5th October 2021.

Author Response to RC1 for manuscript "Proper orthogonal decomposition of ice velocity identifies drivers of flow variability at Sermeq Kujalleq (Jakobshavn Isbrae)" submitted to The Cryosphere.

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.

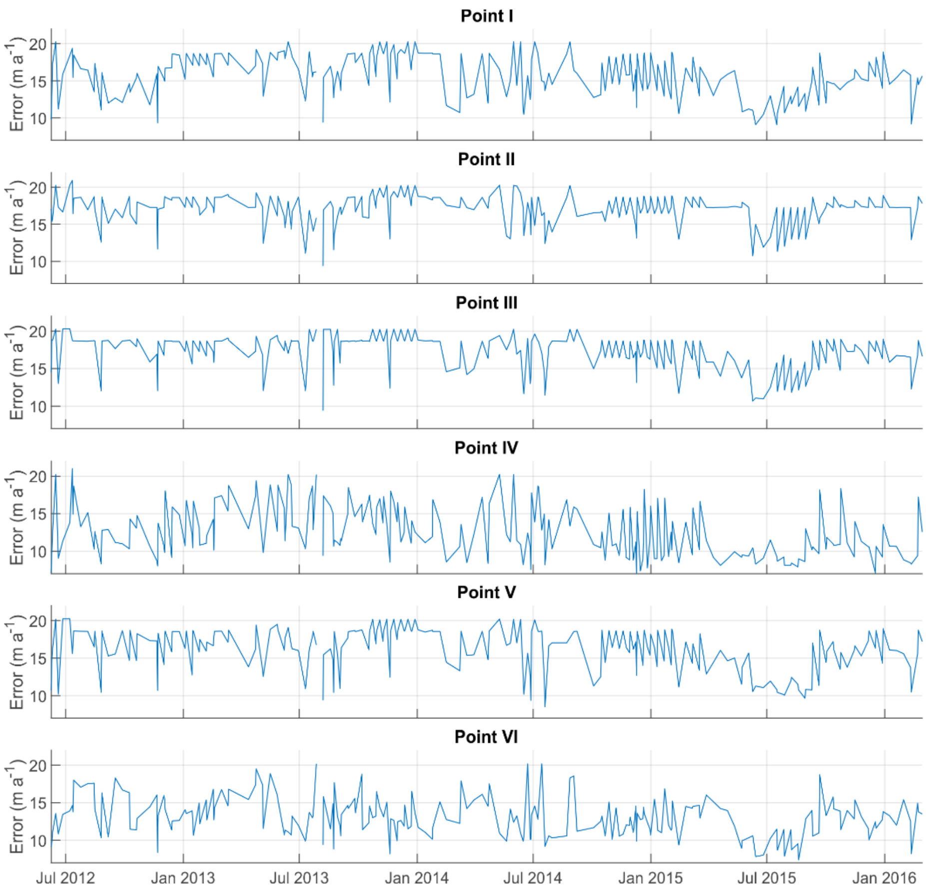
Major comments

> *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.*

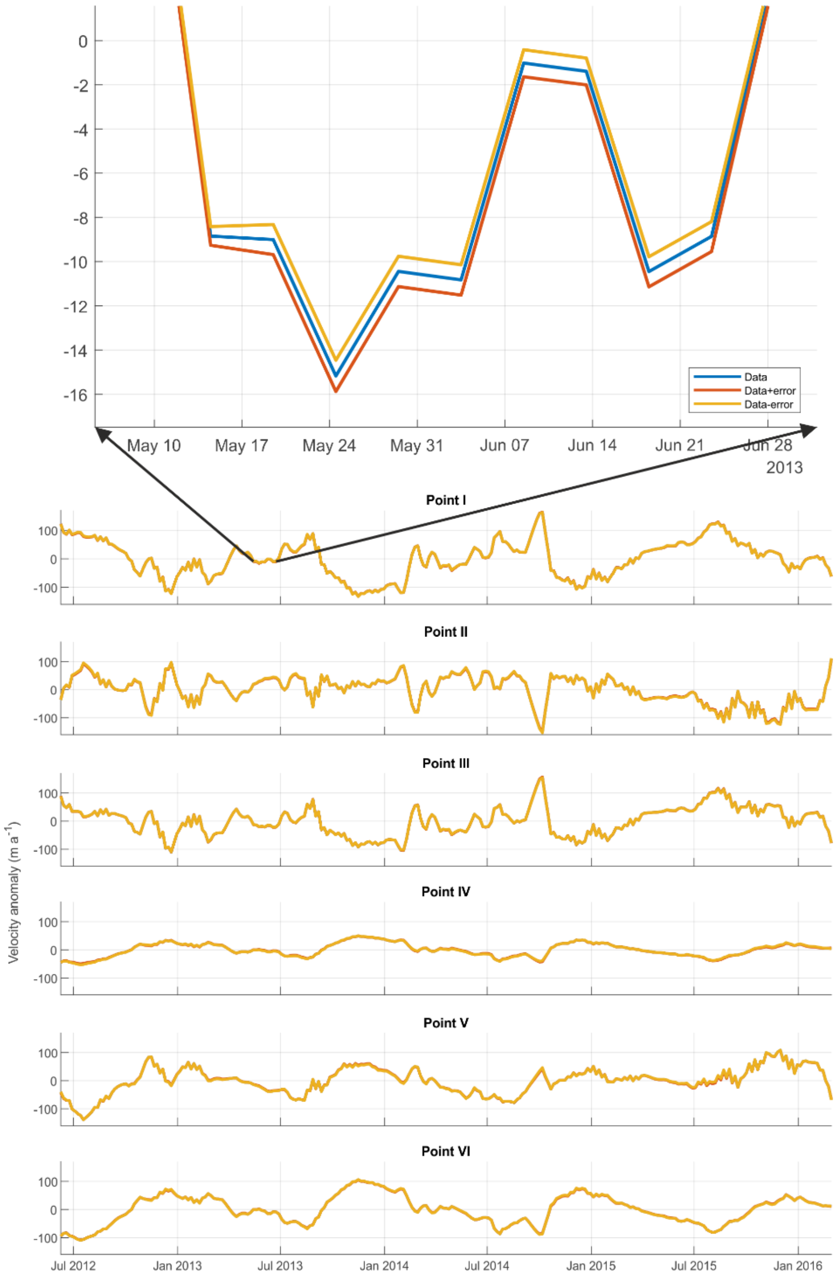
We are very mindful of the issue of not over-interpreting the spatial structures evident in the lower order modes. For this reason, we do not explicitly do this but instead reconstruct the temporal patterns of the components of surface velocity that are a function of modes 2 and 3 combined. The consequent 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.

