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.

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:
    - 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.
    - 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.

- 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.
- 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.
- 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.
- 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.

  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.

- 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?). Some more specific points:
    - 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.
    - 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 of

thinking 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.)

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.
- 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.
- 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…"
- 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.
- 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.
- Line 32: "non-tidewater" needs to be defined.
- 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.
- 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 surface

slope, so velocity fields given in two horizontal dimensions should capture the vast majority of information about the flow.

- Line 44: Rignot et al., 2011, is a 2D, not a 3D, map.
- Lines 44-46: One cannot validate remotely sensed maps of velocity using MAI as it is simply another remote sensing method.
- 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.
- 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.
- Line 84: "Such a large window size…" Why? Is it because of the flow speed or something else?
- 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.
- 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.
- Eqs. 1 and 2: It's useful to bold variables that indicate vectors and matrices for clarity.
- 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.)
- Lines 90-91: What are the sizes of matrices L and A in relation to the number of observations?
- Line 91: It's useful to point out that lambda is a scalar (if, in fact, it is).
- Line 95: azimuth and incidence angles need to be defined for the non-SAR-expert.
- 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.
- 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…"
- 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'.

- Line 120: 'visually indistinguishable' from what? Could the authors show us some examples of the effects of regularization in the supplement.
- 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.
- 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.
- Line 133: Does 'mean' mean time-averaged or is there some spatial averaging?
- 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.
- Line 164: See above comments about unsupported claims.
- Line 166: What do the authors mean by "with different scales?"
- Line 168: Citation is needed.
- Lines 168-170: Unclear what method the authors are referring to.
- 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.
- 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.
- 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.
- 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.
- Line 185: "SAR-derived time series are often compared with GNSS-derived time series…" Some citations are needed.
- 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.

- 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.
- 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.
- 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.
- 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.
- 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.
- 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.
- 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.

- 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).
- 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.
- Line 245: As noted several times above, this qualification needs to be infused throughout the discussion section to provide a more nuanced discussion.
- Where can readers download the software?
- Where can the processed data from this study be accessed?
- Table 1: What is the range of incidence angles in the image? The last column should be labeled as the number of SLC scenes.
- Figure 1: Add citation for ASTER DEM.
- 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
- Figure 3: 'Mean temporal resolution' should be clearly defined and distinguished from the repeat time of a given track.
- 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.
    - The abbreviations for the different glaciers should be defined in the caption.
    - 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?
    - What is the local time of the acquisition (this matters for surface melt)?
    - 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.
    - Panel b: Are these the time-averaged velocities or for a specific time? What data set is used for the contour lines?
    - Panel c: same comment as for panel b about specifying that the velocities are time averaged.
    - Panel d: What are the white gaps?
- Figures 5 and 6: Same comments as for Fig 4.
- 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.