16th November 2021.

Point-by-point reply to Reviewer comments in preparation of the revised submission of the manuscript "Proper orthogonal decomposition of ice velocity identifies drivers of flow variability at Sermeq Kujalleq (Jakobshavn Isbrae)" to The Cryosphere.

Please find below a copy of the rebuttal letters to each reviewer now annotated in red stating what action was taken in response to each individual comment.

We hope this approach is satisfactory and, in combination with the marked-up version of the manuscript, addresses the reviewers' comments going forward.

Bet regards,
David W. Ashmore

RC1

I, and on the behalf of my co-authors, would like to thank Reviewer 1 for their insightful and constructive review of our manuscript. These comments will undoubtably improve the accuracy and clarity of the paper, and hopefully make this description of the first application of POD in this setting more convincing. Reviewer 1's main concerns are to do with the treatment of errors and some elements of the discussion, in particular our application of the Shreve flowpaths. These are discussed in the body of this letter in detail and summarised briefly immediately below.

We emphasise that we believe the use of an "error threshold" to be incorrect here. This is because the stated error for the SAR-derived velocity product relates to the "bulk" velocity signal, not the decomposed modes. We repeat the POD considering these errors and find they have no effect on the interpretation of the dataset modes. We also repeat our Shreve routing using several different subglacial water pressures and find our discussion to be robust. Related to the point about "error threshold" we accept our use of *S(%)* in the submitted manuscript to be imprecise: we have responded in detail below, instead referring to the more common metric of "percentage of variance".

Please see below our detailed response to comments. The Reviewer comments are italicised and indented, and immediately followed by our response in normally formatted text.

Best regards,
David W. Ashmore

RC1.

Review for: Proper orthogonal decomposition of ice velocity identifies drivers of flow variability at Sermeq Kujalleq (Jakobshavn Isbrae) Ashmore et al. 2021

Overview

> *This paper presents a novel analysis of a velocity timeseries maps measured using TanDEM-X/TerraSAR-X and analyzed using proper orthogonal decomposition. POD is a formal method of timeseries into different modes which*

have correlated velocity structure. By examining the time content and spatial distribution of the modes (i.e. spatially correlated time series components that contribute to the time variability of the velocity signal) interpretations are made to gain additional insight seasonal dynamics in the study area that would not be apparent without the use of POD.

Overall, the paper is very well written, and the methodology and application of POD to the study area is clearly explained. The authors present this study as a proof of concept, citing that the application of the technique would be much more useful where the seasonal signal due to hydrology was more pronounced. I agree that the location (Jakobshavn) is less than ideal given the dominance of velocity changes due to terminus variability and the small hydrologic component of the signal. Nonetheless, the technique could be quite interesting if applied to a different region and ideally over a much more expansive area

We are please the reviewer considers the application of POD to have potential in other areas, and that we have clearly explained our application of the technique. This clear communication of the technique is one our key aims of a proof of concept paper. We would also like to highlight to the reviewer that we are currently working with velocity fields from other areas with more diverse behaviour. The issue touched upon here by the reviewer regarding looking at a more expansive region is an important one and is also raised by Reviewer 2. The issue is that the imposition of spatial orthogonality means that spatially variable signals can get split into two or more modes. This a well-known feature of POD analysis and is observed in our hypothesised glacio-hydrological signal. In this case we can see that they are likely related (Figure 3, L285+) – however, if split further the signal becomes uninterpretable. From a more numerical viewpoint POD performance can struggle to resolve interpretable structures if the number of spatial data points (i.e. pixels) becomes significantly larger than the number of timesteps (Sirovich, 1987, Quarterly of Applied Mathematics). Domain size is therefore a critical choice when designing a POD experiment. We respond to this point more fully in the response to Reviewer 2's detailed comments and revise the manuscript to be clearer about these properties of the POD approach.

My main concerns which have to do with error and improving the discussion are outlined below.

Please see our detailed responses below.

Major comments

The study area of examined is only 2.5 x 4 km and the identified modes 2 – 6 show complicated spatial structures on the scale of a few hundred meters. Given that the ice thickness in the Jakobshavn trunk in this region is ~ 1.5 km thick, the authors should consider whether or not observing well resolved spatial structures at this scale is physically realistic. The length scale of basal variability that can be resolved at the surface is explored thoroughly by several papers by Gudmundsson.

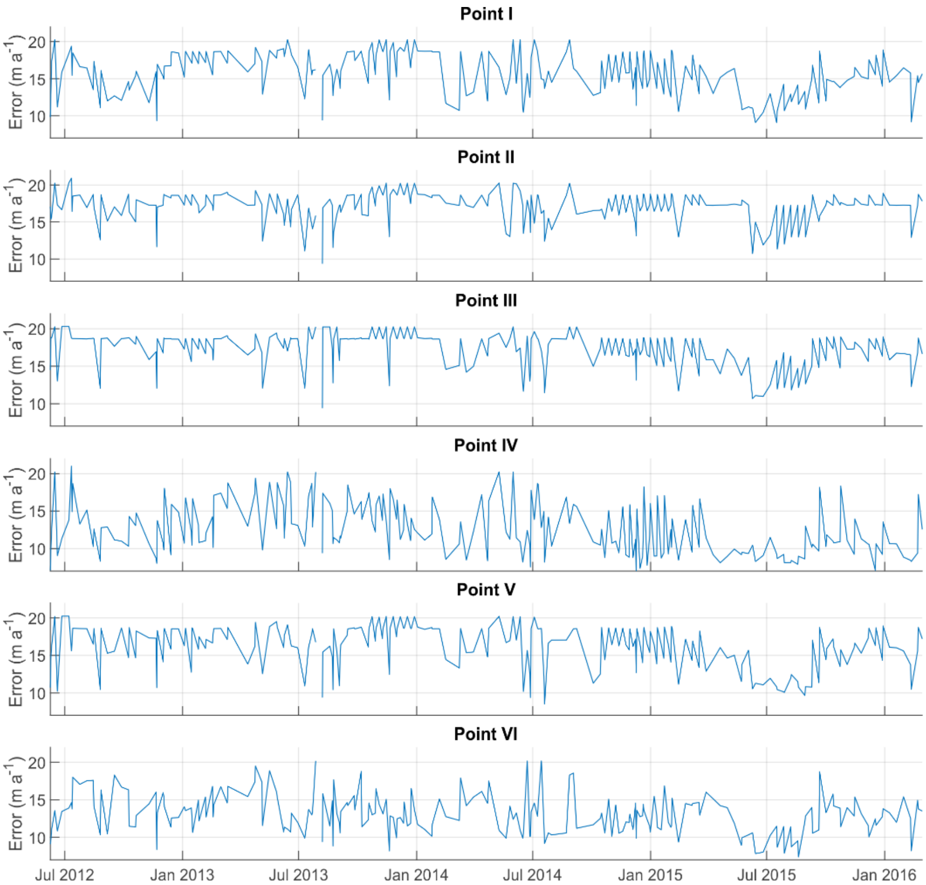
We are very mindful of the issue of not over-interpreting the spatial structures evident in the lower order modes. For this reason, we do not explicitly do this but instead reconstruct the temporal patterns of the components of surface velocity that are a function of modes 2 and 3 combined. The consequent variations in velocity, between selected points, that emerge are not just a function of the combined spatial structures of the modes but also of the changing temporal weightings associated with the spatial modes. Where we do discuss spatial variations between these points, we confine most of our interpretations to spatial variability across the entire study area, for example, contrasting high frequency variability at down-stream sites I-III with longer frequency patterns at sites IV-VI. Consequently, such interpretations are over length scales of 1-5 ice thicknesses (N.B. ice thickness varies between c.300-1500 m within the study area). This is long enough to discount effects of "bridging stresses" but certainly at the limits of what is normally discernible in surface velocities. The highest resolution spatial interpretation concerns the difference in velocity anomalies at point II compared with nearby points I and III which we propose could be related to a difference in the type of subglacial drainage system and its consequent inverse velocity response to enhanced meltwater inputs (i.e. a channelised versus distributed system, respectively) from upstream lake drainages. Here the length-scale of spatial interpretation is < ice thickness. However, we are in unchartered territory, earlier analyses of the length scales over which basal perturbations can be resolved in surface velocity patterns have all been concerned with surface expression in the complete surface velocity, whereas the POD method allows us to look at fractional components of the surface velocity that emerge when long time series of spatially coherent data are analysed as a whole. We have therefore essentially removed the high order temporal and spatial patterns that usually obscure this signal. Note that the velocity anomalies that we consider vary by c. 200 m a^{-1} (Fig 6) which is c.4% of the annual average flow velocity. Given the flow assumptions used, modelled expressions of basal perturbations at the surface are unlikely to be able to discern this magnitude of variability.

