***Change in Antarctic Ice Shelf Area from 2009 to 2019* by Andreasen et al.**

Reviewed by Chad A. Greene (NASA/JPL).

Initial Reactions

In this paper, Andreasen et al. describe a new, independently derived, annual, pan-Antarctic mapping of ice shelf calving fronts. The paper is very well written, the methods are sound and clearly described, and as the authors point out, the work of mapping and understanding calving is an important and timely endeavor. I am happy to see this effort, and I hope that with a few adjustments it will be published in *The Cryosphere*.

Previous Work

My main concern is that this paper is similar to a number of studies that are already in the literature, and although some of the previous work is acknowledged in the present manuscript, it's unclear how the new findings build on previous efforts. If the present study is not intended to explore new ground, that may in fact be okay, as there is real value in independent analysis that replicates established findings. But if the purpose of this study is only to replicate previous studies, then I'd like to see more clarity about which previous results are reaffirmed here, and who might've gotten it wrong in previous studies. If the present work finds any notable disagreement with previous studies, then I'd like to see that clearly stated and I'd like to see some discussion about why different groups might be coming up with different numbers, and what the differences might mean in a broader context.

A couple of Celia Baumhoer's papers are cited in this manuscript, but I'm afraid the most relevant one to the present study has been overlooked. In her 2021 paper, terminus positions were mapped for 1997, 2009, and 2018, and the paper investigated the environmental factors that led to terminus position changes during each epoch. The present manuscript presents effectively the second half of the time series from Baumhoer et al., 2021, but without looking into potential causes of terminus position change.

Some other work worth mentioning in the manuscript includes a pan-Antarctic survey of calving fronts by Miles et al., 2016, a recent regional study of calving fronts by Christie et al., 2022, and a 15-year annual pan-Antarctic calving dataset by Qi et al., 2021. Also, I'm not sure if it's citeable yet, but the authors may want to be aware of the high-resolution IceLines coastline dataset: https://download.geoservice.dlr.de/icelines/files/

Main Findings

As far as I can tell, the analysis is sound, the main findings are accurate, and everything generally agrees with the results of previous studies. It's somewhat tricky, however, to frame the results in a way that won't be easily misunderstood, particularly in this case, where changes over 10 years are dominated by just a few ice shelves whose calving cycles repeat every few decades. I am slightly concerned that a cursory glance at the abstract and conclusions might give the impression that Antarctica is in an overall phase of growth, when the present analysis

has only captured a small portion of the multi-decade calving cycles of the big ice shelves that dominate the continent-wide totals. My coauthors and I ran into this problem when we tried to describe a 24 year calving time series in a recent paper, and I'm not sure if we got the wording exactly right, but we did our best to put the results of our short time series into the context of the longer-term calving cycles of the big ice shelves. I'd like to see some more direct language or clauses in the abstract to make it clear that the authors are not implying that Antarctica is somehow already on track to recovery from climate change.

Less Results, More Discussion

The Results section is lengthy and presents a long list of numbers, most of which are already presented in Table, 1, and at times it's unclear why certain numbers are worth mentioning or how they change our understanding of ice shelf calving. An attempt has been made to provide context in the Results section, for example by mentioning the sea level potential of the Aurora Subglacial Basin in the same paragraph as the calving-front position change estimates for Totten, but no conceptual bridge is provided to link calving processes to the doomsday value of sea level potential. As a consequence, the Results section feels somewhat incohesive at times, and it's unclear how all the facts and figures are related to each other or which findings might be most significant.

I recommend significantly abbreviating the Results section, to put the main findings in clear focus. For anyone who wishes to know the exact amount of area change of a specific ice shelf between two arbitrary dates, I recommend sharing the data, so they can explore it as they see fit. Separately, the inclusion of a Discussion section may provide a better place to tell the "story" of a few key locations that may be of interest. Sticking with Totten as an example (but this is by no means a prod to focus on Totten in the revision), Sue Cook did some modeling work to understand the glaciological factors that can prime Totten for calving (Cook et al., 2018), Bertie Miles looked at environmental forcing and ice-front change there (Miles et al., 2016), and I've got a paper on Totten's dynamic sensitivity to calving (Greene et al., 2018). By following the thread of what causes calving to how calving impacts glacier dynamics, we gain a better understanding of how the present results are related to that 3.5 m sea level potential of the Aurora Subglacial Basin. Readers will appreciate this sort of "tying things together", as it will help us understand the importance of your results.

Data Availability

The real value of this paper is that it describes an independently derived calving-front dataset. The trouble is, the dataset apparently hasn't been placed in any public repository, it's not included as a supplement to the manuscript, and it's unclear if or how anyone will ever be able to access it, use it, build on this work, or directly evaluate the data. I do see a statement that the data will be made available upon request, but I think the field is trying to move beyond the old culture of sharing data via private handshake deals. (Sharing data "upon request" often fails when authors leave academia, and the social dynamic of needing to beg strangers for data tends to favor the well-connected and contribute to the Matthew Effect.) So that the data can be evaluated and we can feel confident that it will be made available to all, I'd like to see the data placed in a long-term data repository or uploaded as a supplement to this manuscript.

