Two decades of dynamic change and progressive destabilization on the Thwaites Eastern Ice Shelf

List of changes in the manuscript:

Note: Line numbers refer to the track-changes version of the manuscript.

Changes to figures:

Figure 1: Added 2000 grounding line, adjusted box sizes to 5 km x 5 km and updated location of Site 1, updated caption accordingly.

Figure 2: Updated dataset

Figure 3: Updated dataset, increased text size

Figure 4: Updated dataset, increased text size, added grounding lines to MODIS images, added labels for ovals, updated caption

Figure 5: Updated dataset, increased text size

Figure 6: Updated dataset, increased text size, clarified caption

Figure 7: Updated interpolation and color bar label, updated caption

Figure 8: Updated caption

Figure 9: Updated dataset

Changes to the text:

100-101: Added text emphasizing comparison of short-term combined record to Sentinel-1 only record

113-118: Added text describing the masking of MODIS data above the grounding line

129-131: Added citations demonstrating the reliability of MODIS data for velocity records

144-145: Described additional filtering for the long-term velocity record

234-237: Clarified use of ASAID grounding line product

294-297: Clarified terms of the mass conservation equation used to calculate basal melt

Section 3.1 (305-403): Adjusted text to properly describe observations of averages within boxes, now that they are larger and the grounding zone site has been moved. Removed mentions of inaccuracies in MODIS data above the grounding line, as these data have now been removed from the record.

Section 3.2 (403-440): Clarified description in text of difference between our short-term combined record and the Sentinel-1-only record

454-455: Noted interpolation method

464-466: Clarified possible reasons for the surprising mid-shelf freeze-on signal

469: Clarified inclusion of ice thinning/thickening in calculations

494, 509, 524, 532, 534: Added reference to numbered dashed ovals in Figure 4

584: Clarified inclusion of ice thinning/thickening in calculations

596: Clarified possible reasons for the surprising mid-shelf freeze-on signal

References: Added citations

Response to reviewer comments

Reviewer comments in italics; Author responses in normal font

Reviewer 2:

Reviewer comment: *Alley et al. have addressed or corrected some of my comments regarding data processing and error analysis.*

Nevertheless, I still have concerns where the authors provided inadequate responses to my comments and I would still recommend major revisions to address them.

First, regarding the datasets used for the analysis, the authors explained that they are not using published NSIDC datasets to complete the period 2000-2012 because the resolution is too coarse (1km) compared to their 500m sampling resolution. I would like to point out that if their sampling resolution is 500m, the true spatial resolution of displacement maps obtained from MODIS are certainly much coarser. Typically, the native resolution of MODIS is 250m, which would mean that cross-correlation from PyCorr is done on 2x2 pixels subimages to match the final reported resolution, which seems unrealistic, if not impossible. In addition, this NSIDC time-series https://nsidc.org/data/NSIDC-0545/versions/1 provided maps in 2000, 2006-2013 at a sampling resolution of 450 m. While I agree that the period 2001-2005 is not covered in the dataset and that MODIS could be useful on the ice shelf to bring additional information, I would still think that this external dataset would have proven useful.

Author response: We appreciate the reviewer's efforts to recommend good datasets that could improve our analysis. We agree that the InSAR time series available from NSIDC has value. However, it is not just the resolution that is the problem, but also the data coverage.

First of all, the interpretation of our processing resolution presented here is incorrect. Feature tracking does rely on matching subimages, often referred to as reference chips, from the first image to search chips defined in the second image. However, the size of the reference chip does not determine the resolution of the final product. The reference chip group of pixels is unique to each flow vector determination, and the final resolution depends on the increment (or grid spacing) at which each displacement vector calculation is made. This is similar to the determination of other common neighborhood operations such as gradients or surface slope, at a posting spacing up to the pixel resolution of the input raster. This is outlined in Fahnestock et al. (2016) in reference to PyCorr specifically, and the general method is described in many other papers and remote sensing textbooks. We calculated our PyCorr image correlations every two pixels in the input MODIS images, yielding a 500 m spatial resolution.