- This is most relevant to looking forward to future uses of POD, and so action here is to include a short discussion of this in Section 4 (Discussion) at ~L618+.

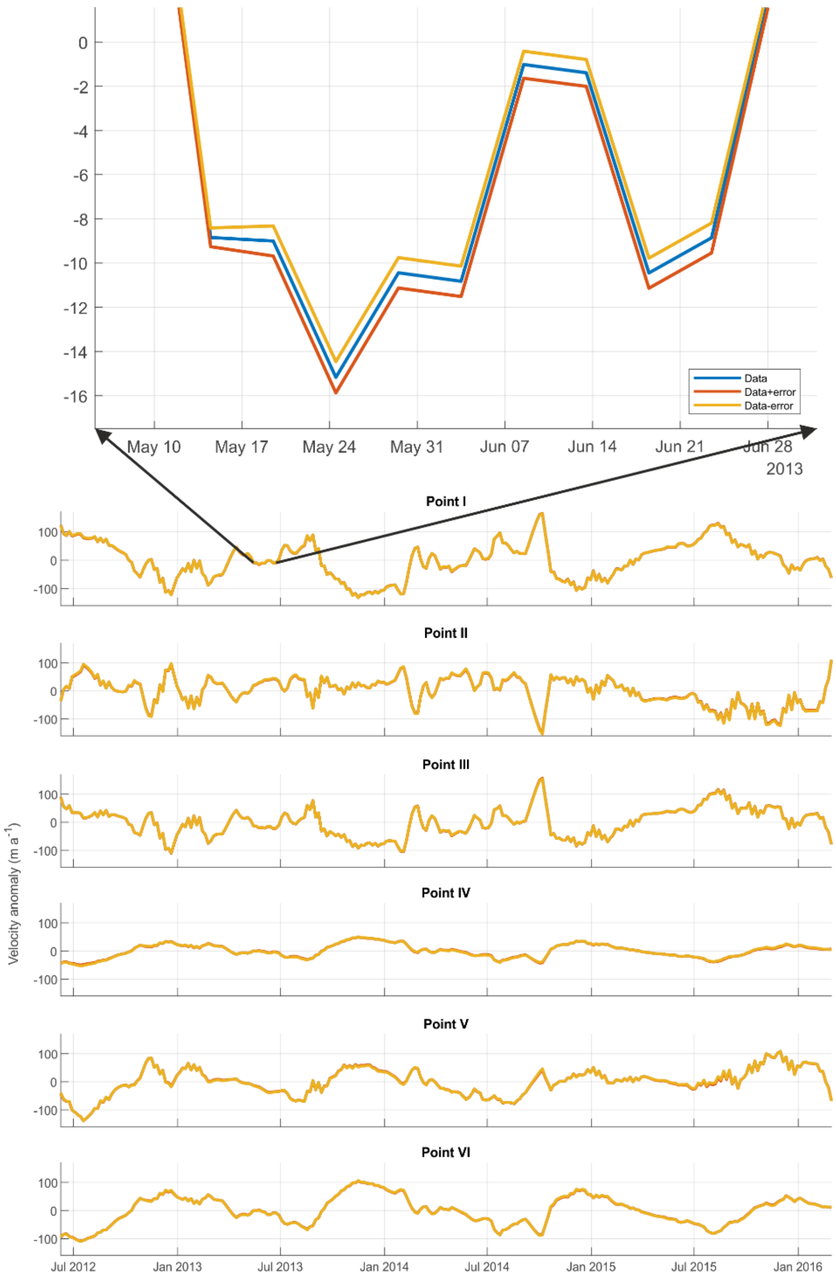
The error analysis is important but unconvincing. The authors analyze whether or not spatial pattern of errors correlates the spatial pattern of modes. However, the modes identified are low amplitude and contribute less than < 5% of the total velocity variance. Are these spatial and temporal variations above the error threshold? To assess this it might be worthwhile putting the error as an envelope on the modeled mode velocities in Figure 6 and maybe add an additional figure using the modeled velocity for specific snap shots in time where the spatial patterns are strong (i.e. during lake drainages) to determine whether spatial variability in velocity are above the error threshold.

