Dear Reviewer,

After carefully reading both reviewers' comments, we conclude that they did not find any major weaknesses or flaws with our manuscript. Our analysis of the comments can be summarized as:

(i) adding four more years of data to the time series (R1) – this is the most significant change suggested, and was only a suggestion for us to consider. We respond to this below.

(ii) restructuring the m/s and further clarify some aspects of the methods (R2) – this is straightforward for us to do, and we are happy to do this.

In response to (i) from R1. We agree that a longer, more complete, record is always desired. However, we explained in our response why it is not a simple matter, as it involves many steps and careful integration of models. We feel that the request is reasonable but should not be considered as a condition for publication. We feel it is appropriate to point out that this paper has now been through a few rounds of revision (with other journals before this), every one of them being fairly slow. We believe that this is why the data now seem dated. Regardless, we believe the dataset in its present form is of high value to the community and will be well received upon publication.

We also must provide you with some context to explain the conditions under which we are working. The lead author Fernando Paolo left academic research in 2020 and does not have the time nor the funding to do additional data analysis. We note that our dataset represents a significant improvement over the only other time-dependent dataset of ice shelf basal melt rates (spanning a similar time interval) currently available to the community (Adusumilli et al. 2020), and that the dataset presented here is already being used in large modeling initiatives (e.g. Estimating the Circulation and Climate of the Ocean – ECCO, see below) and published research (Nakayama et al. 2001, GRL; Greene et al. 2022, Nature).

The melt rates presented here have been used in producing ECCO's latest estimate, Version 4 Release 5, available at https://ecco.jpl.nasa.gov/drive/files/Version4/Release5

REVIEWER #2 COMMENTS

SUMMARY

This research by Paolo and colleagues analyzes 26 years of data from satellite observations of Antarctic ice shelf thickness based on a novel data fusion approach, advanced satellite-derived velocities, and a new surface mass balance model (GEMB). The changes in ice shelf thickness are subsequently related to (changes in) flow and basal melt rates. The study found a pattern of overall thinning around Antarctica, with a slowdown in thinning starting around 2008 (similar to earlier research of for example Adusumilli et al., 2020). The researchers attribute this slowdown to changes in external ocean forcing and potential negative feedback effects of i) accelerated grounded ice flow on ice shelf thinning rates and ii) thinning and melt rates.

We want to preface our response with a big thank you. We know that you are likely very busy and that taking the time to provide a thoughtful, in-depth, review is not easy. We are grateful for your time and input. Your constructive criticism is highly valued. We can see that a restructuring of parts of the manuscript would be very valuable to add clarity, so thank you for bringing that to our attention. We do disagree with other comments but we hope that we have provided sufficient context for you to understand our point of view. We see several of the comments as good suggestions that should not be considered a condition for publication.

MAJOR COMMENTS

This research touches upon an important topic, is original with several novelties and provides new understanding of the melting of ice shelves. Therefore, I think the manuscript ultimately warrants publication in the Cryosphere if it manages to tackle some of the major and specific comments listed below:

-[1] Restructuring: although the paper is overall well written, I think the structure often is complicated to follow with data and methods that are often intermingled and without a clear distinction of results and discussion.

We understand that the current structure of the manuscript might not be optimal. There are mainly two reasons for this. First, our manuscript indeed presents a combination of new data, novel methods, and new findings. It is challenging to maintain a balance throughout the manuscript as some topics are more technical in nature and, therefore, require more extensive descriptions. Second, we tried to follow the somewhat rigid format of the TCD regarding the different sections.

○[2] I think the paper would benefit from a more extensive Data section, where next to the Radar data, also the velocity data, GEMB model data and ECCO model data are described. These data sets now are gradually introduced throughout the manuscript and it is therefore sometimes difficult to keep the overview.

We agree with the reviewer that the manuscript would benefit from a more traditional manuscript layout. As the main issue seems to be an intermingled data/methods section, for the revised manuscript we will recognize the material into the IMRAD format of:

a) Data
b) Methods
c) Results
d) Discussion

○ [3] I think the paper would benefit from a more extensive discussion that actually zooms out, puts the results in a wider context (e.g. relative to the state-of-art) and reflects on the implications. Currently this reflection is very limited

It may be a bit surprising but there is actually only one other high-resolution published map of basal melt rates (Adusumilli et al. 2020). That dataset represents the state-of-the-art and we compare our work extensively to that. We interpreted the findings and discussed the wider implications the best we could within the scope of the manuscript, which is mainly to present a new dataset of ice shelf thickness change and basel melt rates. Accordingly, we must describe in detail the methodology to obtain such estimates. And because the methods and auxiliary datasets can be quite complex, there is a justifiable unbalance in the amount of text dedicated to “Data and Methods” versus “Results and Discussion”.