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.



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).

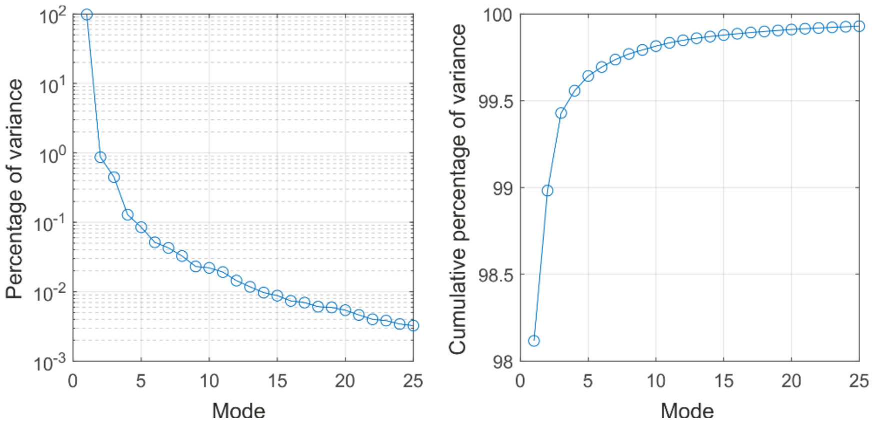
According to Figure 3 panel A the six modes identified only account for a combined ~ 75% of the time series data variance. Is this correct? Is the other 25% of the data variance noise? If so, to me it seems like modes-2-6 are below the error threshold. This ties into the previous question, but if this 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.

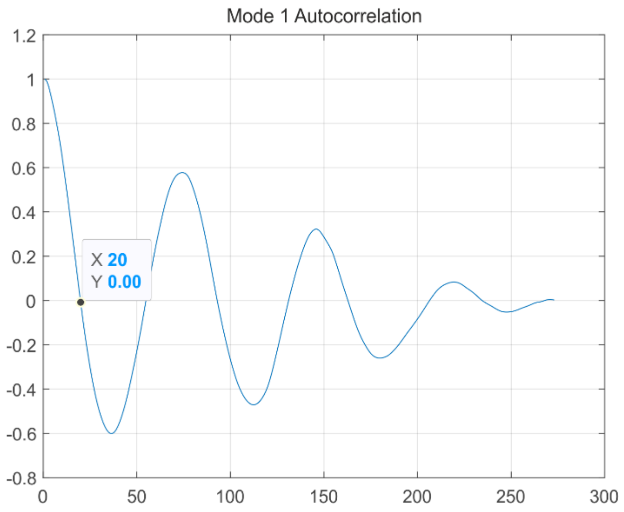
In a revised manuscript the action would be to clarify the language around singular values and eigenvalues, replace usage of *S(%)* with *Var(%)* in Equation 4 and throughout manuscript.

