous manuscripts. This has been *specified in the text.*

Line 200: “not limited by the acquisition geometry”. The accuracy certainly does depend on the geometry (for example if the angle between ascending and descending is 1 deg vs 30). Please clarify.

Reply: We clarified by saying that we refer to the actual Sentinel-1 geometry. “the Sentinel-1 suboptimal acquisition geometry”.

Line 245 “The magnitude” not “A magnitude”

Reply: Corrected.

Line 316. How does 1/10 t 1/30 of pixel precision translate into 4 m for an ~14 m azimuth pixel.

Reply: The 1/10 to 1/30 of pixel is the reported actual measurement precision. In our study we are interested in ground displacements of glaciers. Therefore, the background motion outside of glaciers is considered as noise, e.g. snow drift, landslides, etc. Presence of this background motion contributes to the larger error. Perhaps if we were interested in studying snow drift outside of glaciers (which is irregular in time and space) or similar process we would claim a better precision, close to 1/10 to 1/30 of pixel. We added the following text “Our precision is lower than reported in \citep{strozzi2002} because we intentionally interpret the motion outside of glaciers (e.g. irregular snowdrift, landslides) as noise.”

Line 320-325. It’s a bit unclear about what the various values are (eg. Maximum values 21, 18.. – maximum of what).

Reply: For each pixel on the map during the 2017-2021 period, we compute a linear velocity and its standard deviation (Figures S1-S3) using the linear regression technique. Therefore, for each pixel and each component of the velocity, we get a standard deviation. To report these values in the manuscript we compute the average standard deviation for all pixels. Additionally, we provide the largest standard deviation among all pixels in the map - this shows the worst-case scenario, the largest error.

Line 326. This comparison with GNSS is very unclear and for it to be at all true, it needs a lot of qualification (how GNSS with mm to cm/day can compare with SAR and 4 m over 12 days).

Reply: Conceptually it is the same. Linear velocity is a slope of displacement time series. The precision of a computed slope depends on the number of measurements in the time series when each observation is affected by noise. It is equally true for GNSS and SAR-derived displacement time series. If the reviewer wants, we can remove the reference to GNSS without any loss of clarity in the manuscript; however, we believe the interpretation of this statement is straightforward and unit independent.

Line 335. Please be a bit more clear about the Gardner product – are we looking at the 3-decade+ average of all the available LS?

Reply: In the previous version we indeed used the 3+ decade averaged velocities. In the current version we additionally use Landsat-derived velocities computed during 2017 and 2018 (Figures S7-S14). This now is explained in the Discussion.

Line 355. The definition of Eulerian/Lagrangian is not quite right (https://en.wikipedia.org/wiki/Lagrangian_and_Eulerian_specification_of_the_flow_field). And you really risk offending a lot of SE people who know the distinction.

Reply: According to the referenced website: "The Eulerian specification of the flow field is a way of looking at fluid motion that focuses on specific locations in the space through which the fluid flows as time passes. This can be visualized by sitting on the bank of a river and watching the water pass the fixed location." and "the Lagrangian specification of the flow field is a way of looking at fluid motion where the observer follows an individual fluid parcel as it moves through space and time. Plotting the position of an individual parcel through time gives the pathline of the parcel. This can be visualized as sitting in a boat and drifting down a river."
This is exactly in agreement with what we say. The Eulerian specification is used in SAR measurements, where location a SAR pixel is a location of the observer "on the side of the river". The Lagrangian specification is used in GNSS where we measure "the pathline" of the receiver. We could address this comment in more detail if we better understood the reviewer's question.

Line 380: "downward flow in the lower ablation" What matters is whether the flow is downward after accounting for the surface parallel flow component.

Reply: That is correct, we added "with the slop steeper than surface topography".

Line 385. To make statements about emergence velocity you have to account for surface parallel flow (i.e., the residual after subtracting surface parallel flow).

Reply: That is correct. However, even without accounting for surface parallel flow here, the observation of downward flow having a greater slope than the surface topography implies emergent velocities are significantly less that what it expected and in a balanced glacier system.

Figure 2. The caption needs further explanation. A lot of the other captions are a bit too terse.

Reply: We modified the captions in Figures 2 and 3. We think that other captions are self-explanatory.

