The Cryosphere Discussions

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Reply on RC2

Karen E. Alley et al.

Author comment on "Two decades of dynamic change and progressive destabilization on the Thwaites Eastern Ice Shelf" by Karen E. Alley et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-76-AC2, 2021

Reviewer comments in Italics; Author responses in normal font

Alley et al. present a manuscript describing the evolution of the Thwaites Eastern Ice Shelf (TEIS). As Thwaites is a key glacier for understanding and predicting the future contribution of the West Antarctic Ice Sheet, this study of TEIS brings new information on its dynamic and geometric changes that are certainly important to the community. TEIS buttresses a large portion of Thwaites that has displayed only moderate dynamic changes compared to the main ice tongue. Losing this remaining "barrier" could mean a larger Thwaites debacle in the future and thus an increased contribution of the glacier to sea level rise. To study TEIS, the authors used remotely-sensed observations from MODIS, Landsat-8, Sentinel-1 to document its dynamic and complement them with elevation measurements from ICESAT-1&-2 and optical stereo elevation models from REMA to derive lagrangian elevation changes and basal melt.

If the approach is globally sound, I regret however that this study does not use the best existing data or methodology, and that the presentation of some results or the calculation of errors are not more careful because it may weaken the credibility of the results. Therefore I suggest a major revision of the manuscript before it is suitable for publication.

Author response: We thank the reviewer for spending the time to carefully read and consider our manuscript, and for endorsing our sound approach. We believe that, with further explanation, the reviewer will be pleased with our thoroughness in our current analysis, and agree that we have used the best available data for our specific aims. In the text below, the reviewer has identified one important method that we forgot to mention in the text, made an excellent suggestion for improving one of our figures, and requested further clarification about our error analysis methods. We offer improvements based on all of these comments. However, the reviewer's main comments also suggest adding 10 additional or extended datasets to our manuscript and focusing on alternative research goals. We sincerely hope that the reviewer or others will use many of these datasets in the future to carry out the suggested analyses, which are highly complementary to the work presented in this study. However, they are beyond the scope of this manuscript and would not alter our conclusions, which the reviewer agrees are important to the continued study of Thwaites Glacier.

Reviewer comment:

Speed observations:

While their results are interesting in documenting the progressive weakening of the floating ice shelf, I believe that the existing observations to analyze the TEIS dynamic evolution are underutilized. A large part of the analysis is based on the use of MODIS with a quite low spatial resolution. These data are used to calculate velocity changes, strain rate evolution but also to calculate elevation changes and submarine melt with a Lagrangian approach. However, one may wonder about the robustness of these calculations in view of the large errors associated with these measurements. With an error of several hundred meters per year for an ice shelf flowing at less than 1 km/year, the error on the flow direction is quite large (several tens of degrees). We can therefore question the validity of the measurements with the Lagrangian approach, as well as the calculations of strain rates. Even with filtering and large spatial smoothing, it is clear that the MODIS results form unrealistic patches where the flow direction and amplitude do not seem very homogeneous as it is visible in Figure 9 top-left. This is also visible in Figure 6 where the combined registration seems to bring many biases (especially in 2018) that are not present in the Sentinel-1 record alone. If the MODIS observations were the only ones available to document the velocities in the years 2000 to 2010, I would not see too many problems to use them as they would be the only existing source of information. But, as far as I know, there are many other instruments that allow measurements during this period (even if, of course, this would not match the amount of observations obtained during the last years with the Sentinels and Landsat). Thus the authors could have used higher resolution data from ENVISAT/ASAR, ALOS/PALSAR, RADARSAT, Landsat-7 (between 1999 and 2003) or even ASTER which are publicly available. Some of the speed measurements using these instruments are already available at NSIDC if I am not mistaken and should therefore be considered.

Author response: We explored the datasets suggested here during the initial preparation of our manuscript, and unfortunately they do not provide the coverage or accuracy that the reviewer is hoping for here. Data from ENVISAT/ASAR, ALOS/PALSAR, and RADARSAT, along with several other sensors, are incorporated into the MEaSUREs velocity data that are distributed through NSIDC. Annual velocity grids are available starting in 2005, five years after the beginning of our analysis. Our understanding of the controls on Thwaites Eastern Ice Shelf (TEIS) flow depend crucially on the time period between 2000 and 2005, when the influence of the Thwaites Western Ice Tongue (TWIT) evolved very rapidly. Without data from that time period, we would be missing significant evidence for our conclusions, and these data are provided primarily by MODIS. Furthermore, the annual grids that are available from MEaSUREs lack the spatial resolution and coverage provided by the MODIS data. Annual MEaSUREs grids are provided at 1-km resolution, while our analysis is at 500-m resolution, and several of the grids have significant missing data in the central TEIS. While these data could be processed at a higher resolution, the required work would be appropriate for a separate project, and reprocessing would regardless not solve the coverage issue in either space or time. Ultimately, the available InSAR data would not improve the data needed for our conclusions, and the inclusion of available products would decrease our spatial resolution, which is important for accurate Lagrangian analyses later in the paper.