One very important point is that the published error relates to whole signal – not individual modes of the decomposed signal. As such, using these "bulk" errors from the dataset as a "threshold" for velocity reconstructed with modes is incorrect. In other POD studies using particle image velocimetry fields the errors are manifested in the higher modes (Wang et al., 2015, Experiments in Fluids), not the modes due to physical processes, so it is reasonable to assume similar behaviour here. Notwithstanding this, the published errors range 10-20 m a^{-1} for points I-VI, an order of magnitude smaller than the main velocity fluctuations in the time series reconstructed using Modes 2 and 3 in Figure 6. To add error bars from the SAR-derived dataset to the reconstructed time series in Figure 6, we would need to assume that errors are a continuous variable and interpolate to the points of the regularly spaced time series. This assumption would be wrong: as the error magnitude can vary from 10-20 from one measurement to another. Our implicit assumption is that spatial structure in the errors can be approximated by looking at its summary statistics (Figure 7a-d). We do accept however, that error has structure temporally as well as spatially and that "whole domain" correlations are arguably an unsophisticated tool for this. To assess this for Point I-VI we plot the published error time series below: this shows no seasonality in the errors that correspond to our interpreted Modes.

- Action has been to overhaul this section (3.2.2) and remove the pixel-by-pixel correlations based on the interpolated error magnitude maps.



One approach, however, is to run three versions of POD: with the data, with the data plus the errors, and with data minus the errors. This allows an envelope of U and V Mode values to be calculated – which can then be used to reconstruct the velocity in Figure 6. This then shows the "envelope" of reconstructed velocities accommodating the errors in the SAR-derived data product. In the following figure we see that the differences in reconstructed velocity are imperceptible when interpretated at the scale relevant to our assertions. As such, we conclude our POD is robust against these published errors.



Bearing in mind, that this is the first application of the technique: we assign significant to modes based on their seasonality and by their comparison to several independent datasets (MODIS drainage dates; RCM run off time series). In general, we agree that detailed interpretation around key glaciological features (lakes etc.) is an important avenue and a scope of future work, which must happen through the application of POD to other ice motion datasets in a variety of settings (as mentioned at L469 in the reviewed manuscript).

- In combination with RC2 comments error section overhauled at Section 3.2.2.

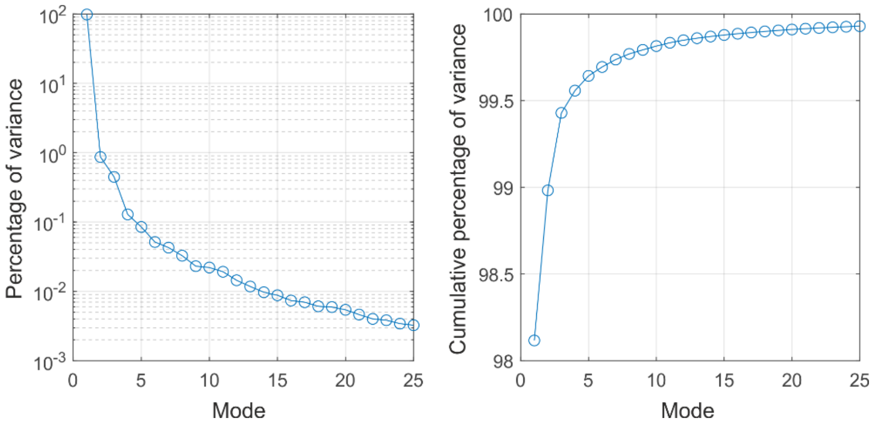
According to Figure 3 panel A the six modes identified only account for a combined ~ 75% of the time series data variance. Is this correct? Is the other 25% of the data variance noise? If so, to me it seems like modes-2-6 are below the error threshold. This ties into the previous question, but if this is the case, an explanation of why this doesn't matter would be useful.

Thanks for this comment. Upon reflection we accept we are imprecise in the way we discuss "*S(%)*", due to the relationship between singular values and eigenvalues, and the often quoted parameter "percentage of variance". To be clear: *S(%)* as currently used in the manuscript is not "percentage of variance" or *Var(%)* because eigenvalues are the singular values (*S*) squared.

If we change Equation 4 in the original manuscript to:

$$Var(\%) = 100\frac{diag(\mathbf{S}^2)}{\sum_{i=1}^{T} diag(\mathbf{S}^2)}$$

we can better discuss the relative importance of the relative importance of the modes. The figure below illustrates the dominance of Mode 1 (98.1%), and the rapid fall off information in the subsequent modes. See the figure below for a "scree plot" showing the fall-off of percentage variance against mode number.



Despite the small *Var(%)* values of Modes 2 (0.87%) and 3 (0.45%) we do believe they contain useful information: indeed, the kink between 2 and 3 indicates meaningful information may be contained in these modes and that they are "paired" in some way. This is in addition to the evidence of "pairing" seen in the temporal coefficients (L225+ and Figure 4). We also note that in the interpretation of signals of this size and smaller is commonly performed in the POD literature: e.g. in Albidah et al. (2021, Phil. Trans. R. Soc. A.) the 13th POD mode is interpreted in detail. This is, arguably, a testament to the potential power of POD when applied to ice surface flow velocity fields, especially when compared to numerous independent datasets as outlined in the previous comment.

In a revised manuscript the action would be to clarify the language around singular values and eigenvalues, replace usage of *S(%)* with *Var(%)* in Equation 4 and throughout manuscript.

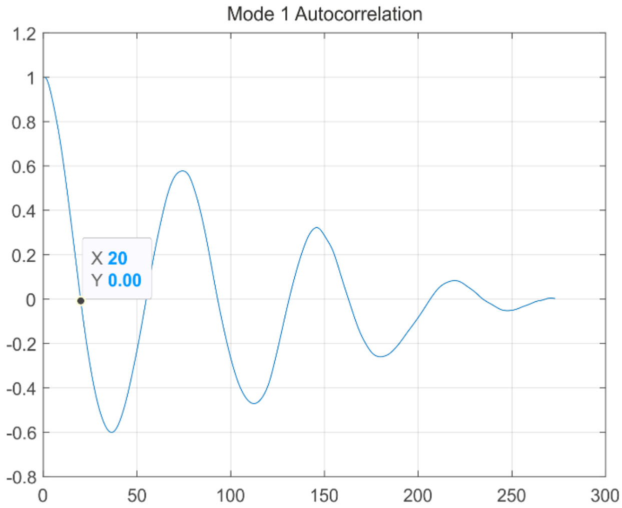
- ➢ Percentage variation replaces S(%) throughout manuscript

Line by line comments:

80: Would you consider this data set fully converged? Is there a variance threshold to be achieved?

Using an autocorrelation approach (integral length scale) we determine convergence of the dataset, rather than a variance threshold. In the figure below we show the Mode 1 integral length scale of ~20, following the criterion of Tropea et al. (2007; Springer Handbook of Experimental Fluid Mechanics Vol. 1), this shows we can call our dataset "fully converged" for our purposes. We can make this clear in a revised manuscript to avoid any ambiguity.