Dear Reviewer,

Thank you very much for providing your valuable comments (in red) that helped us to significantly improve our manuscript. Below we provide our detailed responses to your questions for consistency in red italic font. Text in black is from the first round of review.

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

We thank the authors for their response and revising the manuscript according to our comments.

1. There are still some parts of the manuscript (such as the equations/notations in Section 2) that are confusing or misleading.

2. Also, for some responses, the authors only replied to us but did not reflect any changes in the manuscript which may still be misleading or confusing to other readers. Please be sure to both address the reviewers' comments and reflect (even if the comment may sound simple) any possible changes in the manuscript.

3. The revised manuscript still lacks a section on formal error/accuracy analysis (by comparing to other reference velocity measurements) and a discussion section about current limitations and how to improve.

Below we attached the authors' response and only added highlighted comments to those questions that were not fully addressed.

Reply: We addressed these comments in detail below.

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".

2. 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.

The equations/notations in Section 2 have been rewritten and also additional references have been added. It looks a bit better however, there are still places that look confusing or unclear.

For example,

1. please rewrite the 1st sentence of 4th paragraph in Section 2 to use notation of M x N to represent the dimension of matrix A. You could explain what M or N means using the number of unknowns/observables, e.g. number of SLC images.

   Reply: Corrected as requested. "In the transform matrix A with N columns and M rows..."

2. You did not answer our comment why the velocity vector in Eq. 3 only included V_n3, V_e4, V_v4.

   Reply: Corrected the index, it should have been V_n3, V_e3, V_v3.

3. The RO and AO vector definitions using rho and alpha elements need a vector transpose as they are column vectors.

Reply: We use “{}” symbology to indicate “set” rather than “vector”. Then a set can be converted to a row or column vector depending on the need. When we want to be more precise, we use “()” to indicate matrix (and vector is a matrix).

4. Define right after Eq. 3 what those vector/matrix mean and note clearly the dimension using the above-mentioned number of unknowns/observables.

   Reply: This is now provided.

5. You now added the total dimension of 666 x 1109 in Section 2. However, 666 actually corresponds to the column dimension and 1109 the row dimension, which is opposite to the convention of the using row x column. Please reverse the order unless there is a reason for it. Also, you need to put your response to our comment about how these numbers are calculated based on the number of unknowns/observables into the main text.

   Reply: Reversed as suggested. Provided explanation on how these numbers are calculated. “Thus, the total number of azimuth and range offset maps M equals 446...”

6. In Fig. 1, and also the simplified example of Section 2, you have 3+4=7 SLC images, so according to the statement (1st sentence of 4th paragraph in Section 2), the number of columns should be (7-1)*3=18, which is not equal to the actual number (12) in Eq. 3.

   Reply: The number of SLC images is computed after applying the boundary correction. It is now clarified in the text. “Note that the boundary correction reduces the number of SLC images by two...”

7. Please also put your response to our comment about regularization into the text.

   Reply: Added. “The need for regularization arises...”.

8. It also seems that the actual value of the regularization parameter, lambda, does not matter. Because it got cancelled out in each regularization equation, where there are only two nonzero terms (both terms have lambda’s that will be cancelled out) and all zero values for the other terms. Not sure why your reported value of 0.1 matters.

   Reply: This is a standard methodology used for solving ill-posed inverse problem (see e.g. Tikhonov et. al., Numerical Methods for the Solution of Ill-Posed Problems). In general, we are looking for a solution that minimizes changes in the velocity between consecutive epochs (first objective) while fitting our input data (second objective). Lambda in this case is a parameter that weighs a contribution between the first and second objectives. Large lambda emphasizes the first objective, which produces smooth solution. Lambda can be looked at as a low-pass filter strength. The selection of optimal lambda is done as part of the inversion process.

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).

We only found one sentence in Section 2 and did not see that you chose to use SPO in Section 1.

Reply: Corrected by adding to introduction "We use in this study the SPO technique because it produces deformation maps in range and azimuth directions that do not require phase unwrapping."

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.

