Response to reviewers for "Observing traveling waves in glaciers with remote sensing: New flexible time series methods and application to Sermeq Kujalleq (Jakobshavn Isbræ), Greenland" by Riel et al.

1 Summary

We thank the reviewers for their constructive feedback and very useful suggestions. We provide a point-by-point response to their comments below. Our responses are in blue, and any additional or modified text in the manuscript is provided in quotations. Note that while the line numbers in the reviewer comments correspond to the original manuscript, the line numbers in our responses correspond to the modified manuscript. Also note that we have added two supplemental figures to address some specific comments by both reviewers.

2 Response to Reviewer 1

Comment 1. I would like more description of how the B-splines are constructed in the methods section (lines 101-116). I am not an expert on this approach to time-series analysis and the description provided may be enough for someone with a deeper background. But, for the non-experts, I suggest adding a couple of sentences to explain, in plain language, that there are a set of seasonal B-splines (with period of 1 year) and a set of transient B-splines (with period of < 1 year) that are simultaneously being fit to the observed velocities, if that is indeed the case. This would then connect nicely with the paragraph describing how the data is detrended on lines 129-137. This is my interpretation and, without looking at the tutorial code, I am not sure that I completely understand how the B-splines are constructed. For example, are the seasonal B-splines fit to data that falls within a window of 1 year? This is the kind of thing that would clarify for me, the non-expert, how this approach works.

We modified the paragraph to read:

"In this study, we use a combination of third-order B-splines and time-integrated B-splines (B^i-splines) to populate the columns of $\mathbf{G}$ (Hetland et al., 2012; Riel et al., 2014). Third-order B-splines are suitable for modeling seasonal signals with potential year-to-year variations in amplitude, as is observed in the ice surface velocity and elevation at Jakobshavn Isbræ (Joughin et al., 2010, 2018). To that end, we construct B-splines with effective durations (full-width at half maximum) of three months, spaced 0.2 years apart such that the center times of the B-splines repeat each year. This choice of timescale and spacing allows for reconstruction of complex, sub-annual behavior in the time series data. On the other hand, time-integrated B-splines, which exhibit slow-step behavior at particular timescales (similar to the sigmoid function), are useful for modeling *transient* variations. In this work, we define transient signals as any signal that is non-steady and non-periodic, which encompasses both rapid transients (e.g., speedup following a calving event) and longer-term transients (e.g., multi-year increases in velocity due to long-term changes in air temperatures). This spectrum of behavior can be comprehensively reconstructed through a combination of B^i-splines of different timescales and onset times. For the Jakobshavn Isbræ data analyzed here, we target only longer-term transient signals by including B^i-splines with durations > 1 year in $\mathbf{G}$. Notationally, the partitioning of the design matrix can be represented as $\mathbf{G} = [\mathbf{G}_S,\ \mathbf{G}_T]$ where $\mathbf{G}_S \in \mathbb{R}^{M \times N_S}$ is the submatrix containing N_S B-splines for modeling seasonal signals and $\mathbf{G}_T \in \mathbb{R}^{M \times N_T}$ contains N_T B^i-splines for modeling transient signals. The regularized least squares approach in Equation 2 thus simultaneously estimates the coefficients for each submatrix such that $\hat{\mathbf{m}} = [\hat{\mathbf{m}}_S;\ \hat{\mathbf{m}}_T]$ where $\hat{\mathbf{m}}_S \in \mathbb{R}^{N_S \times 1}$ and $\hat{\mathbf{m}}_T \in \mathbb{R}^{N_T \times 1}$. Simultaneous estimation of seasonal and transient signals allows for underlying tradeoffs between the two signal classes to be maximally resolved by the full timespan of the time series."