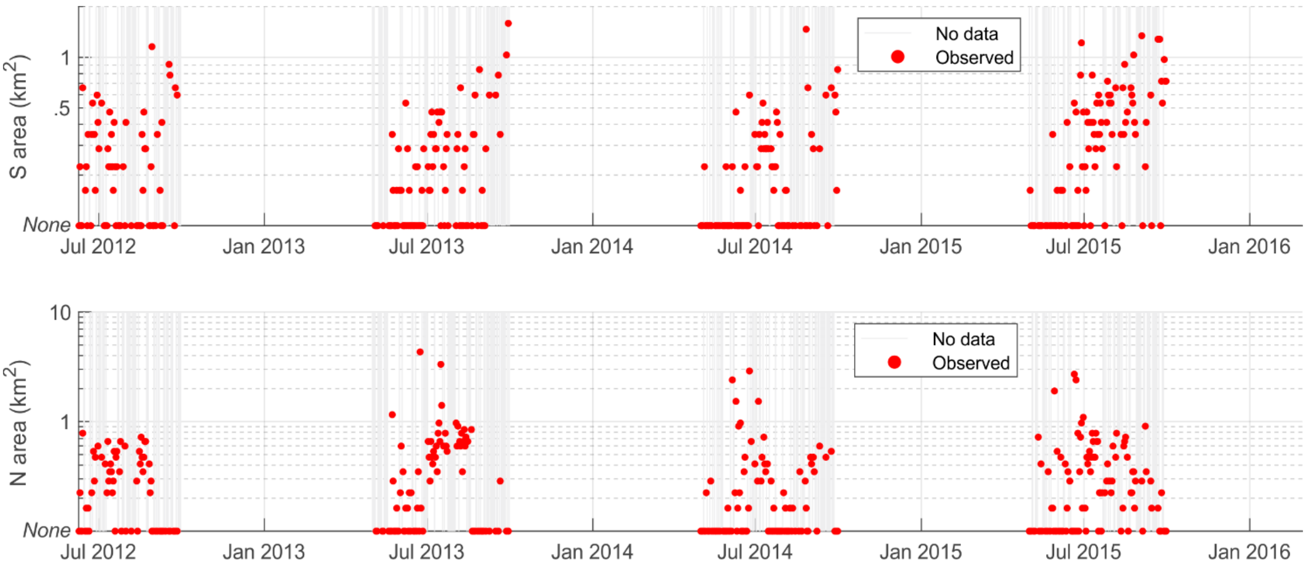
- ➢ Action is to include a sentence on this Section 3.1.



200: Did you visually check to determine whether the 5% threshold rejects questionable images?

Thank-you for this comment. In response we have gone back to our results and found that rejecting the highest 5% of values was not necessary and likely removed MODIS images of large pooled water extents. In the figure below we show that this does not alter that identification of "lake" drainages as performed in the submitted manuscript. This can be included as updated panels in Figure 4 for a revised updated manuscript.

- Action has been to update Figure 4 with the data shown in the below panel, and remove reference to rejecting highest values in the methods Section 2.3.



215: Does using a different assumption on the effective pressure (i.e. effective pressure = some fraction of overburden) change the flow paths significantly from what is presented here?

Varying the effective pressure has the greatest influence on flow pathways derived by hydro-potentials when the gradients in bed topography are low. In our study region, however the basal topography is very well constrained and shows high gradients particularly transverse to the average ice flow direction. We reference the work of Mackie et al. (2021) who robustly show that the flow pathways derived for our study region are insensitive to errors in the basal topography. Consequently, the flow pathways will not be significantly altered by changing the effective pressure, especially for the main flow pathway that follows the main valley trunk, i.e. the pathway which point II sits above. To illustrate this, see figure below which shows Shreve flow paths in the POD domain, as identified as "flow accumulations" of over 100 cells, using Eq 2 from Wright et al. (2016, JGR:ES) for varying water pressures, as a fraction of overburden.

- ➢ Action is to note these results in Section 2.4 (L227.

General comment: Maybe change Points I-VI to alphabetic identifiers. Even though it the Modes use numeric identifiers and the Points use roman numerals it can still be a bit confusing in the text.

We would still prefer not to do this. The roman numerals are unique identifiers and were chosen over numeric identifiers to avoid confusion with both Modes and figures numbers, and chosen over alphabetical characters avoid confusion with figure panels.

- ➢ No action taken

340: How robust are the PDA's if the pressure assumptions in Shreve's calculation are changed?

Please see our response to point regarding L215.

340-342: Not necessarily true. Water routing pathways can end up being really complex and counterintuitive because they ultimately reflect spatio-temporal changes in the basal pressure field and thus are sensitive to transients. For an example of this see Stevens et al. 2018.

This is correct and the work of Stevens et al (2018) is clearly relevant here, however, their modelling doesn't overlap with this area, and the relationship between melt (as a broad proxy for effective pressure) and velocity at marine terminating glaciers is highly variable from site-to-site (e.g. the "types" of Moon et al, 2014). Our response to L215 and the geostatistical work of Mackie et al. (2021) show generally consistent pathways in our study area relative to the points where we perform a more detailed analysis. We acknowledge that transients are likely important, and indeed, such complex routing of water between channelised and distributed subglacial drainage systems is implicit in our interpretation of the spatially varying pressure response to transient periods of high water inputs from surface pond drainage events. We choose not to speculate explicitly about the spatial and temporal scales over which such complex routing may occur as we have no evidence to support this. It is beyond the scope of our paper to incorporate the level of daily-forced hydrological modelling of Stevens et al. (2018). Instead, we confine any insight we have into the temporal and spatial changes in hydrology to the only three independent hydrological indicators we have, namely: (i) the theoretical spatial structure as defined by Shreve's hydro-potential theory; (ii) the location and timing of filling and drainage of surface melt water ponds derived from remote sensing, and (iii) the MAR v 3.11 modelled meltwater runoff output. In a revised manuscript we highlight the uncertainty of Shreve flowpaths, due to transience and variations in basal water pressure as exemplified by the boreholes of Wright et al. (2016), and Stevens et al. (2018), and explain clearly how we use Shreve flowpaths in our study.

- ➢ Action has been to add this reasoning in text at ~L369 .

343: Uncertainties in water pressures can also make a large difference…see Wright et al. 2015.

