The Cryosphere Discussions

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Comment on tc-2021-184

Anonymous Referee #1

Referee comment on "Proper orthogonal decomposition of ice velocity identifies drivers of flow variability at Sermeq Kujalleq (Jakobshavn Isbræ)" by David W. Ashmore et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-184-RC1, 2021

Review for:

Proper orthogonal decomposition of ice velocity identifies drivers of flow variability at Sermeq Kujalleq (Jakobshavn Isbrea) 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.

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

Line by line comments:

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

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

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?

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.

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

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.

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

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

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).

General comment: Why did you chose these point locations?

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.

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