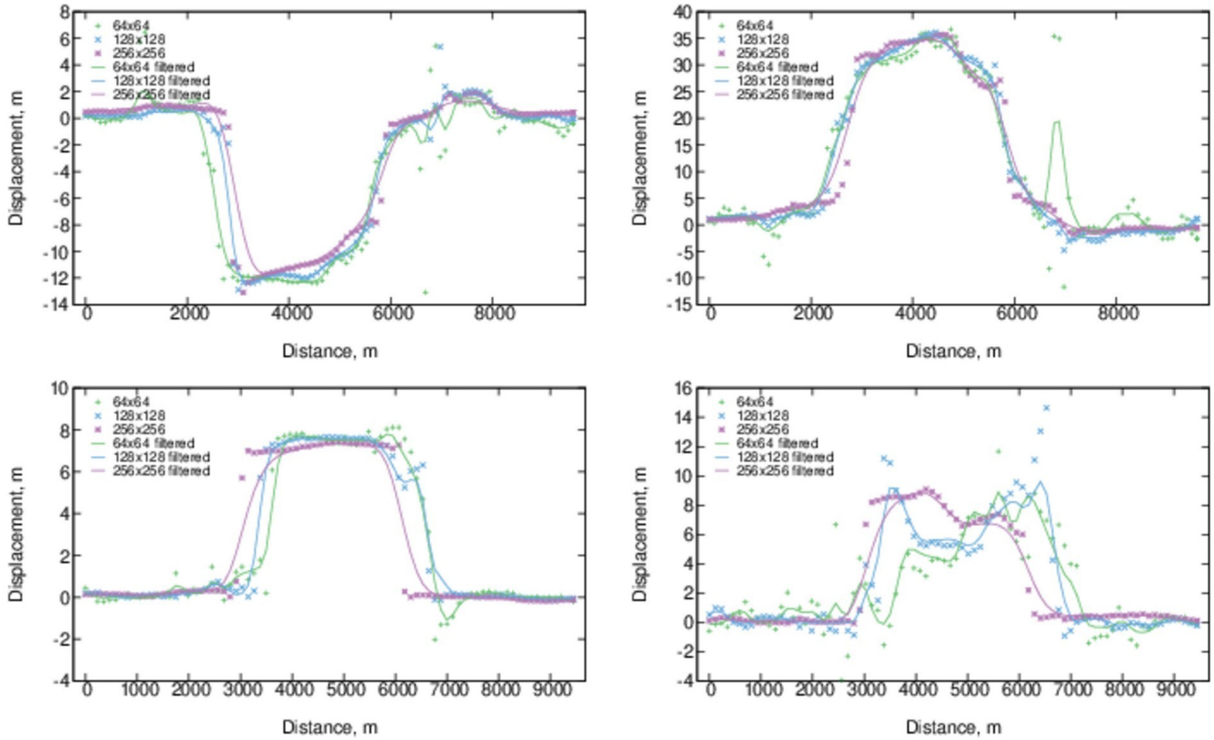

Figure 1: (top-left) Seward range, (top-right) Seward azimuth, (bottom-left) Klutlan range, (bottomright) Klutlan azimuth.

1. Based on what you clarified, using a large window might be okay for your area. But using a filter with larger width (km) is not recommended. Are you saying you replaced the previous median filter (2km width) with a Gaussian one (1.3km 6-sigma)? If so, 3-sigma Gaussian is roughly 650m, which might be okay but still a bit large.
2. From above figure (bottom right), it seems using your new window of 128 x 128 with filtering gives quite different results compared to 64 x 64 without filtering. So the question arises: the large window might be insufficient for this area and also the filter width might be too coarse.
3. You probably want to mention this as a limitation of the current processing and discuss how to improve the results in the future.

Reply: We agree. We added the following text to the discussion: "One of the practical computational challenges of the SPO technique is the selection of pixels, which offsets are computed with high confidence. After multiple tests, we determined that the SNR function works very well but only when the search window is large. However, such a large window applied to the medium resolution SAR data limits the spatial resolution of the results. It is possible to use high-resolution SAR data and the 128$\times$28 pixels search window to overcome this limitation and achieve a high spatial resolution of results; however, such SAR data is not yet readily available on a global scale. The utilization of high-resolution SAR data also allows using a spatial filter with large, in terms of pixels, window size."

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.

Please refer to our above comments on rewriting this sentence and also the problem of applying this sentence in calculating the dimension for the particular case.