Yes, this is a similar point to that made above. It is also worth pointing out that much of the transient water pressure behaviour measured by Wright et al. (2016) and modelled by Stevens et al. (2018) is short term (e.g. diurnal and daily) change. The image-pair separation we use is typically 11 days, and the median separation of velocity images is 5 days so such high-resolution considerations are again beyond the scope of our analyses, even if we did have higher resolution hydrological information. In the future, POD could be applied to a dense grid of GPS receivers, which may yield the detailed insights into processes operating on these (sub)daily processes.

- ➢ No direct action, point dealt with in previous comment

346: and elsewhere: The use of provoke seems a bit strange. Consider changing.

Can revise.

- ➢ Action has been a straightforward edit.

Paragraph starting at 339: I find the interpretation in this paragraph quite specific and speculative for the evidence given. While the author's use the PDA as a reference, the location of the PDA is uncertain. All the points besides III are look similarly close to the PDA (and generally closer than an ice thickness to each other), making the spatial explanations unconvincing. As an example, you claim that points 4-6 are under the influence of channelized drainage because of their inferred proximity to drainage pathways and negative velocity anomalies, but point 2 which is inferred to be directly over a drainage pathway has does not show a negative velocity anomaly? How is this consistent?

I would advise focusing the discussion more on the processes which could result in the fluctuations observed at the points and less on the proximity to inferred drainage pathways. This will keep you from forcing the velocity timeseries into a "box" based on our conceptual understanding of subglacial hydrology and the location of the inferred PDA (which is poorly constrained). By doing this, you could use the measurements to either support or refute the hypothesis that a PDA calculated using Shreve's assumption likely controls hydrology in the region. Focusing on process will also allow for more discussion on some of the more interesting aspects (i.e. the large winter fluctuations).

We consider the second more general point first. We appreciate the appeal of not being too drawn into explaining spatial variability in the context of the Shreve equipotential PDAs given the uncertainties in defining these. However, we do this because the hydrological process explanations ought to be linked to some evidence that is independent of the velocity analyses. Furthermore, these process explanations rely on spatial variability in drainage system types at the same points in time and on temporal variability in drainage system type at the same points in space. Without some independent assessment of the spatial structure, our explanations might become circular.

On the first specific point: we discuss proximity to PDA *confluences* rather than PDAs because of the spatial uncertainties in any specific PDA route. Points I and II are almost twice as far from a confluence than the next most distal point (point V). In lines 348-349 we explicitly point out that point II *does* show negative velocity anomalies over short-time periods as a consequence of being more likely to be influenced by channelised drainage.

- ➢ No direct action in response to these paragraphs. See addition in Section 3.2.1 (~L370) making clearer the reasoning for using Shreve flow paths, and our the checking of their robustness in Section 2.4 (~L229).

General comment: Why did you chose these point locations?

These points are simply a practical method for us to interrogate the large dataset in detail. The points were chosen to sample several points of the domain independently and prior to "knowing" the results.

- We make a note of this in the revised manuscript at L348

General comment: The fact that you infer such a complex velocity structure (if over error threshold) over short length scales imprinted on the main signal is a really interesting result. It might be worth discussing this, as it seems more fundamentally interesting than the relatively unconstrained PDA analysis.

This is an interesting point. In the paper we seek to strike a balance between demonstrating that the method pulls out new coherent patterns and signals not easily seen in the main dataset – but not over-interpreting of the possible underlying causes, for the reasons the Reviewer mentions above. In a revised manuscript we can insert an explicit mention of this around L254: stating that we seek to highlight the POD analysis' ability to draw out hidden patterns, but we recognise the limitations of over interpreting the underlying causal processes at the same length scales.

- Action is to discuss this reasoning explicitly in Section 3.2.

418-419: Without mapping upgradient lake drainages and determining if there is a velocity response the analysis present does not imply this.

We accept this. Our aim was only to highlight that during the melt season local lake drainages tend to correlate to the major velocity excursions in the reconstructed velocity. Will reword to remove this inaccurate and general assertion.

- Action is to remove assertion

RC2.

I, and on the behalf of my co-authors, would like to thank Dr Bryan Riel for their insightful and constructive review of our manuscript. These comments will undoubtably improve the accuracy and clarity of the paper, and hopefully make this first application of POD in this setting more convincing to the community.

We would like to specifically draw attention to two themes of the Dr Riel's comments which we see as strongly interlinked: that our results are influenced by the "travelling waves" recently described by Dr Riel in this journal; and that we should expand our study area to a wider area. By specifically looking at a small area we act minimise the first effect, in addition to minimising the potential effect of spatially variable velocity image coverage. The travelling wave has a reported speed of 0.4 km/day, yet study area is 4 km long, and the image pairs used are typically separated by 11 days. Hence, we expect any wave to travel entirely through our study area and not be resolved by the velocity product on this scale. The strong dominance of Mode 1 in explaining the dataset variance is, as Dr Riel points out, evidence that the area does at this scale broadly move "in phase". Our study area is ~11 km flowline-distance from the terminus most-retreated position where the effect, according to Riel (2021, TC) Figure 2, is becoming less pronounced. We do accept, however, that as the velocity product also uses overlapping pairs we might expect some contamination of the modes, but this is unlikely to have an annual periodicity and is therefore more likely to be accounted for in the higher (i.e., low variance) modes. In the revised manuscript we would endeavour to make this justification explicitly.

Please see below our detailed response to comments. The Reviewer comments are italicised and indented, and immediately followed by our response in normally formatted text.

Best regards,
David W. Ashmore

Overview:

> *The manuscript "Proper orthogonal decomposition of ice velocity identifies drivers of flow variability at Sermeq Kujalleq (Jakobshavn Isbræ)" explores the application of POD to ice velocity time series in order to efficiently decompose time series into orthogonal spatial and temporal modes. This decomposition could potentially isolate velocity signals originating from distinct forcing mechanisms, such as changes in terminus position or enhanced basal sliding due to subglacial hydrology. These signals may or may not be readily apparent from the raw velocity time series, so POD could potentially reveal key signals to enhance our understanding of ice dynamics.*
>
> *Overall, the manuscript was well-written, the methods were clearly described, and the results and supporting data are quite intriguing. POD and its variants have always been powerful analysis tools for spatiotemporal data, and this paper demonstrates the utility of POD to the increasing availability of velocity data over the ice sheets. However, POD also has well known limitations, primarily due to the assumption of orthogonality of the modes. While orthogonality doesn't necessarily affect lower-order reconstruction of the data, it does leave users vulnerable to over-interpretation of the separate modes, and I believe the authors need to explore these vulnerabilities a bit further. I describe in detail the implications of orthogonality below. My other main comment is concerned with the error analysis, which I also describe below.*

Thank-you for your generally positive assessment of our manuscript, and we are pleased that you consider our findings intriguing and that we demonstrate the utility of the POD technique in this new setting.