Second, the problem is not just resolution, but also data coverage. The dataset recommended here is available in 2000, 2002, and 2006-2012, but there is no or almost no coverage on the TEIS in 2000, 2002, 2011, and 2012, and some missing coverage in the middle of the shelf in 2006. As the remaining velocity maps are only available during years where our data provide complete coverage of the shelf, and as our conclusions are already strongly supported by our data, we do not believe that additional datasets are necessary.

Reviewer comment: *Thus, even after the review, I am not convinced by the MODIS result, which also casts doubt on the strain rate analysis. It is clear that the results obtained from MODIS are wrong in many places and remain fuzzy and patchy in others. I have attached the 2006 velocity map at 450 m resolution published by NSIDC, using the exact same color coding between 0 and 3 m/day that in Figure 4, so that the maps can be compared with the result from 2005-2006 in Figure 4 from the authors. The 2000-2001 and 2005-2006 speed maps from Alley et al. indicate that the ice is not moving upstream of the GL but also near the GL on floating ice. One clear example of this issue is the main ice tongue not moving near the GL in 2005-2006 while it should be moving at 2 km/yr or more than 5 m/day. The resulting strain maps calculated from these erroneous displacement maps are therefore also erroneous. Abnormally high strain rates are visible near the GL in 2000-2001 and 2005-2006, but still are highlighted (dashed black ovals in figure 4 for example) as features showing the evolution of strain on TEIS. This sensor-dependent issue is also evident in Figure 3 near the GL and on the grounded ice where the patterns of speed and strain change as soon as the authors include Sentinel1 results. As mentioned, this also raises questions about the quality of data obtained further from the grounding line and the interpretation that can be made from these very uncertain measurements.*

Author response:
Feature tracking is well-established and has been repeatedly validated. We are using the same software as used in the GO-LIVE project (Fahnestock et al. 2016). Furthermore, many studies (e.g. Haug et al. 2010, Chen et al. 2016, Greene et al. 2018) have demonstrated the reliability of feature tracking on MODIS data specifically on Antarctic ice shelves, where large features are successfully tracked, even with the relatively low spatial resolution of MODIS data. We have added mentions of these citations in lines 129-131 in the manuscript. We have quantified both the absolute error in individual image-pair correlations and the empirical uncertainties associated with our combined images. Our conclusions rest upon signals that fall well outside the range of this uncertainty.

However, Reviewers 2 and 3 both point out that the incorrect MODIS results above the grounding line (due to the fixed nature of some surface features on grounded ice, e.g., undulations induced by ice-bedrock interaction) are distracting, which is a reasonable concern. The effect that the reviewer points out here was discussed in the previous submission in lines 104-109, 312-316, and 346-349; it is both expected and acknowledged in the manuscript. Our conclusions do not depend on the data in these areas. Furthermore, we circled this area in Figure 4 to point out this expected discrepancy, not to use it in our analysis. However, we see the reviewers' points that a quick read or glancing through the figures without reading the text could cause a misleading interpretation of our data, and we certainly wish to avoid that. We therefore agree that it is best to mask these areas out in the MODIS data.