Comment 2. At times, I found it a bit confusing tracking what velocity quantity was being discussed (i.e., seasonal velocity, long-term velocity, etc.). I would like to suggest two ways to address this. First, I suggest adding to the methods section (probably to line 137) a statement such as, "Throughout the paper, reference to seasonal velocity represents the quantity d_s and references to long-term velocity represents the quantity ..." Second, I ask the authors to carefully go through the entire manuscript to ensure that all references to "seasonal velocity" and "long-term velocity" do, in fact, refer to these exact quantities. I appreciate that the authors have likely already done this and I commend them for their writing, which is already for the most part very clear. I only ask that a final pass is done through the text before the resubmission to double-check the references to these different quantities of velocity.

We have added the suggested sentence (at the suggested location): "Throughout this paper, references to short-term, seasonal velocity variations refer to $\hat{\mathbf{d}}_S$ while references to longer-term, multi-annual velocity variations refer to $\hat{\mathbf{d}}_T$."

Please note that in the paragraph shown in the response to the previous comment, we added a definition of transient signals to explicitly state that we are targeting longer-term, multi-annual transients in this work. Additionally, after going through the manuscript, we have made the following minor modifications to remind the reader which velocity quantity is under discussion:

Line 243: added $\hat{\mathbf{d}}_S$ and $\hat{\mathbf{d}}_T$

Line 312: added $\hat{\mathbf{d}}_T$

Line 324: changed "transient" to "multi-annual"

Line 409: added "multi-annual ice speeds"

Comment 3. Along the lines of future work, I would like the authors to add some brief discussion on the use of in-situ measurements to measure the propagation of waves. For example, can terrestrial radar or laser scanners be used to provide high-temporal-resolution measurements that can help further constrain glacier waves? This can be added to the end of Section 5.2.

We added the following sentences to (now) Section 5.3:

"A similar constraint may be obtained from terrestrial radar instruments that record velocity variations at timescales of minutes, allowing for high-resolution observations of dynamic responses to calving events or mélange collapse (e.g. Xie et al., 2019; Cassotto et al., 2019). In those situations, temporal basis functions and spatial correlations between basis functions can be used for dictionary construction and time-series inversions."

Comment 4. In a few parts of the discussion, it is stated that velocity and surface elevation are responding to changes in calving front position but this causality is not shown by the results of the paper. It is shown definitively that variations initiate at the terminus and propagate upstream and that these variations are well-correlated with terminus motion. However, causality (in one direction or the other) is not shown by the analysis here. The language surrounding this discussion should be revisited and revised. Perhaps I have missed something and this causality can be inferred but, in that case, it needs to be made more explicit and clear in the discussion. Otherwise, the causality wording should be changed to discussing the correlation between terminus position and velocity.

While several prior studies have demonstrated causality between calving front position and velocity and elevation variations, we agree that our results as presented do not show causality on their own.

We modified the first paragraph in the Discussion to read:

"Decomposition of the time-dependent velocity and surface elevation fields into distinct temporal scales reveals a repeating pattern on Jakobshavn where velocity and surface elevation variations originate at the terminus. The coincidence between speedup and slowdown of the glacier with thinning and thickening, respectively, suggests a dynamic origin to the physical mechanism generating these variations. Prior studies have proposed that this mechanism is primarily characterized by a reduction of back stress at the terminus following a series of calving events, causing ice acceleration and increased driving stresses to propagate upstream which results in the observed high correlation between calving front position and velocity variations (e.g., Nick et al., 2009; Joughin et al., 2012; Bondzio et al., 2017)."

In the first sentence of Section 5.1.1, we removed "in response to changing calving front position".

Line 506: changed "perturbing forces" to "proposed perturbing forces".

In the first sentence of the Conclusion, we modified the following sentences (added text in bold):

"Over Jakobshavn Isbræ, this decomposition permitted a detailed investigation into the spatiotemporal characteristics of the evolving seasonal cycle of ice speedup and slowdown **which are shown to be highly correlated to seasonal terminus variations**. Analogously, longer-term changes in velocity were isolated and **also highly correlated with** longer-term terminus variations."

