Review of “Measuring the state and temporal evolution of glaciers in Alaska and Yukon using SAR-derived 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.
- 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.
    - 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.
- 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.
- 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 non-orthogonal 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.

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.
- 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.