> *I do believe that POD will be an important tool for glaciologists. I also believe that potential users should be well aware of the expected behavior of POD-type algorithms on different classes of dynamical signals. If the authors can sufficiently address and discuss these issues, then this work will indeed be a valuable contribution.*

Thank-you for this assessment. In the submitted manuscript we tried to not present POD as a silver bullet to disentangling processes from observed spatio-temporal ice dynamics that can be applied without caution. We explicitly discuss the splitting of modes throughout and make recommendations for future work to focus on how modes evolve over specific glaciological features (L439-443). In our detailed responses and in a revised manuscript we address these issues and, in general, highlight more clearly the features of the POD approach, and how these must be considered during interpretation.

Major Comments:

> *1) Currently, my biggest concern is the application of POD to signals that propagate in space, which encompasses most of the signals related to ice dynamics. Taking the Sermeq Kujalleq (SK) data presented here as an example, the largest signal (Mode 1) is clearly related to the dynamic response of the glacier to stress perturbations at the terminus. However, previous work has shown that this response has a finite phase velocity, i.e. it takes some time for the signal to propagate upstream (~400 m/day; Riel et al., 2021, as a recent example). The fact that POD enforces every point in the modeling domain to have identical temporal eigenvectors (scaled by the spatial coefficients) means that every point has the same phase for Mode 1, which was stated by the authors. The consequence of this is that POD will end up splitting the spatially-propagating signal into multiple modes (with the*

same periodicity), just as the authors hypothesized for the conjugate Mode 2+3 pair. It's entirely possible that Modes 2 and 3 are partially "conjugates" for Mode 1, i.e. they are compensating for the error of modeling a finite phase velocity signal with an infinite phase velocity signal. Additionally, the spectral content of an upstream-propagating signal can change, e.g. pulse broadening. All in all, any interpretation of the weaker modes for subglacial hydrology is likely to be corrupted by the propagation of the compensation signal for Mode 1.

Please also refer to the top of this letter where we discuss that: (a) our POD domain is small (4 km long) relative to the 0.4 km/day travelling wave; (b) the image-pairs used to derived the velocity image are typically 11 day apart allowing enough time for the travelling wave to pass through, if present; (c) the POD domain is 11 km flow-line distance from the most retreated terminus position where the effect is less pronounced.

The Reviewer is indeed correct that Modes 2 and 3, that we hypothesis are dominated the glacier hydrologically processes, *may* contain some information on the kinematic wave. However, this is an inevitable ambiguity when assigning physical meaning to purely statistical features. We see no evidence for "leakage" from Modes 1 to Modes 2 and 3. In fact, we suggest the opposite, that the Mode 1 is "contaminated" by a hydrological signal at L308-310,

We are careful not to state that this pair of modes is exclusively due to glacier hydrology, rather that we present evidence that the modes are "dominated" by it. It is precisely that POD modes are purely statistical features that we attempt to build the case that they are dominated by glacier hydrological processes through comparisons to independent datasets and are very explicit about this in the manuscript (L249).

We agree that the manuscript should explicitly discuss the travelling wave phenomena recently described by Riel et al. (2021) in text. We particularly value the Reviewers suggestion "(c)" to add a discussion of how different "classes" of dynamic signals expected on glaciers may appear in POD datasets.

- Action is to include discussion of POD signal "styles" in Section 2.1; include a discussion of travelling waves in Section 2.2.1; and include a new Figure of the "scree plot" of the POD eigenvalues and discussion of this in Section 3.1.

In general, the splitting of a spatially-propagating signal into multiple modes will be dependent on the phase velocity. When the phase velocity is very high, as would be the case for a dynamic wave resulting from redistribution of longitudinal stresses, then the splitting will be minimized. However, for slower moving waves, as would be the case for kinematic waves or subglacial hydrological effects, then one would expect the signal to be split into more modes, which would hinder any subsequent interpretation of these modes.

With that said, the supporting data for the hydrological interpretation for Modes 2+3 are compelling (and very cool). If the authors can find a way to account for the leakage of Mode 1 into the other modes to isolate the hydrology signal, then the interpretation would be more convincing. Therefore, I would suggest the following initial diagnostic steps and improvements:

These points are responded to the previous comments and the following comment "(b)" on the distribution of the eigenvalues.

- We consider this dealt with in the previous response.

a) Expand the spatial extent of the POD analysis (e.g., Figure 3) so the readers can see a wider picture of how the spatial coefficients change in space. I think the spatial extent shown in Figures 2b-d would be sufficient. This way, we can get a better sense of any mode splitting due to signal propagation.

As outlined in the top of this letter, the issue with looking at a large area is that the signal then becomes difficult to interpret due to spatially moving signals being split over many modes. Furthermore, Sirovich (1987, Quarterly of Applied Maths) highlights that the number of pixels shouldn't be significantly more than the number of timesteps, and so we can expect POD performance to degrade as domain size increases significantly relative to timesteps.

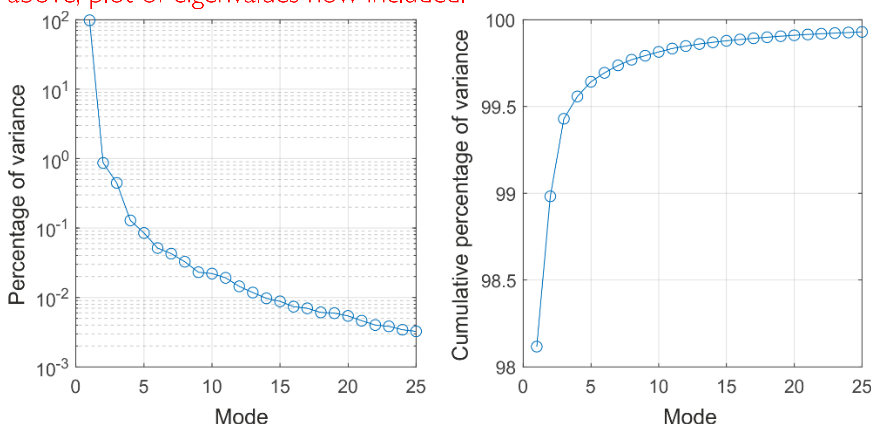
- No action, expanding the study area would undermine results as outlined in the above responses.

b) Include a plot of the singular values for the decomposition. I know these values were included as labels in Figure 3a, but I think a plot can give a better sense of any potential mode splitting. Slowly propagating signals will be split into a higher number of modes, and one would see a more gradual decay of the singular values. Rapidly propagating signals will have a much sharper spectral decay.