Comment 5. Finally, a minor comment that applies throughout the paper. There are a couple of places where the "southern bend" of the glacier is mentioned and I suggest adding something that indicates this region to all of the map-view figures.

We added an annotation for the southern bed in Figures 4 and 5 which show the phase delays for the short- and long-term velocity variations. However, we decided to omit the annotation for Figures 1 and 6 since the southern bend itself is not a central aspect of our results and is not closely related to the features shown on those maps.

Minor comments:

[line 50] What are sub-epoch velocity changes?

"Sub-epoch" is meant to imply temporal interpolation of the data between observation times by the estimated, continuous time series model. We changed the sentence to read "...while also allowing for interpolation of velocity changes between observation times throughout the glacier".

[line 137] This sentence can be removed.

We removed the sentence.

[Fig 1] State in the caption that the map coordinates are polar stereographic north (EPSG:3413).

Added to caption.

[Fig 1] I suggest replacing the manually drawn white lines in panel A with either the calculated contours of maximum shear strain rate or with the contour where the bed is at sea level. This would be a more accurate depiction of the trough and the main trunk of the glacier.

We have replaced the drawn white lines with a zero-meter-elevation contour of the bed and captions.

[Fig 1] The sentence "Mean velocities are added to time series for visual clarity" does not make sense to me. How are the mean velocities depicted in the plots? And what are these means (spatial? temporal?)?

We modified this sentence to read: "For each time-series, mean velocities for 2009 – 2019 have been added as offsets for visual clarity."

[Fig 1] Clarify how the data is detrended in C and D. For example, something like: "white dots indicate (B) observed speeds, (C) observed speeds detrended using seasonal splines, and (D) observed speeds detrended using seasonal and transient splines."

We added the following sentences to the caption: "The detrended short-term observations in C are the observed speeds minus the estimated integrated B-splines. The detrended long-term observations in D are the observed speeds minus the estimated seasonal B-splines."

[Fig 2] The approximate solid black lines drawn are very helpful in illustrating wave propagation and it is clear from the differences in panels A and B that phase speeds of seasonal signals are much different from multi-year signals. However, I would like to see calculated contours drawn on each panel. These could be the zero contours or any other arbitrary value and they can be displayed in grey, with the approximate lines in darker black for illustrative purposes.

We added zero-velocity-contours for the leading edges of the events (summer speedups for the seasonal velocity variations and 2012 speedup and 2017 slowdown for the multi-annual variations). We felt this approach was a good compromise between showcasing the data and limiting visual clutter.

[line 269] I am confused by the phrase "long-term signals removed." Is removing the long-term signal the same as combining the transient and seasonal signals? In other words: $d_L = d - d_T - d_S$, where d_L is the long-term signal shown in Fig. 1C $d - d_L = d_T + d_S$ If this is the case, I suggest rewording this from "the velocity data from 2011 to 2018 at each pixel with the estimated long-term signals removed, d_S" to "the combined seasonal and transient modeled signal, $d_T + d_S$"

"Removing the long-term signal" means removing the transient signals from the full model fit. In this paper, long-term == multi-annual == transient, as described in the modified Section 2 and shown in Fig. 1D. Therefore, we are using Equation 7 to model the time-series in Fig. 1C (as stated in the text). We believe that our modifications to the main comments (1) and (2) provide sufficient clarity for the reader to follow which time-series are being discussed.

[line 300] I would replace "classical" with "time-series".

Done.

[line 314] This is the only place in the paper where Fig 3C is referenced and I think it is completely OK to hypothesize about the connection between phase velocity and thickness/bed but, because a figure is provided, I would suggest expanding on this a bit. Please add a sentence that explicitly states the hypothesis about the relationship between these two variables (e.g., higher/lower velocity in thicker/thinner ice).

We added the following sentence: "For glaciers where ice flow is dominated by basal sliding, phase velocity is expected to scale with the square root of ice thickness and basal shear traction (Rosier et al., 2014), which is roughly consistent with the increase in phase velocity and ice thickness around 8 km upstream, although more work is needed to establish concrete connections."