Unfortunately, implementing this is not easy, which is why we had originally allowed these data to remain. As shown in Figure 1, the grounding line on the TEIS changes rapidly and dramatically through our time period of analysis. However, there are no annual mappings of the grounding line published, nor are there enough data available to produce annual mappings. Since our time series is annual, we cannot accurately mask at the grounding line. We have therefore taken a conservative approach, masking the MODIS data above the 2000 InSAR grounding line (Rignot et al. 2016) for data between 2000 and 2004, above the ~2004 ASAID grounding line (Bindschadler et al. 2011) for data between 2004 and 2011, above the 2011 MEaSUREs grounding line (Rignot et al. 2016) for data between 2011 and 2017, and above the 2017 InSAR grounding line (Millillo et al. 2019) for the rest of the record. In addition, we imposed a minimum speed of 0.4 m/day for all MODIS data, which is more than twice the minimum velocity shown in the upstream Landsat data in the latter part of our record. We described this in lines 113-119 in our manuscript. These measures are likely to remove more data than strictly necessary. However, because we cannot accurately identify annual grounding line change, we take a conservative approach in order to increase confidence in the remaining data. In addition, in response to both this review and Reviewer 3's suggestions to be more aggressive in our filtering, we increased the size of our median filter from 3x3 to 7x7, and increased the minimum threshold for correlation strength and difference in correlation strength from neighboring options.

The reviewer is concerned that the MODIS data are both "fuzzy and patchy," and encourages us to more rigorously filter the data. We have carried out this more rigorous filtering as described above. However, these actions will, by their very nature, make the data both more fuzzy, as it suppresses variability and smooths the data, and patchy, as it eliminates data coverage in some areas during the first few years of our record, when data availability is limited. There is no valid way to address all these concerns with the available data. However, **these changes did not alter any of our conclusions.** The remaining data in our view can be used with confidence, including the area above the grounding line. We believe these data are sufficient for the needs of this paper and for future work with the dataset.

Reviewer comment: *At minimum, the authors should try to filter out the maps from 2000 and 2012 for spurious measurements, and state the real resolution of their mapping with MODIS. With the filtered and correct speed maps, they could then correct the lagrangian elevation changes, correct the strain rate calculation by updating Figure 2, Figure 3 and Figure 4, especially near the GL and on grounded ice, and adjust, if needed, their interpretation of the* *evolution of the strain on TEIS. I believe that it is important that the primary data source (ice displacement) is properly and correctly established, otherwise all the other observables (strain, melt) will be wrong and misinterpreted.*

Author response: The resolution of the MODIS mapping that we have presented is the real resolution. We are using established methods (based on correlation strength and difference between correlation strength and nearby correlations) for filtering out spurious measurements, which were described in lines 130-134 in our previous submission, and are highlighted in lines 141-145 in our current submission. We present empirically derived uncertainties, as well as error estimates from individual image correlations, using well-established methodologies. Our conclusions rest on results that are well above these error and uncertainty measures. We believe that our filtering routines are sufficiently rigorous and scientifically appropriate for MODIS-derived velocity data, and we have clarified that many other studies have successfully used these data for velocity correlations. We additionally have modified the presentation of the data to make the figures more easily interpreted by a casual reader. Furthermore, we have demonstrated that MODIS is the best available for the full period of our study, and in many years the only available dataset to fit the needs of this study. Because of these reasons, and because our results show signals far above the noise present in the measurements, we believe our velocity data and methods are sufficient for this study and beyond.

Reviewer comment: *My second remark concerns the 1996 and 2000 grounding line (GL) from InSAR. I would strongly suggest adding it in the manuscript. Not adding the 1996 GL because it does not overlap their analysis is questionable, but it is their choice. Nevertheless the InSAR grounding line has also been mapped in 2000 which is part of their analysis. In addition, the 2004 grounding line seems to be collected between 1999 and 2003 rather than 2004 as stated here https://www.usap-dc.org/view/dataset/609489 or https://nsidc.org/data/nsidc-0489. Please clarify at which date or period (or the source if not ASAID from Bindschadler et al. (2011)) is their 2004 GL in Figure 1*

Author response: The dates mentioned in the NSIDC description of the Bindschadler et al. (2011) grounding line dataset do indeed make the data collection years unclear, but the description in the Bindschadler at al. (2011) manuscript is much clearer. The dataset is derived from Landsat imagery from 1999-2003, and from ICESat laser altimetry from 2003-2009. The 2004 date we use is the center point of that time period. We have clarified this in the text in lines 236-237. The 1996 grounding line remains outside our study period, and we see no relevance in adding it to the already busy figures. The 2000 grounding line is discontinuous and covers only half of our study area as shown in Figure 1, which is why we had originally declined to use it. However, we have now added it to Figure 1, and used it for masking the MODIS data above the grounding line as described above.