This related to the point made by Reviewer 1 on the "percentage of variance" explained by each mode, as this is the common method of communicating the relative importance of modes in other POD studies. Briefly, in version 1 of this manuscript we discuss the relative importance of the singular values, not the eigenvalues. This resulted in some imprecise wording to be rectified in a revised manuscript. From the eigenvalues we can describe the "percentage of variance" explained by each mode: a common diagnostic metric for orthogonal decomposition work.

The eigenvalues show a strong dominance of Mode 1, explaining 98.1% of the variance. Despite the small *Var(%)* values of Modes 2 (0.87%) and 3 (0.45%) we do believe they contain useful information: indeed, the kink between 2 and 3 indicates that they are "paired" in some way. Arguably modes 2 and 3 cluster outside of normal, smooth pattern of decay: evidence that they contain meaningful information. Note also that the clustering of eigenvalues is only one of two common diagnostic criteria for identifying quasi-conjugate pairs, the other being the phase-shifted features between Modes 2 and 3 and the strong seasonality of their sum which we show in Figure 4.

- Discussed above, plot of eigenvalues now included.



c) For the purposes of a methods paper, it would be hugely beneficial to compare the decomposition between SK and a glacier with known hydrological effects that dominate its dynamics. I would expect quite different spectral decay. Since that would entail a lot of additional work, alternatively, the authors should add some discussion on how different classes of dynamic signals will have different phase velocities and how those phase velocities would affect the POD results.

We do certainly take on board the idea that a short discussion about how one might expect different "classes of dynamic signals" and how they might exhibit in POD modes would be a worthwhile addition to the paper. In a revised paper this would be included in Section 2.1 and referred to in the Discussion and Interpretation. We are currently working with new collaborators on comparable SAR-derived velocity fields from other glaciers with different styles of hydrological behaviour.

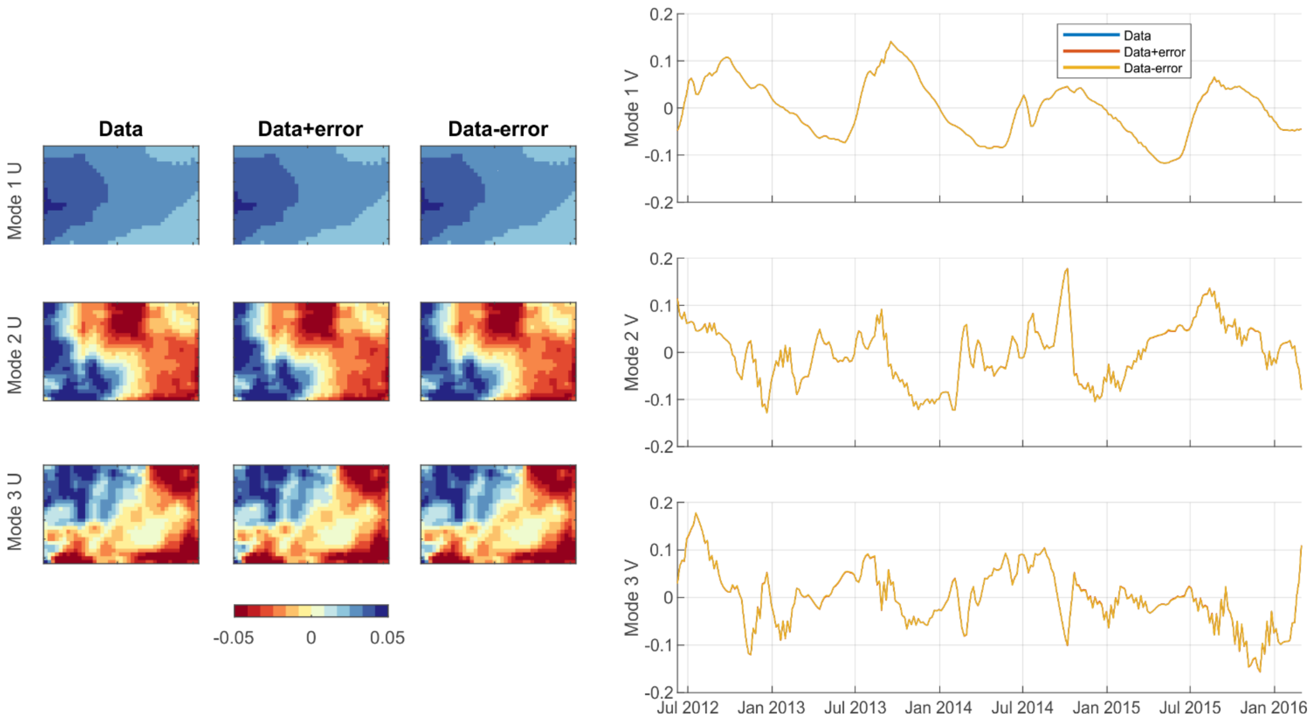
➢ See above, new discussion added to Section 2.1.

2) The error analysis performed here is a bit too simplistic. Mainly, the errors are analyzed as if they are independent from pixel-to-pixel. However, it is possible for the errors to have some non-zero spatial correlation (off-diagonal terms in the scene-wide data covariance matrix), which could be quantified empirically via a variogram over areas that are known to have minimal ice velocity. A more robust analysis would be to generate random realizations of the velocity data (perhaps with covariance matrices with non-zero off-diagonal terms) and re-do the POD in a Monte Carlo sense. How would the spatial coefficients change with the different data realizations? Since POD is computationally efficient, this likely wouldn't take too much compute time.

A related point is also raised by Reviewer 1. The Reviewer is correct that there *may* be complex correlations in space and time in errors, however, it is also important to note that the published errors (10-20 ma^{-1}) are very small relative to the seasonal velocity fluctuation of -1000 to +1500 ma^{-1} and in the submitted manuscript we only assign glaciological significance to modes after comparison to independent datasets.

If I understand the variogram method proposed correctly the issue is that that in our POD domain we purposefully selected a dynamic area with no areas with minimal ice velocity. We do not take a Monte Carlo approach because the stated errors are variable from one image to the next, and so would certainly be "noise" in the data and accommodated in the low-variance Modes.

To address this comment, this we turn also to Reviewer 1's comment on providing an "error envelope". We run the POD again with the errors added, and then with them subtracted. In the below figure we find the POD Modes are insensitive to the published SAR-derived velocity errors and variations in modes are imperceptible when interpretated at our scale of interest and plotted on the same axis.



➢ Action is to overhaul errors section and discuss the points outlined above in detail.

Line-by-line Comments:

- Line 39: Might be too soon to state the modes correspond to "distinct flow features".

Accepted, this statement comes from hydraulic applications of POD where this more appropriate. Will remove.