- [4] Balance of detail: as a reader (and not a core altimetry expert) I was often missing the necessary details to understand the steps in the data processing and reproduce them. Therefore, I think it is key to provide much more detail on the processing methodology at several locations (especially for the steps that are at the core of the altimetry processing). At the same time, I had the impression that other parts (especially the GEMB model section) was very extensive and contained details that are less relevant for this paper (and can be looked up in the GEMB paper). I would therefore limit the GEM section to the core explanation and limit the description here to the steps that are specific to the paper (i.e. calibration of the densification parameters)

This specific comment is in contradiction with the other Reviewer that stated that the: “The methodological improvements implemented in this study are clearly explained in the manuscript”. This is not surprising as readers/reviewers come from a wide range of backgrounds and will not always have the same perception of the paper.

That said, there are many standard procedures that one must follow in using altimetry data for ice-sheet/ice-shelf studies. There is a wealth of literature specific to the use of radar and laser altimeters over Antarctica, Greenland, and mountain glaciers, with detailed descriptions of these measurements and common processing approaches (e.g. Arthern et al., 2001; Bamber 1994; Borsa et al, 2014; Brenner et al, 1983; Brenner et al., 2007; Brockley et al., 2017; Davis 1993; Davis & Ferguson, 2004; Flament & Rémy, 2012; Helm et al., 2014; Hurkmans et al., 2012; Khvorostovsky 2012; Lacroix et al. 2009; Legresy and Remy, 1997; McMillan et al., 2014; Nilsson et al., 2016; Paolo, et al., 2016; Roemer et al, 2007; Schröder et al., 2017; Schröder et al., 2019; Simonsen et al.,2017; Wingham er al. 1986, 1998, 2006, 2009; Zwally et al., 2005, 2012, 2015, 2021). We cite the relevant literature throughout the manuscript. We follow the standard practice of describing in more detail only the novel aspects of the altimetry processing that we introduce and cite literature to cover those methods that have been previously described.

Regarding the GEMB section. At the time of submission, GEMB did not have a stand-alone publication which is why we provided such an extensive description of the model. Modeling of SMB and firn air content is of first order importance for inverting basal melt rates from satellite altimetry. That said there is now a GEMB paper that is near acceptance in Geophysical Model Developments that we can now cite (https://egusphere.copernicus.org/preprints/2022/egusphere-2022-674/). We will modify this section in the revised manuscript and cite the new paper appropriately (also see response to other reviewer)

We also want to point out that the analysis and description of the altimetry data and firn modeling matches or exceeds that of previous ice shelf studies (e.g. Paolo et al. 2015; Paolo et al. 2018; Adusumilli et al. 2021; Smith et al. 2020; Shepherd et al. 2018, and many others).

- [5] Reproducibility: due to the lack of detail on several processing steps (or an condensed explanation for their motivation) I think it is basically impossible to reproduce the method. I applaud the effort of the authors to make code available, but with this type of code a user cannot reproduce the paper. It is a collection of methods and example notebooks that are not related to the paper. I think it is key that the code is provided as such that the reader can at least see which functions are used for which steps etc. Currently this code does not help reproducibility. I would therefore advice to either provide a notebook that runs through a complete workflow (preferential) and/or clearly indicate how the different blocks of code were used in which steps.

There might be a confusion regarding reproducibility of our work. Our data processing includes several essential steps, some of which are standard practice in using altimetry data for glaciological studies and, therefore, are exhaustively described in the dedicated literature (see, for example, Nilsson et al. 2022 and 2016); and some that were developed specifically for this work that we describe in detail (as acknowledged by the other Reviewer). We also point out that some introduced techniques, such as the optimal interpolation accounting for correlated errors, while novel in this field, these methods are widely used in other areas. So we provide the original references instead of reproducing an unnecessary lengthy description. Much of our analysis builds heavily on previous work.

