Response to Reviewer Comments - Speed-up, slowdown, and redirection of ice flow on neighbouring ice streams in the Pope, Smith and Kohler region of West Antarctica

We thank the reviewers for their time and effort in reviewing our paper, "Speed-up, slowdown, and redirection of ice flow on neighbouring ice streams in the Pope, Smith and Kohler region of West Antarctica", submitted for publication in the cryosphere. We welcome the positive feedback and insightful comments which we have endeavoured to fully address in this resubmitted revision, and we hope you agree this improves the manuscript. We have incorporated the suggestions made. The changes are highlighted in the manuscript. Please see below a point-by-point response to the reviewers' comments, where all line numbers refer to the revised manuscript file.

Line	Comment	Response
Reviewer #1		
1	My comment #7 around agreement with MEaSUREs. Thanks for the explanation here. Do you know why the differences are so large for the Vane Glacier, and is it significant? Did the authors include in the updated manuscript a comment on the data in the table provided in their response document? I couldn't see it, but I think both the table and the text provided would be helpful additions to the supp info.	**Done.** We thank the reviewer for their suggestion of including the additional information in the supplementary materials. We have added this new table. Regarding Vane Glacier, it is a much smaller ice stream (~half the size and speed of the next smallest stream) and is highly constrained by the surrounding topography. As described in the paper the errors in the ice speed products are higher in highly crevassed regions such as shear margins and can be a characteristic of smaller outlet glaciers such as Vane where the tracking algorithms don't perform as well. We see this in our spatially variable error estimate; however, it is not captured in the MeASUREs error product. The spatial resolution of both ice speed datasets is different, so given the smaller width of this ice stream there may be an element of spatial aliasing where slower moving areas are incorporated into the mean around the central faster flowing stream. This is more likely to affect the coarser resolution MeASUREs product so we would expect the difference to be greater here. We used a linear fit in all ice speed trend analyses to minimise the impact of any offset between the two speed products. **Edit Supplementary:** We have now included the response to comment 7 in the supplementary information including adding the comparison table as Table S3.

		Comparison of the Sentinel-1 and MeASUREs ice speed data overlap period between 2016 and 2017 We compared the two ice speed datasets in this time period in the 2.5 km diameter regions at the grounding line of all 8 glaciers (Table S3). Overall, the Sentinel-1 velocity measurements are slightly faster than MeASUREs result on all glaciers, with an average speed difference of 21 m/yr (5%) in 2016 and 17 m/yr (3 %) in 2017. If we remove the slower flowing Horrall and Vane glaciers which flow at 401 and 203 m/yr respectively, the absolute difference reduces slightly (19 and 13 m/yr respectively), but the percentage difference reduces substantially to 2 and 1 % for 2016 and 2017 respectively. This is well within the error on our speed measurements. The majority of this speed difference is likely caused by differences in the underlying spatial resolution of the satellite data and the step and window size used for the feature tracking. It is well known (Lemos et al., 2018) that finer spatial resolution satellite datasets allow you to track ice speeds at high spatial resolution which then detect small regions of fast flow. Equally using a larger window and step size in the feature tracking step will tend to effectively smooth the output ice speed result which subtly reduces the average mean speed, and it also tends to increase spatial coverage slightly. As we move into an era with more SAR satellites that enable us to track ice speed at different resolution from different sensors for any one location, it will be increasingly important to characterise these differences, in the same way the satellite altimetry community is already doing for laser and multi-frequency radar altimetry products. There is also a slight difference in the time periods covered by the two products: Sentinel runs Jan-Dec whereas MeASUREs runs from July to June. We know from IMBIE studies that any difference in the spatial and temporal domain of different datasets can result in differences between them. These differences may be due to error in the products; however, they may also be due to real geophysical change that

		occurs between the time periods. We used a linear fit in all ice speed trend analyses to minimise the impact of any offset between the two speed products. **Table S3**. Comparison of the Sentinel-1 and MeASURES ice speed data overlap period between 2016 and 2017