[lines 332-334] This sentence is accurate but the "while" clause does not make sense to me. I am reading it as "along the trunk there is lowering, while on the slower ice, there is lowering." Please clarify. Perhaps this sentence is meant to say that there is a confined region of high thinning along the trunk and near the front, while on the slower ice there is still thinning but lower magnitude.

We changed the sentence to read: "Within the main trunk of the glacier, we observe a clear association between the 2012 speedup and lowering of the ice surface due to dynamic thinning, whereas on the slower ice, thinning is more diffuse and occurs at a lower rate."

[lines 334-336] This sentence makes two claims without providing evidence. First, that the slower ice was thinning before the observation period. Second, that high melt started in 2009. Both of these must be backed up with either a figure or a citation.

After more discussion, we decided to remove references to high melt prior to the observation period since thinning of the inland areas in the 2000s for Jakobshavn is more likely a dynamic response to the speedup in the main trunk in 2004 (following disintegration of the ice tongue). We have modified this sentence to read:

"A comparison of time series for points on and off the glacier (Figure S1) suggest that much of the ice in the surrounding areas has been lowering since before the observation period. In these areas, thinning has been attributed to inland diffusion of steepening surface slopes following speedup and thinning of the fast-flowing trunk in 2004 in response to disintegration of the ice tongue (Krabill et al., 2004; Joughin et al., 2008)."

[line 360] I would find it helpful to distinguish the results presented in this paper from earlier work here. Adding a clause to this sentence such as, "Consistent with earlier work ..., but at a higher-temporal resolution, we observe ..." or "Consistent with earlier work ..., but using our novel method that is better able to isolate seasonal signals, we observe ..." [lines 360-361] This paragraph and the corresponding figure describes the relation- ships between (1) seasonal terminus positions and seasonal velocity variations and (2) long-term terminus positions and long-term velocity variations. Thus, I suggest re- wording this sentence from "we observe a strong correlation between the seasonal variations in ice velocity and the year to year variations of the front" to "we observe strong correlations between variations in ice velocity and variations of the front at both the seasonal and long-term time scales"

We modified the sentence to read:

Consistent with earlier work (e.g. Joughin et al., 2012; Cassotto et al., 2019) but using our method to decompose velocity time-series into short- and long-term variations, we observe strong correlations between variations in ice velocity and variations of the front at both seasonal and long-term time scales.

[Fig 7] I suggest using a sequential colorscale, rather than a divergent one, to represent different years. The current colorscale makes it impossible to distinguish 2009 from 2018.

We chose the cyclical color scale to emphasize the different seasonal behaviors between the two different clusters of years, rather than emphasizing any one year specifically. However, we updated the figure to use dashed lines for 2009/2010 to make it easier for the reader to distinguish between individual years (as suggested by Reviewer 2).

[Fig 7] In panel D, in addition to coloring the points according to year, distinguish the two groups separated by terminus position using different symbols (e.g., circles and squares).

We have modified the figure and caption accordingly (diamonds and circles).

[Fig 7] In panel D, it is not clear to me how the seasonal velocity variation quantity is calculated. Please add this to the text or the caption.

Here, we are simply using the reconstructed seasonal velocities, $\hat{\mathbf{d}}_S$. We have modified the sentence introducing panel D as: "D) Correlation between terminus position and seasonal velocity (i.e., $\hat{\mathbf{d}}_S$) at the same location as in (A) and (B)".

[lines 392-395] Strictly speaking, the results do not show that velocity and surface elevation variations are changing in response to changes in the calving front position. They are certainly correlated but causality one way or another has not been shown here. I suggest rewording this.

Please see response to main comment 4.

[line 410] I would add the word "transient": "... as well as the transient response ..."

Done.

[lines 410-411] This sentence is a bit confusing to me. What is meant by quantifying "wave propagation to phase velocities and attenuation length scales?" Does this mean quantifying the relationship between wave propagation distance(?) to phase velocities and attenuation length scales? Something seems to be missing here.