Minor comments

Throughout: Area change estimates are presented to 0.1 km^2 precision. That's probably a tad too precise, particularly given that uncertainty is stated as being 1 km^2.

L7: "50-years" hyphen is unnecessary.

L15,16, and a few other places: Only the word "Antarctic" needs to be capitalized in the phrase "Antarctic ice shelf" or "Antarctic ice shelves". I think we only capitalize "Ice Shelf" when it's part of the official name of a specific ice shelf.

L51: "there are only five examples of regional assessments that have been updated since 2011" The wording here might make some folks feel left out. I'm thinking of *Antarctic ice-shelf advance driven by anomalous atmospheric and sea-ice circulation* by Christie et al., 2022, *Environmental drivers of circum-Antarctic glacier and ice shelf front retreat over the last two decades* by Baumhoer et al., 2021, *Pan–ice-sheet glacier terminus change in East Antarctica reveals sensitivity of Wilkes Land to sea-ice changes* by Miles et al., 2016, and a handful of other studies that have looked at the histories of single ice shelves or neighboring ice shelves. Consider rewording the sentence to focus on the positive—Talk about the work that *has* been done, rather than the focusing on what *hasn't* been done.

L53: "In this study we address this gap…" It's not entirely clear what gap is being addressed. Consider wording more along the lines of, "In this study, we build on previous work to answer such-and-such remaining question" or "We build on previous work to gain a better understanding of such-and-such." (The "yes, and" rule of improv is often a good starting point for motivating scientific studies, and it always feels better than "yes, but".)

L62: Hyphenate "cloud-free".

L83: The method of quantifying uncertainty in terminus pick position sounds sensible to me. How does picking uncertainty propagate into uncertainty in final estimates of area and mass change?

L97 and elsewhere: "We computed the mean annual rate of calving by dividing the total area change by the number of years observed…" The pedant in me is reacting to this framing. Ice shelves may grow at a linear rate, and they may retreat at an ~linear rate when successive small calving events occur over many years, but in the case of a single calving event over the course of the observation period, it feels somewhat inappropriate to describe this as a rate of change. It's more appropriate, in my opinion, to talk about the cumulative change over the observation period, without dividing by time.

L102: How are uncertainties in ice thickness handled when estimating ice mass changes? Keeping in mind that Bedmap2 ice shelf thickness is estimated by subtracting modeled firn air content (order of 20 or 30 m) from surface elevation measurements and applying hydrostatic

inversion (multiply by 9.3), the firn correction alone can influence ice thickness by hundreds of meters, and firn is rather poorly constrained in Antarctica. I realize there's no good way to validate ice shelf thickness where it has not been directly measured (and even radar has its uncertainties), but it would be good to have some approximate bounds on the mass change estimates that are presented in this study. I recommend making some reasonable guess at thickness uncertainty, and propagate it into the mass change estimates.

L106: I'm not entirely sure I follow the logic of the ice shelf area uncertainty estimates. Above, the uncertainty in picking position is estimated at 254 m, and that sounds very reasonable to me. I interpret line 106 to mean that the 254 m value is not considered in the area uncertainty. Line 106 says accuracy is rounded to 1 km^2, but it's unclear whether the 1 km^2 uncertainty applies to each ice shelf separately, or Antarctica as a whole. My intuition says 1 km^2 may be a reasonable estimate of area uncertainty for a small ice shelf, but those 254 m position errors are likely highly correlated along the edge of the bigger ice shelves like the Ronne. The Ronne front is some ~2000 MODIS pixels wide, so a fully correlated 250 m picking error should result in something like 125 km^2 uncertainty for the Ronne, if I've done the math correctly. Perhaps errors are not fully correlated along the entire Ronne ice front, but I suspect the measurements are not accurate to 1 km^2 for the big ice shelves.

L219: I think "tsunami" can be uncapitalized.

L291: Units appear on this line as m/a, whereas in the rest of the manuscript it's m/yr. According to the style guide (https://www.the-cryosphere.net/submission.html) they should all be written exponentially (m yr^{-1}).

L306: The heading "Rapid Area Growth" strikes me as a little funny, given that it's occurring at a glacial pace. Perhaps "Steady Area Growth" would be a better descriptor? Feel free to disagree.

L357: Liu et al., 2015 is incorrectly cited as an example of a study that estimates steady-state calving flux. Similar to Qi et al., 2021, they actually just counted the icebergs that were bigger than 1 km^2 (and the uncounted icebergs smaller than that might be why their calving estimates are so much lower than Rignot's). If you'd like to cite another highly relevant paper that used steady-state analysis, check out Depoorter et al., 2013.

Results: Cook Ice Shelf drains a major marine-based subglacial basin, and the ice flow has been shown to be sensitive to changes in the terminus position (Jordan et al., 2022). Is there a reason Cook was excluded from this study?

References

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