Time Period	Horrall	Kohler West	Kohler East	Smith West	Smith East	Pope	Vane	Haynes	Average (all)	Average (fast flowing)
MeASUREs minus S1 (m/yr)										
2016	-17	-18	-28	-8	-28	-14	-42	-15	-21	-19
2017	-33	-3	-28	-12	-6	-2	-21	-29	-17	-13
Average S1 Speed (m/yr)										
2022	401	715	1215	1188	1093	772	203	810	800	966
Difference between Measures and S1 speeds as % of 2022 speed										
2016	-4	-3	-2	-1	-3	-2	-20	-2	-5	-2
2017	-8	0	-2	-1	-1	0	-10	-4	-3	-1

2	My comment #14. Sorry, I'm still a bit hazy on the edit at L220. Do you mean something like: "We note periods of rapid speed-up 2005-2011, with an average speed difference across all ice streams of 14%. During 2014-2017, the average speed increased by 12%, although there were periods of slow down between 2011-2013 (4%) and 2017-2020 (2%)…" Perhaps the speed up percentages could be reported as +% and the slow down negative -% to better differentiate.	**Done.** **Edit L162:** 2011 to 2013 (-4%) and from 2017 to the end of the study period in 2022 (-2%)
Reviewer #1		
3	I thank Selley et al., for taking into consideration all of my (and other reviewers) suggestions. The authors made a great effort at revising the manuscript to incorporate all of the comments. There are some remaining comments to be addressed, which, when done so, I am sure the manuscript will be ready for publication. My only major comments are on the discussion sections 4.3 and 4.4. I think section 4.3 would be improved upon if the authors incorporated more studies to strengthen their speculations regarding the stability of Dotson and Crosson ice shelves and/or included other potential mechanisms that could be at play (though not analyzed in this study). For section 4.4, I still found the discussion around MISI to be unclear. Could it	**Comment.** We thank the reviewer for their insight and suggestions on the manuscript. We have endeavoured to strengthen the sections they're highlighted and have and incorporated their suggestions as outlined in the responses below.

	happen in the PSK region? If so, on what timescale (Reed et al., 2024 suggests decadal timescale is possible)? Please also see my comment in the section below.	
4	Lines 46-48 – is the time period of the basal melt rate the same as the thinning rate? If not, can you specify? Thanks!	**Done.** **Edit Line 48: "**with average basal melt rates of 5.4 ± 1.6 and 7.8 ± 1.8 m/yr between 1994 and 2018 (Adusumilli et al., 2020),"
5	115-116 –Here, you say you have annual resolution for the 17.5-year study period but in the sentence before (line 112) you say 2009- 2019, so what did you use for the other 7.5 years?	**Done.** **Edit Line 115: "**2005 to 2022 using Moderate Resolution Imaging Spectroradiometer (MODIS) imagery"
6	128-130 –These lines may fit better in the methods section.	**Done.** **Edit Line 105: "**For time series analysis, we extract ice speed measurements along flowline transects located on the fast-flowing central trunk of all 8 ice streams (Fig. 1a and Fig. 2) and compute grounding zone time-series averaging within 2.5 km diameter circles where the flow lines intersect with the grounding line (Rignot et al., 2016)."
7	138 – I would find it helpful if you have "Measures period 2005-2022" in this section, since the other dataset is referred to as "Sentinel-1 period 2015-2022" for consistency and clarification.	**Done.** **Edit Line 139: "**from all speed data (MeASUREs and Sentinel-1) 2005 to 2022 and for the Sentinel-1 period 2015-2022 across the PSK region"
8	198-199 – I would suggest including more of this discussion regarding damage and buttressing in your discussion section. (See comment for lines 238-240).	Please see response to comment 11.
9	Figure 5 –The figure caption is missing information on panel C. Also, for panel C the color of the font, location of the rift outlines are a bit confusing, I suggest moving the text of the years and/or making the text a different color.	**Done.** We thank the reviewer for highlighting this oversight and have amended the caption. Regarding the recommendations for the rift colours/text we have tried to make our figures as clear as possible and the text describes the narrative of the crack formation and therefore, we have not changed the figure further. We can assure the reviewer that we have tested many options here and we do feel the current version displays the information most clearly. **Edit Figure Caption 5:** "**(B)** Zoom of Crosson Ice Shelf calving front locations from 2005-2022**. (C)** Location of the rift approaching bear island through the Sentinel-1 period (2015-2022).