This sentence was meant to convey that we cannot fully quantify wave propagation behavior (e.g., dispersion relations, modification of waveform shape with upstream distance, etc.) with the data resolution we have. We can only quantify the scalar quantities of phase velocity and attenuation length scale. We changed the sentence and the following sentence to read:

"At the moment, data sparsity only allows for quantification of phase velocities and attenuation length scales for describing overall wave propagation behavior. As more data become available, the time series methods outlined above should allow for observations of waveforms manifest in surface elevation fields and broader and more refined constraints on the functional form of dispersion relations (the relationship between frequency and wavelength) for individual glaciers."

[lines 412-413] Please reword to be more explicit about what is meant by "broader and more refined constraints". Does broader mean for more glaciers or at more frequencies? Does more refined mean smaller uncertainties? If so, state this explicitly.

We modified the second half of that paragraph to read (added text in bold):

" As more data become available, the time series methods outlined above should allow for observations of waveforms manifest in surface elevation fields **in addition to constraints on dispersion relations (the relationship between frequency and wavelength) on individual glaciers that cover a broader range of frequencies with finer resolution in the frequency domain**. Realizing this potential for remote sensing time series is important because the characteristics of wave propagation, specifically the dispersion relation as defined for a wide range of frequencies, are intrinsic properties of dynamical systems, if we define the system in this case such that it includes the glacier and boundary conditions. **As such, time-dependent velocity and elevation data for glaciers characterized by a wide range of sliding speeds and geometries can be used to determine the relative contributions of forcing frequency, ice thickness, glacier width, and basal traction on measured phase velocities attenuation lengthscales, thereby providing a method for inferring relevant mechanical and rheological parameters.**"

[line 489] Can anything more be added about the kinds of waves that are observed on Rutford? Did previous work categorize what kind of waves those are? If so, I would add that here to enhance the contrast between the kinematic waves on Jakobshavn and the other type of wave on Rutford.

We are currently working on a theoretical framework for relating wave propagation at sub-annual timescales to intrinsic physical properties of laterally-confined glaciers and ice streams. To our knowledge, no previous work exists that fully characterizes wave and dispersion behavior at these timescales. Nevertheless, the mechanisms proposed for driving velocity variations at Rutford all share the feature of dynamic redistribution of longitudinal stresses (and possibly combined with basal drag reduction through subglacial hydrology).

We re-organized the paragraph a bit and added a sentence that explains the above comment in more detail. The relevant portions are as follows (added text in bold):

"On Rutford — with a mean flow speed near the grounding line of approximately 375 m/year — velocity variations driven by ocean tides propagate upstream with a phase velocity of approximately 24 km/day for the first 40 km upstream of the grounding line, and then at a faster rate of 34.3 km/day further upstream. Thus, observed waves on Rutford propagate two orders of magnitude faster than those we observe on Jakobshavn. The attenuation length scales are also markedly different: approximately 45 km on Rutford versus 7 km (seasonal) and 14 km (multi-annual) on Jakobshavn. Several observational and modeling studies have suggested that at Rutford, downstream variations in buttressing stresses over the ice shelf, grounding line position, and/or water pressure at the bed (Gudmundsson, 2006, 2007; Rosier et al., 2014, 2015; Minchew et al., 2017; Robel et al., 2017; Rosier and Gudmundsson, 2020) drive the variations in flow speed over the tidal cycle. The marked differences in forcing frequencies and propagation speeds and distances between Rutford and Jakobshavn therefore suggest fundamental differences in wave types and forcing mechanisms. **Indeed, while previous work cited above suggests that waves on Rutford are influenced by the viscoelastic properties of ice expected at fortnightly periods, the much longer periods of variability on Jakobshavn render elasticity of glacier ice negligible and thus unlikely to contribute in any meaningful way to wave propagation. We further discuss the distinctions between wave types below.**"

[line 597] "IceSat-2" should be changed to "ICESat-2"

Done.