The reviewer also suggested using Landsat-7 between 1999 and 2003 and ASTER. We have already included all available Landsat-7 data in our analysis, including from that time period, as it is included in the ITS_LIVE data cited in the text. We will make sure to clarify in the text that this is already included. We worked extensively with ASTER during our data preparation, and unfortunately found that it was not suitable for the analysis. There are relatively few images of this area available from ASTER, and many of the ones that are available suffer from cloud cover. During the 13-year period between 2001 and 2013, when Landsat-8 data are unavailable, three seasons lack any cloud-free imagery of the TEIS at all, and four more have a single day of data with incomplete coverage of the ice shelf, severely limiting the potential for successful velocity correlations. In addition, many mid-shelf correlations from ASTER imagery are unsuccessful. We will note this in the methods section of our manuscript.

Aside from the lack of availability from other datasets, we find that the MODIS data are sufficiently accurate for our analysis. The error figure that the reviewer cites of "several hundred meters per year" and "several tens of degrees" could be reasonable for a single correlation, but it overestimates the error for the averaged grids we have provided; as shown in figure 2, error bars are at maximum approximately +/-100 m/year, typically under 10% of the flow speed, or +/-10° (these error bars are considerably smaller later in the record, when Landsat-8 data are available). We will revise the text to make the true error ranges in our stacked velocity grids clearer. Furthermore, the velocity changes that we discuss in the conclusions that are important in understanding the overall ice-flow history on the TEIS are well outside the error bars, giving us confidence in the conclusions.

Overall, we believe that we have used all available velocity datasets that add value to this analysis, and that the data that are available are sufficient for the conclusions we have drawn.

Reviewer comment: *Concerning the Sentinel-1 processing, it seems that the tidal signal is not corrected while this signal strongly affects the range component of the Sentinel-1 at 6 and 12-day repeat cycles. This problem is probably mitigated by the fact that the data are averaged by quarters. Nevertheless, this may lead to additional errors that are currently not taken into account and therefore should be at least discussed to evaluate the impact it has on the data.*

Author response: Thank you for catching our omission - we have corrected for tides using CATS 2008 in the Sentinel-1 processing, we just forgot to note this correction in the text. We will update the text to reflect this.

Reviewer comment: *Elevation data and basal melt rate:*

Regarding the elevation data, the authors use the REMA as a reference to compute lagrangian elevation changes compared to IceSAT (2002-2009) and IceSAT-2 (2018-present). REMA is vertically referenced to CryoSAT-2 elevation. Why not use directly the CryoSAT-2 observations ? If this is due to possible error due to penetration in Ku-Band in firn and/or snow, then the same concern could be raised for the calibration of REMA.

Author response: Many other authors (e.g. Smith et al. 2020) have carried out Eulerian analyses of ice-thickness change and basal melt on Antarctic ice shelves, including the TEIS. These analyses are well-suited to large-area averages, as effects of ice-thickness advection largely cancel out. However, we wanted to examine spatial variability in ice-thickness change and basal melt at a higher resolution than has been necessary for many past analyses, which requires a Lagrangian approach. Because Lagrangian approaches require migration of measurements from the measurement epoch to a reference grid, we either need to use a full-coverage DEM or to interpolate between points from an altimeter. As the REMA mosaic shows, the topography of the TEIS is complex and varies on spatial scales smaller than can be captured by interpolating between CryoSat-2 point measurements. It is therefore better to use the altimetry data to reference a full-coverage DEM, as has been done with REMA.

Reviewer comment: *It is also unclear if the authors have used individual REMA strips from GeoEye and Worldview acquired between in 2013 and 2014 and then referenced them to CryoSAT-2 themself, or if they used an already mosaicked REMA product where* *they have no real control on the quality of the results. I imagine that it is the latter because otherwise there would have been the possibility to correct for the tides which apparently was not done. Here several other questions are raised: (1) why not use the complete REMA archive which provides data over a longer period (2012 to 2018) than 2013-2014? It would be possible to calculate the displacement directly on the REMA DEMs which would allow to obtain almost perfect co-registration for the Lagrangian calculation (much better than using flow velocities obtained by other sensors). Obviously the vertical errors would remain high (+/- 6m) but that does not seem to be too much of an issue here.*