Reply: This comment has been addressed and commented above. The entire modelling section was rewritten.

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.

As mentioned above, you did not answer our comment why the velocity vector in Eq. 3 only included V_n3, V_e4, V_v4. What about the other missing terms at t3, t4, t5? Also, as mentioned above, please denote number of unknowns/observables (e.g. number of SLC images as N) and use N to express each vector/matrix dimension right after Eq. 3. This is pretty standard way of introducing vector/matrix notation in writing scientific articles.

Reply: This comment has been addressed and commented above. The entire modelling section was rewritten.

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.

Please reflect that change not only in the current response but also in the revised manuscript, otherwise it is still confusing to others.

Reply: Corrected.

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.*

As mentioned above, you need to move your response to the revised text as well. Once you define number of unknowns/observables as N or M as suggested above (e.g. number of SLC images as N), it is pretty straightforward to make this calculation by substituting N=223.

Reply: This comment has been addressed and commented above. The entire modelling section was rewritten.

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, underdetermined problem.*

Even though only some readers might be interested in this topic, you still need to include it in the text to be complete. Also it is not trivial and widely used in the literature on ice velocity mapping.

Reply: We added this information to text as requested.

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.

It is now clear to us. However, it is strongly recommended to rename the term "mean linear flow velocity". Alternatively, you should add a few more sentences from the above response to the main text otherwise, the readers might still feel confused and thought it was a statistical averaging mean value.

Reply: This information is now provided in the model section. "For simplicity of presentation, a linear trend is computed by applying linear regression to the derived values in such a way as to illustrate the 3D displacement time series and three linear rate maps are used for visualizing the results. Note, that in the case of …"

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.

Please objectively report your above calibration approach and clearly state that this is a limitation of the current processing chain in the main text. It seems too empirical and will be problematic for fast moving glacier areas. We would like to see some validation of the velocity results by comparing to other reference velocity measurements with some accuracy or error analysis, which is completely missing in this work.

Reply: The need for calibration has nothing to do with our approach; it is a requirement for any SAR-derived deformation product (InSAR and SPO). The travel time of the SAR pulse depends on the precise location of the satellite and the state of the atmosphere. Because the state of the atmosphere changes and the position of a satellite is known imprecisely (and then imprecise coregistration and many other factors), the travel time difference between the first and second acquisitions (which is the deformation product after some manipulation) is determined with very low accuracy. But the measurement precision is very high. There is, of course, a fundamental difference between accuracy and precision, and in the SAR case, it manifests in such a way that all measurements are determined up to a constant. The process of determining this constant value is called calibration. Only in certain cases, a calibration process is difficult (e.g. large earthquakes can produce deformation across the entire image). In our case, ice flows along small (compare to the entire image) valleys, all we need to do is to measure offset in areas where there is no ice flow, from millions of pixels laying outside of glacier valleys we need to determine just one calibration constant. This can be done very accurately. We would be happy to compare our results to any other results. However, to the best of our knowledge, such data does not exist. All results used in this study are provided with confidence intervals, standard deviations, and coefficients of determination. In the revised version we also provide correlation and covariance matrices for synthetic tests. Most of the observed signals have been previously reported in cited papers.

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.

It is the term "coherent" that sounds confusing to us. Please define the coherence you are referring to here or use another word to convey the exact idea.

Reply: Coherent pixel is a pixel in which displacement is determined with high-confidence and which is retained for further processing. It is now corrected in the text.

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.

As mentioned above, although it seems to work for your area (note it is not convincing without a formal error analysis), you should explicitly add this as a limitation of the current processing routine, and explain how to improve it in the future for fast glacier outlets. The current revision of the manuscript still lacks a formal discussion about current limitations and how to improve.

Reply: We believe we now addressed this comment in detail in discussion section ("One of the practical computational challenges..."). This is not a limitation of our technique but of SPO technique in general, our technique can be applied to data with any spatial resolution equally well. The proposed solution is to use SAR data acquired with higher spatial resolution. At the moment, such data is available only for some regions and it is not free.

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 in Alaska and Yukon using SARderived 3D time series of glacier surface flow" by Samsonov, Tiampo, and Cassotto

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 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 an interesting and useful method and focus on an area where glaciers are exhibiting fascinating dynamic behavior. As such, this work will likely be of interest to the TC readership.