[line 631] Please add a sentence describing, briefly, the caveats to the conclusion that the observed waves are kinematic in nature. These caveats are very nicely discussed in detail in Section 5.1 and I think they need to be summarized in the conclusion.

We added the following sentence: "However, the dispersive nature and higher phase velocities of the observed waves relative to previously proposed kinematic waves necessitates further investigation into their physical drivers and the overall dynamic response of glaciers to stress and mass perturbations."

[Data availability] Please add the DOI for the OMG DEMs (`https://doi.org/10.5067/OMGEV-GLNA1`)

Done.

3 Response to Reviewer 2

Comment 1. My main criticism is that it was difficult to follow why and how different time periods were used for various analysis throughout the paper. I would like to see either more coherence in selected time periods used, or more description up front in the introduction/motivation to explain why various subsets of the time period are used at different points throughout the text. For example, the abstract and introduction refer to a 2009- 2019 decadal study, shown in completeness Figure 1. However, Figure 2 then only shows 2011-2018, and subsequent analysis average similarly segmented time periods. As another example, mean time of peak seasonal velocity and phase velocities seem to be computed using only 2011-2018 velocity data.

As a general note, our analysis strategy was to focus on the years where the seasonal velocity variations were similar with regards to amplitude and upstream propagation characteristics. The years 2009-2010 were somewhat anomalous in this regard (especially 2010), mainly in terms of timing of the seasonal speedups. Therefore, for computing and presenting the seasonal phase delay (e.g., Figure 3), it was important to not introduce bias into the phase estimation from the years where the seasonal cycle was shifted within the year. In Section 4.1, we added the following sentences to motivate these decisions:

Line 277: “...occurring around mid-September. The exception to this timing is the 2010 speedup which starts earlier in the year and may have been driven by a combination of warmer air temperatures and cooler ocean temperatures influencing mélange rigidity during the course of the seasonal cycle (Joughin et al., 2020).”

Line 294: “Note that the years 2009-2010 are excluded in order to avoid introducing biases into the phase estimation from differences in onset times of summer speedups.”

However, we do see the value in visualizing the entire time span of the data in order to clearly see the change in short-term velocity variations after 2012. We have modified Figure 2 to include the entire time span (please also refer to our response to Reviewer 1 where we have added zero-velocity contours to the images). From the updated Figure 2, we can now see that while the summer speedup in 2010 has an earlier onset time than the subsequent years, the phase velocities for 2009-2010 are quite similar to the other years.

Comment 2. Reference point used for correlation analysis: Why is a point 1.4 km upstream of the pinning point used specifically for comparison to velocity? I see that you found the highest temporal coherence between maximum retreat and maximum seasonal velocity at this location, but it would be helpful to have more information on how this coherence was derived, and why then it serves as an ideal reference point.

Please also include a map view of the reference point and pinning point on the map. I found it hard to follow why sometimes the point 1.4 km upstream of 2017 front was used, versus the pinning point, as reference in the figures. What information is lost if, as a suggestion, the pinning point is used as the single point of reference throughout the text?

The main reason the pinning point is not used as the reference point is data availability: velocity estimates are generally not available downstream of the calving front (where they do exist, the velocity estimates are of the melange and not the grounded ice). Therefore, we chose a reference point where velocity data were available for the entire time span of the data. Strictly speaking, full data availability is not required since we can interpolate through temporal data gaps with the B-splines, but we would risk over-smoothing the seasonal velocity variations, particularly in the summer months where the front retreats past the pinning point. Overall, the reference point was not chosen to maximize coherence between front position and velocity; it was simply chosen to remain close to the calving front while ensuring as much velocity data are available for our analysis time period. In fact, due to the fast wave speeds of seasonal variations in the first 5-10 km upstream of the 2017 front (Figure 3), any point within that region would show very similar coherence with the front timeseries. Towards the end of Section 4.4, we also cite a recent study that compared velocity to front position for a moving point: “...comparison of front position with velocities at a moving point 1-km upstream of the front still show lower correlation for the years 2009 – 2010 (Joughin et al., 2020)...”.