The many processing steps and large-scale analysis with 26 years of satellite altimetry data, satellite velocities, surface mass balance modeling, and auxiliary geophysical fields, were performed with the aid of a supercomputer (HALO at JPL). It would be virtually impossible to provide code that would run on a personal computer. Nonetheless, all of our data and code have been made publicly available, increasing transparency and reducing barriers to future efforts. To our knowledge, our work is the first large-scale altimetry effort looking at ice shelves (or ice sheets for that matter) that has released all of their code and made all of the data publicly available. We point to another analysis of ice shelf melt rates recently published in Nature (Adusumilli et al. 2021) for which they satisfied the reproducibility by simply providing the statement that “The Matlab, Python and shell scripts used for the analyses described in this study can be obtained from the corresponding author upon reasonable request.”. We feel we have gone far beyond the minimum requirement for reproducibility and have provided the glaciological community with a well documented Python library that is able to reproduce the entirety of the analysis presented in this manuscript (https://github.com/nasa-jpl/captoolkit) and has already been used in other published works, despite being developed for the study presented here (e.g. Smith et al., 2019; Alley et al. 2021; Nilsson et al., 2022).

We applaud the reviewer's concern and attention to reproducibility (something that we are also passionate about) but we feel that we have gone far beyond the minimum acceptable requirement and that requesting notebook style workflow that can run on other systems is a nice to have but should not negatively impact the evaluation of this work.

- [6] Dynamic vs melting processes: the authors show two processes that could explain the slowdown (i.e. increased ice flow and thinning into cooler water), but fail to provide a consistent overview of the relative role of both (although they have the data to do so). Currently, they show individual examples for 4 ice shelves (Table 2) where they quantify the role of dynamic thinning vs. melting,. I suggest that they do this quantification for all ice shelves (in Table 3) and also add the role of dynamic thinning in all plots of time series to allow the reader to better understand the importance of both processes.

There are two important things to keep in mind. The main objective of this manuscript is to present a dataset of ice shelf thickness and basal melt rates. This is, however, accompanied by an extensive analysis that highlights some novel findings (perhaps we could adjust the title to better reflect this). The dynamic component (i.e. ice velocity) is needed in order to derive the basal melting from the total thickness change. While we make an effort to obtain the best available ice velocity estimates for the ice shelves in question, further analysis into the ice shelf dynamics is beyond the scope of this manuscript (which is already quite extensive).

Another key point, as explicit in the title, is the focus on "West Antarctic Ice Shelves". There are two reasons for this. First, this is the region where the thickness change signal is highest and, second, this is where we have the most complete time-dependent velocity data. So we could not make a similar temporal analysis for the ice shelf dynamics (and consequently basal melt rates) for all ice shelves. As stated in the manuscript, we use a mean velocity field for all ice shelves other than Pine Island, Thwaites, Crosson and Dotson (the focus of our paper).

SPECIFIC COMMENTS

L50: I think it would be good to have a separate data section where next to the Radar data, also the velocity data, GEMB model data and ECCO model data are described

This is a good suggestion and it would certainly improve the readability of the paper. Please see response to the second major comment.

L80-85: this methodological step might need a bit more extensive explanation as it is currently very condensed and therefore not easy to interpret unambiguously. It would also be good practice to clearly show each of these steps in the available code (reproducibility). Now the reader has to guess what happens and also does not have the code to better understand what happens.

The code and its documentation is available here (https://github.com/nasa-jpl/captoolkit) and is already being used by others to process satellite altimetry data (e.g. Smith et al., 2019; Alley et al. 2021; Nilsson et al., 2022).I think what the reviewer is asking for is a notebook that could be run by the user to execute each step of the processing. As mentioned in the response to the 5th major point, this is not easily doable as the code was designed to run on large datasets with the aid of supercomputers. Refactoring the code to provide localized examples that can be run on personal computers, though valuable as an explanatory tool, is outside of the scope of this project.

L92-96: again this methodological step might need a bit more extensive explanation as it is currently very condensed and therefore not easy to interpret unambiguously and/or to understand the motivation. It would also be good practice to clearly show each of these steps in the available code (reproducibility). Now the reader has to guess what happens and also does not have the code to better understand what happens.

Please see responses to major comment 4.

L112 (but also later throughout the paper): when using text citations the brackets should be placed differently

Thank you for catching this. We will fix this in the revised manuscript

L128-130 "Global MDT … ice shelves": not clear if this is in general or in this study?

This is a general problem with Global MDT. We will clarify this in the revision.

L131-136: again this methodological step might need a bit more extensive explanation as it is currently very condensed and therefore not easy to interpret unambiguously and/or to understand the motivation. It would also be good practice to clearly show each of these steps in the available code (reproducibility). Now the reader has to guess what happens and also does not have the code to better understand what happens.

Again, we want to emphasize that the code is available but likely not as user friendly as the reviewer is hoping for. Please see responses to major comment 4.

L144-147: again this methodological step might need a bit more extensive explanation as it is currently very condensed and therefore not easy to interpret unambiguously and/or to understand the motivation. For example it is not clear what robust multi-variate regression is (ref?), if it is done for every pixel separately?