In the figure above, I plotted the GL from ASAID (Bindschadler et al. 2011) as a thick dark grey line and from InSAR as thin colored lines. It appears not only that the ASAID GL seems off by several kilometers in many places but also that having the complete evolution from 1996 to 2017 does not seem to be less relevant to their analysis.

Author response: As we have shown that the Bindschadler et al. (2011) grounding line is not precisely coincident in time with any of the InSAR grounding lines, it would be surprising if it exactly matched any of them in this rapidly evolving region.

Reviewer comment: *Finally, the authors mentioned in their responses that the lagrangian analysis for calculating elevation changes requires a gridded dataset, which I believe is not true. There are many studies published using lagrangian approach on non-gridded datasets such as ERS, ENVISAT, IceSAT or CryoSAT (Adusumilli et al. 2020; Moholdt et al. 2014), some of them would have had sufficient resolution to capture the TEIS evolution (Gourmelen et al. 2017).*

Author response: The reviewer makes a good point, and we certainly should have been clearer in our explanation. We meant that Lagrangian approaches require *interpolation* of some sort, which is often achieved through the use of a gridded dataset. A high-resolution DEM such as REMA will have the highest accuracy in a Lagrangian analysis, as interpolation is minimized and is therefore ideal to accurately infer changes on the ice-shelf surface. As TEIS has extremely complex basal topography, particularly in extensive heavily crevassed areas, we are convinced this is the only method to give us the results we need for this study. Adusumilli et al. (2020) and Gourmelen et al. (2017) began with an altimetry dataset, but created a gridded dataset from it, which is functionally the same approach that we used, but the resulting reference grid is at a much lower resolution than the REMA grid we used (8m). Moholdt et al. (2014) used a "nearest neighbor" approach, where the nearest altimetry point to an advected location is used to calculate the elevation difference. In another example, Sutterley et al. (2019) was similar but used a "triangulated irregular network" approach with airborne altimetry measurements. In other words, the locations were interpolated rather than the surface elevation value. This is often the only reasonable option when data are sparse, but it again will result in a much lower resolution/spatial coverage and lower accuracy result than the approach we have used with REMA. Furthermore, REMA has become the community's standard product for ice-surface elevation and we certainly see value in pointing out the changes that have occurred since its release in 2018.

Works cited in response to Reviewer 2:

Adusumilli, S., Fricker, H.A., Medley, B. *et al.* Interannual variations in meltwater input to the Southern Ocean from Antarctic ice shelves. *Nat. Geosci.* **13,** 616–620 (2020). https://doi.org/10.1038/s41561-020-0616-z

Bindschadler, R., Choi, H., Wichlacz, A., Bingham, B., Bohlander, J., Brunt, K., Corr, H., Drews, R., Fricker, H., Hall, M., Hindmarsh, R., Kohler, J., Padman, L., Rack, W., Rotschky, G., Urbini, S., Vornberger, P. and Young, N.: Getting around Antarctica: New high-resolution mappings of the grounded and freely-floating boundaries of the Antarctic ice sheet created for the International Polar Year, The Cryosphere, 5(3), 569–588, https://doi.org/10.5194/tc-5-569-2011, 2011.

Chen, J., Ke, C., Zhou, X., Shao, Z., and Li, l. "Surface velocity estimations of ice shelves in the northern Antarctic Peninsula derived from MODIS data." *Journal of Geographical Sciences*, 26: 243-256, (2016).