Line by line comments:

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

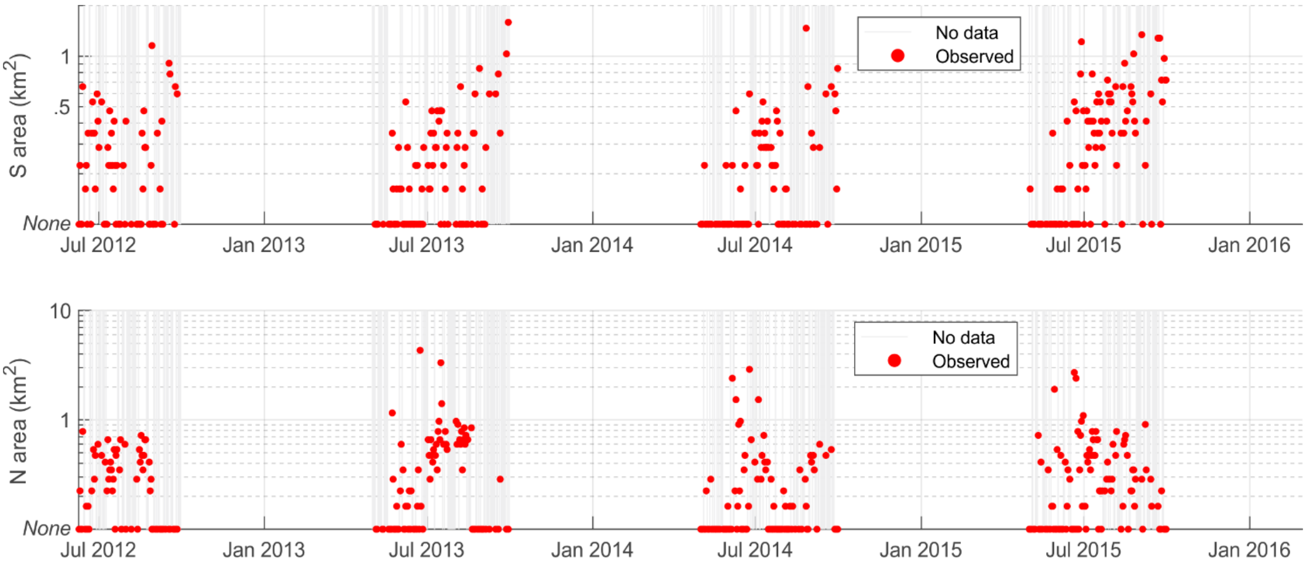
Using an autocorrelation approach (integral length scale) we determine convergence of the dataset, rather than a variance threshold. In the figure below we show the Mode 1 integral length scale of ~20, following the criterion of

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.



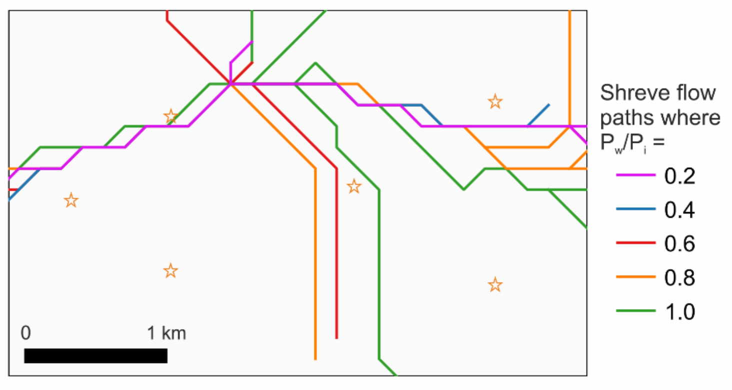
200: Did you visually check to determine whether the 5% threshold rejects questionable images?

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.



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.



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.

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.

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.

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

Can revise.

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.

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.

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-interpretating 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.

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.