- Action has been to remove this mention and edit sentence slightly for clarity

- Line 77: The distinction between eigenfunctions and eigenvectors can be a little misleading since an eigenfunction is instantiated as an (eigen)vector. In other words, they are both vectors, and my intuition is that the temporal vectors are closer to being instantiations of actual eigenfunctions. I would suggest to either replace the use of eigenfunctions with eigenvectors or to add a bit more detail into why the use of eigenfunctions is more appropriate.

Accepted, change to eigenvector.

- Action has been to remove reference to "eigenfunction" and tighten-up references elsewhere

- Line 79: What is meant by a "fully converged dataset"?

We use an autocorrelation approach (to ensure a sufficient number of integral length scales), this shows signals repeat sufficiently for POD analysis.

- See response to RC1 comment

- Line 81: Orthogonal does not by definition mean independent. Achieving statistical independence (e.g., minimizing mutual information between modes) tends to be the objective of Independent Component Analysis and can result in modes with very different spatial and temporal eigenvectors. More importantly, orthogonality does not guarantee that independent forcing mechanisms will be expressed in the different modes.

Agreed, we can change this to spatially coherent and remove "by definition" which is potentially misleading.

- Action has been to remove these phrases

- Line 99: Is there any indication of the numerical algorithm being performed by the "economy" SVD? Is it performing a full SVD and just removing a certain number of singular values? Or is it performing some form of randomized SVD? This information may be useful for reproducibility purposes.

We expect the economy SVD to have no effect on results. The approach is designed by Mathworks MATLAB, and from their documentation: *"The economy-size decomposition removes extra rows or columns of zeros from the diagonal matrix of singular values, S, along with the columns in either U or V that multiply those zeros in the expression A = U*S*V'. Removing these zeros and columns can improve execution time and reduce storage requirements without compromising the accuracy of the decomposition"*

- No direct action taken here.

- Line 124: I think it's a little confusing to state "the study area must be dynamic" since that may be interpreted as only dynamic time series can be decomposed with POD. I think what's being stated here is that dynamic glaciers tend to be well-observed by satellite platforms, so I would suggest some slight re-wording.

Our argument is that to a POD is more likely to reveal statistical structure from an area with fast and variable flow, such as the trunk of a major outlet glacier (cf. a low-melt ice sheet interior). We will reword to make this clearer.

- Action has been to reword sentence for clarity

- Line 141: Is there really a requirement for the time series to be stationary in order to decompose them? What happens if non-stationary time series are used? I ask mainly because multi-year signals can be quite important for understanding the full spectrum of ice dynamics, and it would be interesting to see how POD handles those.

The time series must be repeatable in term of its coherent signals, if not there is a strong risk of losing coherent structures in higher modes that are noisy and impossible to interpret.

- Action is to reference integral length scale criterion at L237, Section 3.1, in response to RC1.

> *- Line 145: A bit awkward to describe a slowdown with an "increased rate". It would be clearer to just state the decrease in velocity values after 2016.*

These are different statements; the velocity is already decreasing but does so at an increased rate. Will reword for clarity.

- Action has been to reword sentence for clarity.

> *- Line 165-166: Are the formal data errors accounted for during the inpainting and interpolation procedure? If not, then every point is assigned equal weight, which could affect final interpolated signal.*

No, but the errors 10-20 ma^{-1} are very small for annual fluctuations of between -1000 ma^{-1} and +1500 ma^{-1}. As mentioned in text a maximum of 4 (of 232) measurements are interpolated over in a small area of the domain.

- No action taken.

> *- Line 167: Is there any difference between post-smoothing the columns of V and pre-smoothing the time series of X?*

Thanks for this point. Yes, there is a difference, our approach follows the robust methods fully justified by Higham et al. (2016, Measurement Science and Technology). We will add this short justification in text.

- Action is to edit to point the reader to this paper.

> *- Line 195: What is the resolution for the "high-resolution" MODIS analysis?*

MODIS is daily and posted at 250 m pixel size. We can state this in-text.

- No action. After double checking details of the MODIS band 1 is already stated at L185.

> *- Line 220: Here's the main issue: the results are stating that the entire area moves in-phase, but previous results show the phase changes with upstream distance, with a finite phase velocity of ~400 m/day.*

In general, this point is rebutted in the detailed comment above. We state here that the Mode 1 velocity move in phase: this is essentially just a description of the POD algorithm output. This is an important distinction from saying the whole glacier *itself* moves in phase. Naturally, these travelling waves must be accommodated by the higher modes if resolved in our study area – but not Mode 1. While it is certainly important to acknowledge the limitations of the approach it is also important here to underline the stated aims of the paper: to apply technique to a new data type, and build a case through the comparison of POD to independent datasets that this technique shows something never seen before.

- See above responses

> *- Figure 3: It would be useful to include a length scale bar in the images.*

Okay, can include.

- Included

- Line 286: "we sum of Modes 2 and 3" -> "we sum Modes 2 and 3"

Thanks, amended.

- ➢ Edited

- Line 289: What are possible artefacts? If one were to interpret a splitting of a spatially-moving, seasonal signal into multiple modes as artefacts, then those would also exhibit strong seasonality.

Okay, can be more explicit here in text. We would consider a "split" spatially moving, seasonal signal due to glaciological processes to not be an artefact as this is precisely the signal of interest in a study like this.

- ➢ Reworded for clarity, please also see discussion of expected behaviour of POD signals.

- Line 425: Conversely, it would also be useful to apply POD to ice flow model outputs (forced with distinct mechanisms) to determine how well the different signals can be separated.

This is an interesting approach, and a test of POD capabilities with synthetic data: assuming the model itself was sufficiently accurate.

- ➢ No action here

- Line 440: Again, this is a key point: spatial patterns of ice flow will split into multiple modes based on the propagation speed of the signal. Slower propagation speeds will be split into more modes.

Discussed at length previously. We underline again here that in the Conclusion we made the point that POD users must be very careful what temporal and spatial scale you do the analyses, at as will depend on the particular process you wish to investigate.

- ➢ No direct action, see above responses

- Line 460: Consider re-use of the word artefacts here as well. Mixing of signals into multiple modes could be considered an artefact but is definitely not a minor effect.

We discuss at length the mixing of Modes 2 and 3, and will add sentences considering the potential for Mode 1 leakage.

- ➢ Added general point regarding this in the conclusion.

- Line 467: My intuition is that hydrological effects will have slower spatial propagation speeds than redistribution of membrane stresses (such as the response to terminus forcing), which would cause more modal mixing.

Okay, can note this at this point.

- ➢ Small edit regarding different propagation speeds. Point about splitting made in previous paragraphs.