Fahnestock, M., Scambos, T., Moon, T., Gardner, A., Haran, T. and Klinger, M., 2016. Rapid large-area mapping of ice flow using Landsat 8. *Remote Sensing of Environment*, *185*, pp.84-94. https://doi.org/10.1016/j.rse.2015.11.023

Greene, C.A., Young, D.A. Gwyther, D.E., Galton-Fenzi, B.K., and Blankenship, D. "Seasonal dynamics of Totten Ice Shelf controlled by sea ice buttressing." *The Cryosphere*, 12: 2869-2882, (2018). https://doi.org/10.5194/tc-12-2869-2018

Gourmelen, N., Goldberg, D. N., Snow, K., Henley, S. F., Bingham, R. G., Kimura, S., … van de Berg, W. J. (2017). channelized melting drives thinning under a rapidly melting Antarctic ice shelf. *Geophysical Research Letters*, 44, 9796– 9804. https://doi.org/10.1002/2017GL074929

Haug, T., Kääb, A., and Skvarca, P. "Monitoring ice shelf velocities from repeat MODIS and Landsat data – a method study on the Larsen C ice shelf, Antarctic Peninsula, and 10 other ice shelves around Antarctica." *The Cryosphere*, 4: 161-178, (2010). https://doi.org/10.5194/tc-4-161-2010.

Milillo, P., Rignot, E., Rizzoli, P., Scheuchl, B., Mouginot, J., Bueso-Bello, J. and Prats-Iraola, P.: Heterogeneous retreat and ice melt of Thwaites Glacier, West Antarctica, Sci Adv, 5(1), eaau3433, https://doi.org/10.1126/sciadv.aau3433, 2019.

Moholdt, G., Padman, L., and Fricker, H. A. (2014), Basal mass budget of Ross and Filchner-Ronne ice shelves, Antarctica, derived from Lagrangian analysis of ICESat altimetry, *J. Geophys. Res. Earth Surf.*, 119, 2361– 2380, doi:10.1002/2014JF003171.

Rignot, E., Mouginot, J. and Scheuchl, B.: MEaSUREs Antarctic Grounding Line from Differential Satellite Radar Interferometry, Version 2, NASA National Snow and Ice Data Center Distributed Active Archive Center, https://doi.org/https://doi.org/10.5067/IKBWW4RYHF1Q, 2016.

Sutterley, T. C., Markus, T., Neumann, T. A., van den Broeke, M., van Wessem, J. M., and Ligtenberg, S. R. M.: Antarctic ice shelf thickness change from multimission lidar mapping, The Cryosphere, 13, 1801–1817, https://doi.org/10.5194/tc-13-1801-2019, 2019.

Reviewer 3:

Reviewer comments in italics; Author responses in normal font

Reviewer comment: *As a 3rd pair of eyes as a new reviewer, I have focused mostly on reading the arguments of previous reviewers while simultaneously having a fresh look at the paper.*

In this review analysis I agree with the previous reviewers that i) the paper is important and ii) the results are sound. I do however also agree with R2 that many of the data sets and results are presented as such without being extremely careful in their interpretation which partly weakens the credibility of the results. If the same results are presented more rigorously, the paper would be more convincing. Moreover, I think many of the figures can be improved to increase readability.

I also don't think the author's response properly addresses the weak points identified by R2 and another major revision might be needed.

Author response: We thank the reviewer for taking the time to add a third perspective to our paper. We have done our best to address all comments from Reviewer 2 and from this review to enhance the presentation of our results while maintaining the scientific quality and integrity of our work. We have taken several steps to improve the presentation of the MODIS data, as outlined in the comment responses below, including masking the data upstream of the grounding line, imposing a minimum speed for MODIS correlations, increasing the size of our median filter, and increasing the size of the sample boxes that are averaged and plotted in Figures 2 and 5. In addition, we have interpolated the data in Figure 7 to make the data presentation clearer, and we have clarified our text to emphasize that dynamic ice thinning/thickening was already included in our basal melt rate equation (Equation 3). Finally, we have done our best to answer each minor comment and update the figures and clarify the text accordingly.