To de-emphasize the choice of “1.4 km upstream”, we have modified the text to read (Line 383):

“The timing of maximum retreat for a given year is closely associated with the timing of peak seasonal ice velocity within a few km of the front position. Here, we choose a point approximately 1.4 km upstream of the 2017 terminus in order to maximize data availability close to front position for all years.”

We have added markers in Figure 4 to indicate the location of the reference and pinning points. We have also added a supplemental figure (Figure S2) showing the location of the pinning point relative to contours of the bed topography.

Comment 3. Multiyear variations in surface elevation: It would be helpful to have more text describing the motivation for selecting the particular time epochs shown in Figure 6. Each of the 4 panels represent elevation changes over time intervals of varying lengths, from ?1.5 years to ?2.5 years. The selected intervals also exclude July 2015-December 2015. There may be a reason for this, but without more context it seems too arbitrary.

Our original motivation for selecting those time periods was to emphasize distinct velocity and elevation change patterns corresponding to dynamical events (e.g., 2012 speedup and 2017 slowdown). However, we agree that this rationale can be opaque to readers, so we remade Figure 6 to show velocity and elevation differences for uniform time periods of 2.2 years (see Figure S1 to see how the time periods brackets the aforementioned dynamical events). The derived conclusions are unchanged, and the maps successfully emphasize our original intended features.

Comment 4. Discussion: I would like to see the discussion expanded to include considerations/limitations of this framework when applied to other glacier sites outside of Jakobshavn Isbræ (for example, in areas with notably lower SNR than Jakobshavn). Do you anticipate reduced SNR would limit the feasibility of this technique (and how may opting to enforce spatial coherency impact interpretations of phase velocity, propagation delay, etc).

We have added a section in the Discussion (Section 5.2) that addresses the applicability of these methods to other study areas and the main challenges we foresee. We include the text here for completeness:

“5.2 Applicability to other study areas

The GIMP velocity data over Jakobshavn Isbræ has high SNR for both the short- and long-term variations, which facilitates reconstruction of the spatiotemporal evolution of the traveling waves discussed in this work. Additionally, the dense temporal sampling relative to the signals of interest avoids potential issues related to oversmoothing of short-term velocity variations. However, many other glaciers and ice streams in Greenland and Antarctica will not have the same level of data coverage as Jakobshavn, which may limit the recovery of similar dynamical signals. Data coverage in this context is specified by temporal sampling and spatial continuity of velocity data where the former is likely to be the primary limiting factor for time-series analysis. For example, velocity data derived primarily from optical platforms are generally restricted to the summer months where cloud and snow cover effects are minimized. This asymmetry in coverage for a given year will alias reconstruction of seasonal velocity cycles, which would likely cause artifacts when attempting to quantify wave properties like phase velocity. We estimate that velocity data provided at monthly intervals constitute the lower bound for temporal resolution in order to quantify wave behavior at sub-annual timescales using the methods presented here. Of course, higher phase velocities for certain classes of dynamical signals may necessitate remote sensing data with finer temporal resolution (Minchew et al., 2017).

Spatial resolution and spatial data gaps can also limit characterization of wave behavior and other changes in ice flow. For example, regions near glacier termini will undergo periods of missing data associated with termini retreat where velocity data cannot be obtained over open water. The temporal interpolation properties of B-splines can mitigate these effects to some degree, but study areas with more persistent spatial gaps will likely benefit from incorporation of spatial coherency, which enforces that neighboring grid points share similar temporal behavior. However, data that require stronger levels of spatial coherency may also result in reconstructed signals that are oversmoothed, which would bias phase velocities and decay lengthscales to lower and higher values, respectively. In these situations, it would be beneficial to incorporate independent data sources like GPS time-series to provide additional validation data for 'tuning' the time-series analysis parameters. Overall, we expect that current and future remote sensing platforms will provide high-quality data similar to the GIMP data over Jakobshavn Isbræ, and we discuss those implications next."

