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Comment on tc-2021-76

Anonymous Referee #2

Referee 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-RC2, 2021

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.

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.

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.

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.

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. (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. (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. (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 ?

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.

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 ?

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.

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.

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.

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.