Reviewer comment: *MAJOR COMMENTS:*
- I agree with the authors that the MODIS velocity data set is an important data set that can fill in several gaps that cannot be obtained by other existing data sets, it still seems that the MODIS data product is far from perfect with several patches of speed and direction that seem outliers and therefore incorrect (both in Fig 4 + 9). This makes the interpretation of derived strain rates, flow directions dubious. Etc. I would recommend to do a much more rigorous velocity data filtering / post-processing to identify the source of these errors and mitigate, filter it out.

Author response: We have taken several steps to improve the quality of the MODIS data. First, we have done our best to manually mask out the MODIS data upstream of the grounding line, where we do not expect it to be accurate. We initially included these data because annual grounding lines are not available for masking and our analysis focuses only on the floating ice shelf. However, we accept the reviewer's point (also expressed in a comment below) that these data can be distracting and confusing, particularly if one were to only look at the figures in the paper without reading the main text. We therefore masked the MODIS data above 2000 InSAR grounding line (Rignot et al. 2016) for data between 2000 and 2004, above the ~2004 ASAID grounding line (Bindschadler et al. 2011) for data between 2004 and 2011, above the 2011 MEaSUREs grounding line (Rignot et al. 2016) for data between 2011 and 2017, and above the 2017 InSAR grounding line (Millillo et al. 2019) for the rest of the record. In addition, we imposed a minimum speed of 0.4 m/day for all MODIS data, which is more than twice the minimum velocity shown in the upstream Landsat data in the latter part of our record. We describe this in lines 113-119 in our manuscript. These are both conservative actions, which are likely to remove more data than strictly necessary. However, because we cannot accurately identify annual grounding line change, we would rather take a conservative approach in order to have as much confidence as possible in the remaining data.

Our original data were filtered using a 3x3 median filter and by imposing minimum threshold values on the feature correlation strength and on the difference in correlation strength between successful correlations and neighboring options. These are well-established and acceptable methods for filtering velocity fields produced through feature tracking (e.g. Fahnestock et al. 2016). In response to the reviewer's concerns, we have increased the size of our median filter to 7x7 and increased the minimum thresholds for correlation strength and correlation difference. In addition, we increased the size of the squares we use for average values in Figures 2 and 5 from 3x3 km to 5x5 km in order to reduce any possible effects of noise in our analysis. We note that the more rigorous filtering has caused some data gaps early in the record, particularly right at the grounding line, so we had to shift our grounding line box to an alternative location.

Overall, these measures have increased the quality of the data above the grounding line, increased the size of patches in the first few years of the record that lack any data, and decreased the amount of detail in our long-term velocity record. While this does have some disadvantages, we believe that it overall improves our velocity record. Furthermore, these adjustments did not alter any of our conclusions, as they were well outside any noise in the record before this additional filtering, and are robust to these changes.

Reviewer comment: *- Once the TEIS-TWIT shear margin disintegrated, I don't think it is fair anymore to calculate the strain rate and/or interpret it. This has become a loose connection of icebergs that do no longer strain each other.*

Author response: Masking out data in the shear margin presents a similar difficulty to masking out data above the grounding line: we do not have any objective way of delineating where coherent ice-shelf ends and mélange begins. However, in this case the data calculated in the shear margin are not necessarily incorrect, they just represent something different. Several studies have shown that ice mélange, such as the iceberg debris in this shear margin, plays a role in affecting backstress at tidewater glacier and ice-shelf fronts (e.g. Pollard et al. 2018; Cassotto et al. 2015). The strain rates calculated within this mélange are related to the amount of backstress it imposes on the intact ice shelf and upstream glacier. While this area is not directly part of our analysis in this paper, and therefore does not affect the results, we feel that removing it is not necessary and likely to be detrimental to future use of the dataset.