Figure 2: Not a critique, but comment: This is a great figure that illustrates a lot of information in a concise way, clearly showing interannual variations in amplitude and inland propagation of signals from the front. The figure caption also included an excellent description of how phase velocity was extracted from the tangent angle.

Thank you! Please note that we added zero-velocity contours to Figure 2 to address comments by Reviewer 1 (in addition to extending the visualized time period).

Minor comments:

Figure 3a: I suggest scaling the y-axes such that a range of the same magnitude is shown for both. This would allow the reader to quickly compare relative changes in slope with distance between mean velocity without amplitude.

We experimented with a common y-axis for Figure 3A, but we found that it was more difficult to visualize the differences in decay lengthscales between the seasonal and multi-annual signals. As a compromise, we scaled the mean velocity axis to be twice that of the amplitude axis to make it easier for the reader to perform a conversion between the two. We updated the caption by adding: "(note the 2x scaling factor for the mean velocity axis)".

Figure 4: why are data from 2016 excluded from either group?

2016 was somewhat of an anomalous year for the seasonal cycle because the calving front had not sufficiently advanced during the winter months (Figure 7C), leading to a velocity at the beginning of the summer season that was higher than the other years from 2011 - 2018. We discuss this in the text around Line 383, and added a clarifying sentence in the caption for Figure 4.

Figure 5: Please add a note to caption to remind reader that red lines delineate winter 2017 reference calving position.

Done.

Figure 7a and c: It is very difficult to differentiate between years 2009/2019 and 2017/2018. If keeping the same color scale is preferred, I suggest making 2009/2010 dashed rather than solid lines to make years more distinct.

We have updated the figure to use dashed lines for 2009/2010. We prefer this cyclical colormap in order to emphasize differences in behavior between the two clusters of years, but we agree that the dashed line helps the reader better distinguish individual years within the clusters.

Figure 7d and correlation analysis: Are the velocity values shown here (and used for correlation analysis) taken from the continuous fitted time series? If so, what is the sampling frequency from these curves (every week, every month?) Are the extracted velocity values uniformly spaced in time?

For all reconstructed (continuous) time series of velocity and elevation, the sample spacing is approximately 4 days, spaced uniformly in time. We added a sentence to the beginning of Section 4 to state this.

Line 334: “A comparison of time series for points on and off the glacier suggest that much of the ice in the surrounding areas has been lowering since before the observation period, which is associated with a period of exceptionally high surface melt starting around 2009.” Can you include a citation for this?

After more discussion, we decided to remove references to high melt prior to the observation period since thinning of the inland areas in the 2000s for Jakobshavn is more likely a dynamic response to the speedup in the main trunk in 2004 (following disintegration of the ice tongue). We have modified this sentence to read:

“A comparison of time series for points on and off the glacier (Figure S1) suggest that much of the ice in the surrounding areas has been lowering since before the observation period. In these areas, thinning has been attributed to inland diffusion of steepening surface slopes following speedup and thinning of the fast-flowing trunk in 2004 in response to disintegration of the ice tongue (Krabill et al., 2004; Joughin et al., 2008).”

Line 387: “After the disintegration of the ice tongue between 1998 and 2004, the front rapidly retreated about 4 km over the period from 2004 to 2011.” Please include a citation for this, as front analysis used in this study did not start until 2009.

We actually use calving front data from the Greenland Ice Sheet Climate Change Initiative (CCI) from 2000 to 2016 (see Figure 7 where we compare velocity and front position starting in 2004). In the text in Section 3.3, we originally had a typographic error by stating the CCI data spanned from 2009 to 2016. We have corrected this. Additionally, we added a citation for the ice tongue disintegration between 1998 and 2004 (Joughin et al., 2004).