We will clarify, and point to a technical reference (Holland and Welsch, 1977).

L149-217: I think this section can be strongly reduced to keep only a condensed overview of GEMB (while refering to the GEMB main paper). Many of these methodological details are not relevant for this study (in contrast to other locations where the text is extremely condense on things that are important)

Please see response to major comment 4.

L223: what is the typical depth of 550 kg/m3? If it is not extremely deep, why would you need a spinup of

The 550 km/m3 depth varies from 5 to 50 meters in depth, depending on its location (for more please see the GEMB manuscript Gardner et al., 2023 Figure 4). Spinup to a historical climate state is necessary in order to remove spurious trends in densification at both the 550 and 830 density horizon. In this way, modeled forward densification response is ensured to be resulting from the climatological forcing of the simulation. This model spinup procedure is the standard in the literature (i.e. to produce other state of the art firn products like RACMO-FDM and GSFC-FDM).

7750 years as the firn will be mostly dependent what happens in the last years of the spinup period.

Trends in firn air content (FAC) are highly sensitive to the spin up period. The model must be run to equilibrium otherwise there will be residual/erroneous trends in FAC that will adversely impact estimates of glacier mass change. Through our analysis we determined that a spinup < 7750 years will produce model results with erroneous trends in FAC.

L257: 28 seems reference is in wrong referencing system

Good catch! We will fix it. Thanks

L260: what are 5 month bins for every 3 months?

This is a sliding window. At every 3 months time steps we aggregate (bin) 5 months worth of data, so each time window overlaps by one month on both ends.

L259- 269: again these methodological steps might need a bit more extensive explanation as it is currently very condensed and therefore not easy to interpret unambiguously and/or to understand the motivation. It would also be good practice to clearly show each of these steps in the available code (reproducibility). Now the reader has to guess what happens and also does not have the code to better understand what happens.

We can add further clarification to some of these concepts.

L276: for readability it would help if the actual terms in the equation are repeated

We will replace the acronyms with the respective equation terms.

L292-301: I do not understand the processing of the velocity data. Again these methodological steps might need a bit more extensive explanation as it is currently very condensed and therefore not easy to interpret unambiguously and/or to understand the motivation. It would also be good practice to clearly show each of these steps in the available code (reproducibility). Now the reader has to guess what happens and also does not have the code to better understand what happens.

We agree that the description of how the velocity data is merged is quite dense but the description is explicit and complete. Both datasets are freely available and all processing steps are explicitly defined, and therefore fully reproducible. A notebook would be a great addition to reduce barriers for others to replicate the work presented but, again, we do not think that it should be a necessary condition for publication. We will revisit this paragraph during revision to see if you can improve the overall readability.

L305: what are insignificant changes? How insignificant are they?

Indistinguishable from noise given the magnitude of the uncertainties in the data.

L312 "mean rate of thickness change from both ends of the trend fit" Is it just first vs last value or are annual mean or so used?

Correct. First vs last value (which is a conservative estimation in the presence of nonlinear trends).

L317: what is k? I guess the iterator over n?

Correct. We follow the convention of using *i,j* for spatial and *k* for temporal iterators.

Section 2.8: I think the uncertainty quantification needs much better explanation. Currently many of the uncertainty terms fall out of the sky and their motivation and derivation is unclear. As such it is difficult for the reader to assess what these uncertainty terms are and/or what they mean.

We present an extensive description of the uncertainties compared to any previous ice shelf melt work. The estimation of our uncertainties is in fact quite standard. It is mostly based on quadratic propagation of the reported errors (e.g. from model outputs and velocities) and calculated standard deviations, adjusted for the degrees of freedom in question.

Section 2.9 seems a section that is not properly placed. It is self-contained and contains results before even the main results are shown. I propose to move this section to supplementary material or appendix to allow the paper to focus on the main story

We can rearrange the structure of the manuscript as specified above.

L438: I think it is good practice to separate results and discussion so the discussion can really reflect on the outcome of the results

Agreed. We were following the TCD suggested layout. We can rearrange the structure.

L453-456: this is a methodological description and should not be part of the results

We can try moving this paragraph when rearranging the structure.

L484-486: this is a methodological description and should not be part of the results

This information is providing context for the “results and discussion”. With the new structure (see comment above) all this will be better separated.

L490-492: this is interesting and is probably the key of the paper. I therefore suggest that the separation of thinning factors is quantified for all ice shelves and not only done in an anecdotal way for only 4 ice shelves.

We addressed this in detail above. These ice shelves are the focus of the analysis.

L496-497: I think this probably on overstatement as it might be read that it counts for all ice shelves. This might be true for Dotson, but is definitely not true for all ice shelves.