Reviewer comment: *- L310-315 and Fig.4: This is a nice illustration of the dubious results due to the low confidence MODIS velocity field (e.g. with land ice flowing upstream in 2001-2002, 2005-2006) and hence potential dubious interpretation of the strain rates. I think such low quality data should be removed.*

Author response: We agree, and have now removed these data, as described above. We do note that these data have never been part of our analysis.

Reviewer comment: *- Fig.5: what is the reason for the JJA2018 dip in mid-shelf long strain rate? I guess it noise (also visible in Fig.6), but that really makes me doubt the validity of the uncertainty intervals in Fig.5 (and of the uncertainty overall), especially as -8e-5 is way outside of the strain rates in Fig. 2 as well.*

Author response: That's a good question. This anomaly remains, even after our more aggressive filtering, although it is somewhat subdued after increasing the size of the boxes we are using for averaging. While it's possible it is noise, it is also possible that this is a real feature in response to an external forcing, such as the removal of fast ice during the spring season, which has been shown to influence the velocity of other ice shelves (e.g. Greene et al. 2018). A detailed analysis of the external factors that influence seasonal strain rates is beyond the scope of this manuscript, but we hope this will provide the foundation for future work. We have added a mention of this in lines 415-416 in our manuscript.

Reviewer comment: *- Given the large salt/pepper effect in Fig.6, I doubt what the added value of Fig.5 is as it only showing a constant + potential salt and pepper effects*

Author response: Based on this comment, we have clarified the text concerning Figures 5 and 6. Figure 5 shows averages from our lower resolution, long-term record. Figure 6 shows both our long-term record and a higher resolution record derived only from Sentinel-1. The salt and pepper effect is in the Sentinel-1 record, not in our long-term record, which is much smoother because it has the benefit of averaging data from several sources (Sentinel-1, Landsat, and MODIS). Figure 5 does not suffer from the salt and pepper effect. We have clarified this throughout section 3.2 in the text. In addition, as noted above, we increased the size of the boxes used for averaging in Figure 5 from 3 km x 3 km to 5 km x 5 km, which reduces the possibility that noise is significantly influencing those averages.

- L404-411: this is another dubious result. If the REMA analysis is incorrect here, why would it be correct elsewhere? Especially as it shows several areas that are remarkable but never mentioned. The positive basal melt rates for example could also indicate dynamic ice convergence etc. Just saying that REMA is wrong there (and only there) is a weak argument.

Author response: The REMA mosaic is a gridded dataset, and it is reasonable to expect that discrepancies are more likely at seams in a gridded dataset. While this may not be the whole explanation, we feel that it is an important aspect to mention. We have edited our text in lines 464-466 and 595 to qualify this statement and offer other explanations. However, this anomaly is not the result of dynamic ice convergence, because that is something we have explicitly accounted for in our calculations. The second term on the left-hand side in Equation 3 is the ice thickness multiplied by the vertical strain rate, which is ice thinning/thickening. We solve this conservation equation for basal melt rate alone, so we have already removed ice thinning/thickening from our results according to our best available data and methodology. We have clarified that we have accounted for this effect in lines 294-297 and 583.

Reviewer comment**:** *- Basal melt rate analysis: the authors attribute all changes to basal melting, but don't quantify the role of convergence/divergence or changes in firn air content etc. I think these should also be quantified if the role of basal melting is quantified. Given the slowdown near pinning point I would expect for example a lot of thickening due to compression, but this is never mentioned nor quantified.*

- Based on the previous comments I agree with R2 that the analysis of basal melting should be clarified with a better quantification of the uncertainty and inclusion of the patterns of thinning/thickening due to changes in SMB or con/divergence.