This revised version of the manuscript is much improved from the original version. The authors added more context and details for their methods, attempted a minimal synthetic test to show that the method works, and improved the presentation and discussion of their results. The tone and precision of the writing in this draft better positions the work in the broader context of remote-sensing methodology and scientific knowledge. Overall, I enjoyed reading this draft and think that it is moving toward being suitable for publication, though I have some comments below that may be useful to consider.

- I think the authors need to do more to distinguish the methodology presented in this work and that of Guo et al., 2020. The authors merely mention Guo et al. in the introduction and say that they are presenting here an 'independently developed version of the algorithm' (line 46) in this manuscript. Some discussion of the differences between this algorithm and Guo et al. are needed as the current wording suggests that the authors developed an identical algorithm. If the algorithms are identical, the authors need to say so, otherwise they risk confusion for readers looking to implement or further develop the methods. In line 46, the authors point out that their software contains options to call other methods (published elsewhere) and Tikonov regularization schemes, but this is irrelevant to the distinction in the methods and algorithms. If there are no significant differences between these methods and those of Guo et al., 2020, it would seem that the presented method is not 'novel' (as stated in lines 119 and 326), and such statements should be removed.

Reply: As you can see, the Guo et al., 2020 manuscript does not provide details about their methodology. We, however, noticed that their methodology applies weights in the transforms matrix based on pixel spacing (which is not justified in the text but given as a fact). Our technique does not require such weights. In any case, it is not critical for us to call the technique novel, so we removed "novel" from this manuscript and commented about the weights.

- The synthetic tests appear a bit perfunctory and are certainly not as generally applicable as the authors' language suggests. At the very least, the sythetics need to be explained more thoroughly and in greater detail. I have a few comments:
    - One of the challenges of capturing the signals that the authors attempt to capture is that the three components of the velocity vector defined in an east-north-up (or other geographically referenced coordinate system) are not independent of one another. Rather, one would expect that the components of the velocity vector covary as they are representing the 3D flow of a glacier that is responding to some combination of internal and external forcing. So, while it's an interesting exercise to evaluate whether the method is capable of inferring time-varying signals in different velocity components that are unrelated to one another (i.e., different periods of variability), it's not a realistic test of a method meant to be applied to the natural environment. In other words, it's not that hard to infer different components when their time-varying functions are orthogonal. The challenge is in separating variability in the individual components when it is the speed (magnitude of the velocity vector) of the glacier that is varying with some given frequency and amplitude. Such a test has not been conducted and needs to be to show that the method works.

Reply: As requested, we performed an additional synthetic test where north, east and vertical components of displacement are {2f(t), f(t), 0.5f(t)}, where f(t) is a combination of harmonic and linear functions (Figs 3 d-f). As you can see the reconstruction of this signal is very good.

The numerical analysis of covariance matrix is only meaningful when the actual variables are independent, as in our previous orthogonal tests. We previously showed that covariance terms for orthogonal tests are equal to zero (this can be seen from time series figures – reconstruction is very good).

When all three components of the displacement vector change coherently by design the covariance terms are naturally not equal to zero. What valuable information can be extracted from the covariance matrix in this case? In that case it is only meaningful to analyze the correlation matrix. It is expected that in this case all terms should be equal to 1. We present these covariance and correlation results in Table 2 and also discuss in the text.

    - The covariance matrix (mentioned on lines 140 and 144) is never defined in the paper and needs to be if it's to be discussed. This is especially true given the authors finding that geometry doesn't matter in their method based on the covariance terms taking on a value of zero. This is a surprising finding that contradicts decades of work in GPS positioning and other work in inferring multi-dimensional surface velocity fields from SAR data (as referenced in a previous review), so a little more discussion would be useful as would an explanation for why it is the authors' method doesn't suffer the same challenges as well-established methods that attempt to do essentially the same thing.
    - Lines 142-143: Why would the rank be 3 if you have ascending and descending range and azimuth offsets? These represent 4 unique viewing geometries, so one would expect the rank to be 4.