10	222-225– Here, the font size changed in text	**Done.** **Edit Line 222-225:** Font size changed to match.
11	238-240 –Here you discuss the potential of Crosson, rebuttressing, however in the results (see comment on lines 198-199) you mention that there is potentially reduced buttressing from damage. How do you reconcile these two potential results, and can you expand a bit more in this paragraph on that?	**Done.** We have provided additional discussion of the potential variation in buttressing due to damage but also potential pinning points as the reviewer recommended. **Edit L241:** Change in ice shelf pinning can cause destabilisation through reducing the structural stability of the shelf when they are in advanced stages of thinning (Benn et al., 2022) but also by potentially initiating calving events (Arndt et al., 2018). These processes may impact the ice shelf at different times in different regions, potentially driving some of the observed speed change.
12	Discussion is overall much easier to read with new subheadings and strengthened text. Section 4.2 –I really appreciate how you've strengthened this paragraph!	**Comment.** We thank the author for their insights and suggestions which led to the strengthened discussion.
13	Section 4.3 – I agree that your results are compelling, regarding the (de)/stabilization of the ice shelves, however, I would like to see a bit more discussion about other potential mechanisms (e.g. basal melting, wave action, etc.) and how the migration of the ice divide interplays with those other processes. Though this paper below is still a preprint, I don't suggest citing it, but rather using it to inform more of your discussion on this topic, perhaps they have relevant references for this section of your discussion: Wild, C., et al. "A Tale of Two Ice Shelves: Competing Glacial Dynamics During the Unpinning of the Dotson-Crosson Ice Shelf System, West Antarctica." Authorea preprint (2024)	**Done.** We thank the reviewer for highlighting this preprint to us and offers further insight to the dynamics at Crosson and Dotson. However, as this preprint only became available several months after the submission of our paper it did not inform the results of this study which is why we haven't cited it. We look forward to seeing the final published version of his paper and to having discussions about the complementary nature of our work. We have taken this feedback onboard and added some text on further potential mechanisms. **Edit Line 280: "**There has been significant variability in sub-shelf melt rates in the Crosson Ice Shelf spatially and temporally over the past 20 years, with melt peaking in the early 2010s (Jenkins et al., 2018). Additionally changes to pinning points can further cause destabilisation through initiating calving events (Arndt et al., 2018) and in advanced stages of thinning (Benn et al., 2022). Further work exploring the chronology of changes to stress structures on the Crosson Ice Shelf would be invaluable to further establish the interplay of these driving mechanisms."

14	Lines 286-288 –As stated in my last review, I really think it's important for the authors to address the decadal timescale of which MISI occurred on PIG that Reed et al., 2024 investigates. I am only requesting a few sentences and to tone-down the conclusion that MISI may not be a major dynamical factor at short timescales, especially when this is not a primary component of the study. I am disappointed to see that they did not address this in the last round of revisions. I strongly encourage them to do this when they next revise the paper.	Done. We thank the reviewer for their insight and have amended the text to include Reed et al., 2024 and nuance the strength of the MISI argument. We do think there is value in including MISI discussions as it has the potential to impact the glaciers in this region and we think our results will be a useful contribution to future studies on this topic. **Edit Line 297: "**short (sub-decadal) timescales across the PSK region. Conversely, more recent modelling work based at Pine Island Glacier indicates irreversible grounding line retreat between the 1940s and the 1990s is an example of MISI with recent changes being primarily driven internally (Reed et al., 2024)." **Edit Line 305**: "or it could be driven by internal ice dynamic feedbacks (such as MISI)(Reed et al., 2024)"
15	Line 301 – I would suggest adding the line about thinning reducing driving stress earlier in the discussion and not only including it in the conclusion.	**Done.** **Edit line 252: "**The region continues to thin and therefore thinning induced reductions in driving stress may have contributed to the observed slow down."