Author response: We have quantified convergence/divergence, firn-air content, and SMB, as explicitly stated in Equations 2 and 3. Our treatment of firn-air content is described in Equation 2, and we outlined our estimates of temporal variability and uncertainty in lines 267-274 in the first revision, and lines 282-288 in the current submission. The mass conservation in Equation 3 accounts for ice convergence/divergence in the second term on the left-hand side and surface mass balance in the first term on the right-hand side. The basal melt rates calculated from Equation 3 and shown in Figure 7 therefore represent only basal melt, according to our best available data and methodology - surface mass balance and convergence/divergence (which, when multiplied by ice thickness, estimate ice thinning/thickening) have already been removed. If the reviewer is referring to calculating ice-thickness advection, that would only be relevant in an Eulerian analysis. The Lagrangian method uses the total derivative following ice-parcel flow, which means that ice-thickness advection is already accounted for by taking measurements of change over the same column of ice. This is a standard methodology; we based our analysis on the descriptions of this method in Dutrieux et al. (2014) and Jenkins and Doake (1991), but many other papers have used the same calculations.

Reviewer comment: *- L461-470: based on the apparent noise in flow direction in the MODIS only record, I think it is over-ambitious to interpret the pre-Sentinel flow directions around the pinning point.*

Author response: Many studies (e.g. Haug et al. 2010, Chen et al. 2016, Greene et al. 2018) have demonstrated the reliability of feature tracking for ice-flow speed and direction on MODIS data on Antarctic ice shelves. In addition, our analysis of trends around the pinning point takes into account patterns that are consistent over many years and tens of kilometers, and the observed changes are consistent with patterns observed in speed, shear-strain rate, and visible changes in surface features in the shear zone. With these considerations and our own interpretations revealing very little noise around the pinning point, we find it reasonable that conclusions may be drawn from these patterns.

Reviewer comment: *- L 475-480: is the development of these rifts not the result of compression that result in transverse fractures?*

Author response: This section analyzes the observation of increased concentration of strain over time along individual rifts; we did not seek to provide a specific reason for rift formation in a fracture mechanics framework, which would be an appropriate topic for future work. However, as the rifts in question are not transverse to the axis of compression, we find it more likely that shear stresses are responsible for their initiation/propagation.

Reviewer comment: *MINOR COMMENTS:*
- Fig.2, 3, 4, 6: axes, labels, text is too small to read

Author response: We have increased the text size in all of these figures.

Reviewer comment: *- Fig.2 + 5: right column is not properly aligned with the rest*

Author response: Thank you; we have adjusted the alignment in both figures. Note that only the bottoms of the graphs are aligned. Because Matlab formats the graphs with the exponent (10^{-5}) above the graphs in the right column, the y-axis is slightly shorter for each graph than in the other columns.

Reviewer comment: *- Fig.4+8: please label the different ovals so it is clear what they mean and where they are discussed. I think the caption should also summarise what is seen in the ovals as to make it the reader easier and not requiring him/her to search.*

Author response: We have added numbers to the ovals in Figure 4, and now describe what these are showing in the caption and in the text. We have also added descriptions of the arrows and ovals in the caption for Figure 8.

Reviewer comment: *- Fig.4: adding grounding lines and velocity outline would help the reader to orient.*

Author response: We have added the 2011 grounding line to the MODIS images to facilitate comparison with the speed, direction, and strain rate maps.

Reviewer comment: *- Fig.7: axes/label problem: if this figure shows surface lowering than positive values should indicate lowering and negative values should indicate uplift, which is not the case*

Author response: We have adjusted the axis label to read "Surface height change," which is more generalizable.

Reviewer comment: *- Fig.7 shows a clear interpolation problem which should be masked*

Author response: We have fixed our interpolation, and described our updated method in the caption of Figure 7 and in lines 449-450.

Reviewer comment: *- I agree with R2 that overplotting of points in Fig.7 is misleading (as we the reader is biased by the last points being plotted that hide the points below). Re-gridding to a raster (with missing values outside the transect) would be more scientifically correct.*

Author response: As suggested, we have regridded these data to a raster over the transect paths using an inverse distance weighting approach, which is noted in the caption in Figure 7 and in lines 449-450.

Works cited in response to Reviewer 3:

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