Reply: Our covariance matrix is defined in a standard way as a square matrix that gives the covariance between each pair of elements of a given random vector. It is symmetric and positive semi-definite and its *main diagonal contains variances (i.e., the covariance of each element with itself). The random vector in our case consists of three components of velocities measured at each epoch.*

As you can see in Table 2, the covariance terms for uncorrelated input signal increase with the magnitude of the noise. However, they still remain small at the current noise level. This can be seen in our results as well. Each map pixel is processed independently in time yet the displacement (or velocity) field appears correlated (consistent velocity over all glaciers in all three components).

In MxN matrix the maximum possible rank is min(M,N), in our case the matrix is 3x4 (i.e. three unknown components of velocity and four equations), therefore the maximum possible rank is 3. The problem is over-determined and the solution is found in the least-square sense.

- The authors' mention of the tensor rank seems to indicate a misunderstanding of the point of geometric influence and the role of the covariance matrix. The rank simply shows that there are 4 unique viewing geometries, which is enough information to invert for 3D velocity field. That is obvious and has never been in question. Rather, the question lies in the accuracy and precision of the inferred components. In other words, given a finite signal-to-noise ratio, is there enough information to constrain the 3D velocity vector components in time? The authors have shown that the answer is maybe, but the physical contrivance of their synthetic tests (as noted in my comment above starting with 'One of the challenges…') leaves the question open. The rank of matrix A can only provide a negative answer to this question as it is merely a necessary (not sufficient) condition for inferring the information that the authors purport to infer.

Reply: In general, the number of nonzero singular values of matrix equals the rank of the matrix. The conditioning value measures how much the output value of the function can change for a small change in the input argument, it is equal to the largest singular value divided by the smallest singular value. So, when the smallest singular value is very small comparing to the largest singular value the conditioning number is large and the numerical problem is unstable (i.e. sensitive to the noise). In our case the conditioning number is small so the solution is stable.

- Further to my point about the covariance matrix, one of the major shortcomings of the methodology presented here is the lack of any formal uncertainties. What I mean is that it's possible to compute the uncertainties in the offset fields, and in a methodology as developed as MSBAS, these uncertainties should be carried through to some formal error estimate for the resulting velocity fields, and this is where imperfect observational geometries (as is virtually always the case with satellite observations due to the nonorthogonal angles between orbits) will amplify errors. Thus, spatial variability, as quantified here by the authors, is better than nothing, but not as good as formal UQ.

Reply: All velocities are now provided with standard deviations (Figures S1-S3) and the coefficients of determination R2, Figures (S4-S6). Previously this information was not shown in the manuscript but was included in the supplementary files (to be submitted to the data repository). You can also see error bars in time series (Figures 11-12). We agree that it would be great to have an equation that measures uncertainty in a formal way. Note, however, that since the precision of input data (offsets) can be considered equal for all pixels because it only depends on sensor parameters then the precision of computed variables (3D velocities) is also constant. Intuitively, there is not much value in a single *precision number. Practically, the spatial variability is the only currently available methodology for SBAS-derived methods.*

Minor comments:

- I still fail to see the value in the discussion contained in the paragraph beginning in line 275 as the distinction between Eulerian and Lagrangian coordinates is well established, but perhaps the authors are aware of some related controversy that needs to be addressed. The wording in this manuscript (line 281) suggests that no such controversy exists for the TC readership (and I know of now such issues), so I still contend that this should be removed from the paper. That said, the authors are clearly intent on making this well-known point, and I won't bring it up again. I'll simply end by saying that if the authors are intent on making this point, they should at least say something about the conversion between Eulerian and Lagrangian coordinates.

Reply: We do not mind repeating this, perhaps well-known point, because to us, geophysicists mainly working with small deformation of rigid objects this is an unfamiliar concept, although it is much better understood in the cryospheric field. We commented that the material derivative can serve as a link between Eulerian and Lagrangian descriptions of continuum deformation.

- A few sentences in this manuscript are identical to those found in Samsonov et al. 2021 (Remote Sensing of the Environment). This isn't a major issue as these are minor sentences that give context, but a bit of editing will avoid the appearance of copy-paste between two published works. This comment should not be taken by the editor or anyone else as an ethical issue, merely a logistical detail as the published manuscript was probably written around the same time as this one and it's easy for these things to happen.

Reply: We believe that after this revision this issue is resolved.