Author response: While some REMA DEMs are available spanning the mentioned time period between 2012 and 2018, almost all the coverage in this area is available between 2014 and 2016. Even with the high rates of change on TEIS, the errors associated with differencing two REMA DEMs during this time period would be too high to obtain meaningful results. Two other barriers stand in the way of this sort of analysis: 1. Lagrangian analysis requires a reference grid with complete or near-complete coverage, so that there is data availability at any location a point migrates to. This would necessitate mosaicking available DEM strips, which is exactly what has been done with REMA; we have neither the computing power nor the expertise to do this mosaicking better than the original REMA authors, which is why we have used a single REMA mosaic tile. 2. We have shown that velocities on the shelf have changed significantly over time. With incomplete coverage from REMA strips, we would not be able to obtain annual velocity grids that capture these changes, instead having to rely on longer time-averages that would miss these changes and introduce larger errors into the Lagrangian analysis. Our annual velocity analysis is thus more suitable for Lagrangian calculations on the TEIS.

Reviewer comment: *(2) Why not use CryoSAT-2 directly, using these observations, there would also be the possibility to correct the tides which cannot be done in the REMA mosaic.*

Author response: As noted above, a Lagrangian analysis requires a gridded dataset, and gridding of available CryoSat-2 data does not have the necessary resolution to capture the high spatial variability in TEIS topography.

Reviewer comment: *(3) As a complement, there might have been the possibility to obtain high resolution elevation data from TanDEM-X that would have been the perfect complement for this study.*

Author response: As the reviewer notes, this is a great idea for a complementary study that could extend the work done here. However, it is well outside the scope of our methodology, and the conclusions we have drawn are well within the error bounds of our data, so this additional dataset is unnecessary in the current study.

Reviewer comment: *(4) Lidar data from Operation IceBridge probably exist during the studied period and would certainly provide constraints from REMA DEMs or add additional measurements to IceSAT. Why not include them ?*

Author response: As in the previous question, a complementary study could certainly decide to go in this direction, but it would add little to the analyses we have presented. IceBridge lidar data are sparse on the TEIS; the year with the most extensive data coverage is 2009, when 6 flight lines crossed the TEIS. All other years have even sparser coverage. Coverage that coincides with collection of REMA DEMs is far too sparse for effective vertical referencing. In addition, IceBridge data collection began after the ICESat era, which means that there is little to no separation in time between the available IceBridge transects and REMA DEMs. With less vertical change over a shorter period of time, trends would not fall outside the error range. We believe the IceBridge data are extremely valuable for analyses of specific areas, and our team has a separate study in review that utilizes these data, but they add little to the large-scale analyses that are the subject of this study.

Reviewer comment: *If the authors seem to have done a good job in correcting for tides, taking into account the firn to convert elevation to ice thickness and surface mass balance in the melt rate calculation, it is unfortunate that these corrections are not shown as supplemental material of the paper as maps. In the same way, error maps could be shown to evaluate spatially the robustness of the different observations. I am also unsure if the evolution of firn air content over time is taken into account when calculating thickness changes.*

The error calculation for the elevation changes and for the melt rate calculation remains also rather unclear. The errors for the firn and for the SMB are not provided. The error for elevation changes are estimated to be to the order of 1 m/yr, therefore the error on melt rate without the additional errors coming from firn, surface mass balance or flux divergence should be alone about 10 times larger (9.41 to be exact with the chosen density in seawater and ice) but surprisingly the authors found basal melt error lower than for the surface elevation changes. This needs to be clarified.

Author response: Tide corrections are derived from the freely available CATS2008 model (https://www.esr.org/research/polar-tide-models/list-of-polar-tide-models/cats2008/), and maps of tidal variation can be readily created using this model. As the data were collected at many different times, it would be impractical to show all of these maps, even in supplementary information. The model used for firn air content and surface mass balance are at a very coarse spatial resolution, so a single average value is available for the TEIS; our error is therefore an area-averaged estimate for the TEIS, and showing this as a map would be uninformative. We have adjusted our analysis to include the firn-air content (FAC) generated from BedMachine, which takes into account spatial variability across the TEIS, and use the SNOWPACK model to estimate a spatially averaged variability over the time period of our study. This variability in time is used to make a an error estimate of 1 m for FAC, which we use in our error analysis for basal melt rates