Again, every statement is referring to the ices shelves highlighted in this study.

L498-502: I think the context and perspective is important here. By just showing the changes in time-variable melt, the reader only sees one part of the puzzle. Therefore, it is important to simultaneously show the (time series of) dynamic contribution so the reader can see both terms in perspective.

The main objective of this manuscript is to introduce a dataset of ice shelf thickness and basal melt rates (perhaps we should modify the title to better reflect this). We however went a step further and also presented novel results. A more in depth analysis of the ice shelf dynamics is beyond the scope of the manuscript (we do quantify and state the dynamic contributions for the ice shelves in question). Moreover, to oceanographers and modelers using our melt rate product what matters is meltwater production.

L517-520: this is a methodological description and should not be part of the results

This will hopefully be addressed with the restructuring.

L526-528: please provide references for these notions

We will add the references.

L533-534: I think it is key for this paper that the relative contribution of this tandem should be quantified in order to understand the relative contribution of both processes

This is an attempt to offer a plausible explanation for what the data show. This paper is not about an in-depth study on ice shelf dynamics. (and it's already fairly extensive)

L539-542: again I think it is not good enough to hypothesize and hand-waive at this feedback mechanism. I think the authors have the data to quantify the relative contribution of both processes to asses their relative contribution

Again, this paper is not an in-depth investigation on ice shelf dynamics. It is a derivation of a dataset of ice shelf thickness and basal melt rates. We think it is perfectly acceptable to offer a plausible explanation consistent with our current knowledge (and the physics) of the ice shelf system.

Conclusion: this section is written in a complete different style (all we sentences). It would benefit from a more general conclusion style

We are not sure what "a more general conclusion style" means.

L556-561: I do not agree that these feedback mechanisms are not included. An ice sheet model advects ice (and hence thickness) and ice shelf draft plays a role in the melt parametrizations. Therefore, I do not see why the models should not account for these feedback processes.

Point taken. We will revise this statement. Also, if the physics of these feedback mechanisms is well understood, our explanation above is well-founded.

FIGURES

Figure 1: I don't think this figure add a lot to the understanding of the methodology (especially not a)) and provides an unbalanced view of the methodology (e.g. why only a figure of this step) as it is very anecdotal. I think it would be much more insightful to have a flowchart (or equivalent) of the complete processing steps instead of an anecdotal figure.

Lake Vostok is a reference site for testing altimetry measurements over ice surfaces. Previous studies have used this same location to compute records of height change. This step (the backscattering correction) is arguably the single most important correction applied to radar altimetry measurements over icy surfaces. The end product (the time series of height change) over Lake Vostok must show almost no long-term change in height. This is what we are showing.

Figure 3+4: I would also opt to remove this figure. They show GEMB results but are not necessary relevant for the key message of this paper.

We can remove these figures as we are shortening the GEMB section (see above comment), and delegating most of the technical description of the GEMB model to the respective (about to be published) paper.

Figure 5: it would be insightfull to also plot the data based on which these curves are estimated to give the reader an impression on how representative these estimates of decorrelation are

This work is based on satellite altimetry data, which means data coverage is extensive and dense, with data density increasing with latitude, characteristic of all satellite data.

Figure 6: Scalebar? Location?

This is Ross Ice Shelf. We will add that to the caption.

Figure 7: why is only half of Antarctica shown?

Because, as the title of the manuscript says, our focus is “West Antarctic Ice Shelves”. Also, just a matter of practicality. Having the full continent would not facilitate visualization as the overall figure will be reduced and the ice shelves will become too small to see.

Figure 8: I would also opt to remove this figure. It adds very little to the understanding of the paper and it would be more insightfull to show figures that actually show the processing steps

This figure is key to show the domain of the modeling exercise, the four ice shelves we focus on.

Figure 10: what explains the enormous changes for Nivl and Lazarev between this study and Adusumulli?

We haven’t looked into individual ice shelves outside the ones we focus on in this study.

Figure 11: nice figure!

Thanks! These figures take a lot of work.

Figure 14: It would be good to indicate in these figures how much can be explained by dynamic processes and how much by basal melt changes

Figure 16: for perspective it would be good to also plot the time series of the dynamic component to allow to put both processes into context

The main point of these figures is to show precisely the total thickness/meltwater signal. We quantify the dynamics in order to derive the basal melting. As we explained above, we only have time-variable velocities (i.e. dynamics) for Pine Island, Thwaites, Crosson and Dotson. All other ice shelves use a time-average velocity field (as done in previous work).

Additional References:

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