Thank you for the very detailed reading of our manuscript; we inadvertently used the error associated with surface height change (dh/dt) rather than the error for ice thickness change (dH/dt) in our basal melt error analysis. We are happy to provide the detailed error calculations as supplementary information, and have attached a document to this post that shows this error analysis alongside a revised figure as suggested in the next comment. The correct basal melt error calculations are: 11.5 m/yr for REMA to ICESat-2, and 7.2 m/yr for ICESat to REMA. Note that, despite the high values attributable primarily to the uncertainty in REMA, the areas of high basal melt that we have noted in the text as important (particularly in the shear zone upstream of the pinning point) have basal melt rates in the range of 10-20 m/yr, with the highest values more than 50 m/yr, which is well outside of this error range, and does not call any of our conclusions into question. We also note that the consistency between the ICESat/REMA and REMA/ICESat-2 epochs suggests that error over most of the shelf is considerably lower than this estimate, although the sparse data from ICESat prevents a more robust analysis of this similarity.

Reviewer comment: *Figure 7 is not very appealing. The use of point shapefile to show changes in surface elevation and basal melt makes the graph quite messy and complicated to read. It would have made much more sense to create an interpolated and filtered spatial map from this point cloud. An evaluation of the total melt and a comparison with existing results would have been welcome. Melt rates are evaluated for two periods 2003-2013 and 2013-2020 with IceSAT and IceSAT-2, respectively. However I could not find any analyses of potential changes in melt pattern or elevation changes. How much the basal melt has changed ? What are the implications of relative changes in thickness ?*

Author response: We agree with the reviewer that the point representation we have presented is not ideal. We had presented it in this way in order to have a consistent symbology between the ICESat and ICESat-2 data points. While we could create an interpolated and filtered spatial map of the ICESat-2 data with reasonable coverage of the ice shelf, a similar presentation of the ICESat data is not reasonable, as they are far too sparse for interpolation on an ice shelf with so much topographic variability. In our attached document, we have produced an alternative Figure 7 with the ICESat data appropriately left in a point representation and the ICESat-2 data gridded across the entire shelf.

Because the ICESat data are so sparse and variability in thinning and basal melt rates so high, in addition to relatively high error estimates, our opportunity for comparison is very limited and we can have very little confidence in generalized statements of regional patterns of change based on the available data. However, consistency between the datasets in a few key areas of high basal melt rates and thinning rates suggests persistent forcing on average over the last two decades. As explored in detail in our discussions, this has important implications for the weakening of already-weak areas of the ice shelf. A more detailed study focusing on melt rates and changes in melt rates would be valuable, but it is not the goal of the current study.

Reviewer comment: *A vertical cross-section along the flowlines would have proved useful to illustrate the melt rate and thickness changes along TEIS, especially close to the pinning point and the grounding line. Potentially this could have been compared with OIB radar flight lines directly measuring thickness at different dates. Overall, I think that the results and discussions about melt rate and thickness changes need to be more quantitative. Indeed, there is a crucial need to better model the interactions between the ocean and the glaciers in this region. By providing a more rigorous and quantitative analysis of melt patterns and evolution, the authors would provide an important input to a better understanding of the circulation of ice shelf cavities in the Amundsen Sea embayment.*

We have another paper in review (Wild et al., TCD) that uses OIB radar flight lines to look at changes near the grounding line and pinning point. While we agree that a more quantitative discussion of change would be useful to the community, we have done what is appropriate for the available data, and an analysis of melt patterns and pinning point evolution is not primary the goal of this paper.

Reviewer comment:

Other specific comments:

Figure 1 shows the grounding line evolution from 2004 to 2017. It is again rather unclear why the authors have not used published datasets (NSIDC) that provide grounding line position since 1992. It would have appeared that the delimitation of the grounding of 2004 is not correct. Already in 1996, the InSAR grounding line was several kilometers further back in many places.

Author response: The 2004 grounding line is the published grounding line as downloaded from NSIDC. The citation is provided in-text in the caption (Bindschadler et al. 2011) and as a full citation in the list of references. As our analysis begins in 2000, a 1996 grounding line would be less relevant to our paper.

Reviewer comment: *l597: The authors mentioned that Adrian Luckman analyzed "Sentinel-2", I believe that the authors meant Sentinel-1, as no mention of Sentinel-2 is done in the manuscript.*

Author response: Thank you; we will correct that typo.

Reviewer comment: *The authors provided datasets used in the study at the following link : https://doi.org/10.15784/601433. This is a very good initiative and I hope that if the manuscript is accepted the link will work successfully as it is not currently the case.*

Author response: We echo the reviewer's emphasis on the importance of sharing data. The link works just fine for us; we hope the reviewer will contact the USAP-DAC (https://www.usap-dc.org/contact) to address any technical problems they are facing.

Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2021-76/tc-2021-76-AC2-supplement.pdf