Dear Editor, dear reviewers,

We thank warmly the reviewers for the careful reviews and for their comments. We propose a new version of the article taking into account the remarks of the reviewers. We explain in details the reason of our choices.

Sincerely,

Floriane Provost, on behalf of all co-authors,

NOTE: In the following document, the referee comments are in normal fonts and the answers are in blue font.

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Reviewer 2 comments:

This manuscript adds to the literature on an important topic in glaciology: controls on glacier calving. Specifically, the authors construct a time-series of ice-front change and related variables (ice-flow velocity, strain rates, and sea-ice conditions) for Astrolabe Glacier in East Antarctica to better understand the causes of several calving events during the record. The general approach is useful, and the questions addressed are interesting. I think there is a fundamental issue with the analysis related to the treatment of sea ice that is important to address, and the paper could use some editing and polishing.

Major comment: Treatment of sea ice

The paper links the calving behaviour of Astrolabe Glacier to sea-ice forcing, which is presented fairly generally in the abstract. However, most of the rest of the paper refers to this analysis as addressing "landfast sea-ice forcing." Landfast sea ice is a specific sea-ice configuration that is attached to land, which may mean that it provides more buttressing potential than freely floating sea ice. There are indeed several papers, cited in this manuscript for comparison, that attempt to address the role of land-fast sea ice in calving and glacier behaviour. However, this study only quantifies sea ice, not land-fast sea ice. Looking at sea-ice extent and concentration is not equivalent to assessing the presence of land-fast sea ice, and it means that the analysis cannot be as directly compared to studies that assess land-fast sea ice. Instead, the differences between the studies and the implications for differing mechanisms should be explored in more detail.

We agree that landfast sea-ice is a specific condition of sea-ice. There is no landfast sea-ice dataset that covers the period until 2021, and we hence used the [Fetterer and Windnagel, 2017] dataset of sea ice extent and concentration.

As shown now on Figure 1 (below), sea ice extent is extracted along the coast where sea ice is likely to be attached to the coast. Likewise, for sea ice concentration, the pixel is centered on the Astrolabe ice tongue and is even smaller with a size of 25 km x 25 km (Figure 1, revised), hence we do think it is fair to assume that when sea ice is present, it is connected to the land in these locations.

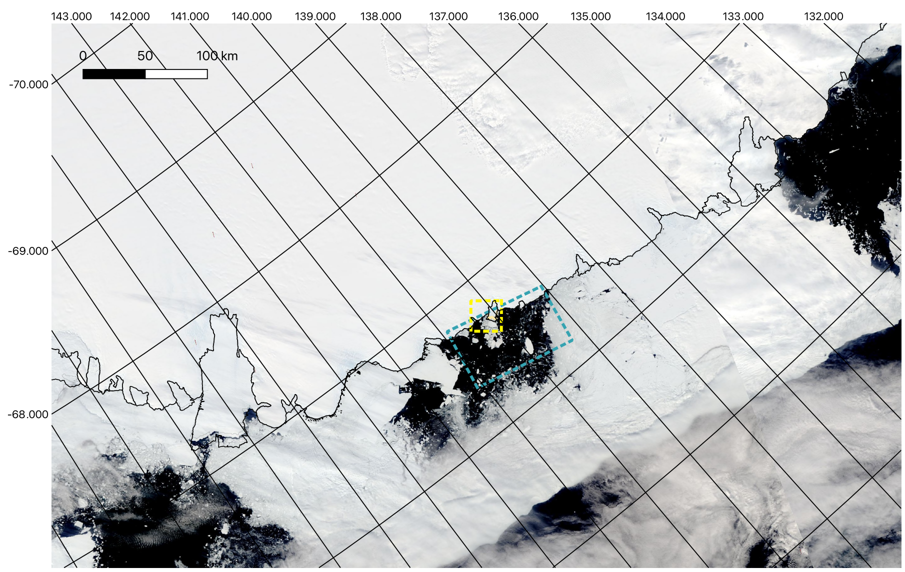


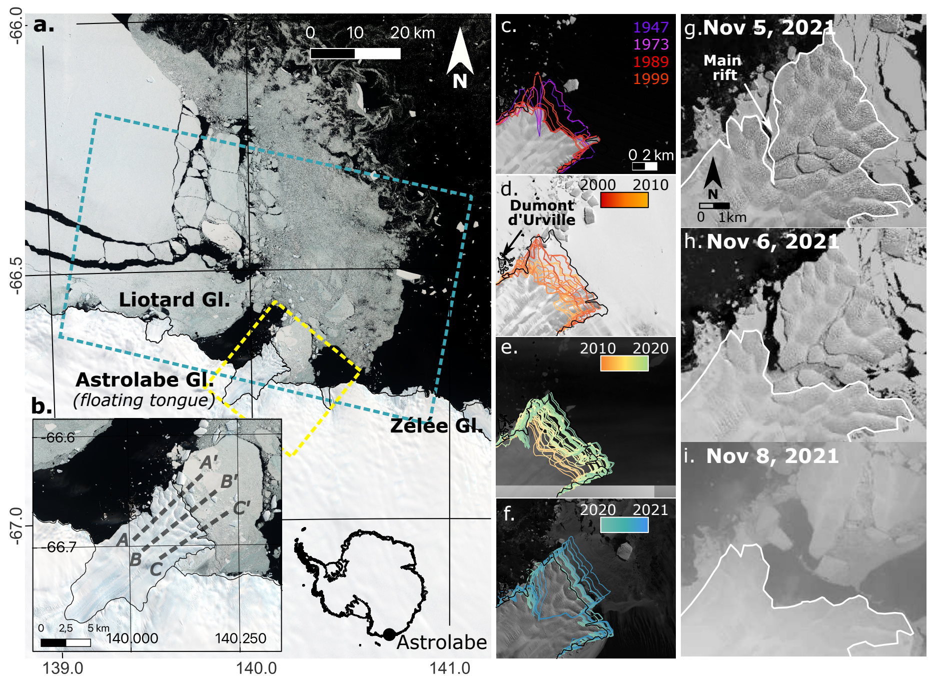
Figure 1: MODIS acquisition of January 10, 2013 centered on the Astrolabe glacier. The acquisition shows the polynya that developed in January-February 2013 at the Astrolabe glacier. The blue square represents the extent of 4,000 km^2 box used to extract sea-ice extent. The yellow square represents the extent of the pixel of the [Fetterer and Windnagel, 2017] product located on the Astrolabe glacier. The area of the pixel is around 600 km^2.

In order to validate this assumption, we used the [Fraser et al., 2020] dataset of landfast sea-ice coverage for Antarctica. This dataset is derived from MODIS imagery, two times a month, with 1 km resolution from 2000 to 2018. Overall, this dataset confirms the trend we show in Figure 5a, and b (Figure 2, below). The only exception is year 2013 where disappearing of landfast sea ice is observed in the [Fraser et al., 2020] while the extent of sea ice remains maximal in [Fetterer and Windnagel, 2017]. This difference is mostly due to difference of spatial and temporal resolution of the two dataset and to the apparition of a polynya at the Astrolabe location in early 2013 (Figure 1). Such a polynya is never observed in the period 2000-2021, but for austral winter 2013.

Figure 2: Comparison between the extension of sea-ice from [Fetterer and Windnagel, 2017] and the extension of landfast sea-ice from [Fraser et al., 2020] for the two areas: a) the 4,000 km^2 box (in blue on Figure 1), and b) for the pixel area surrounding the glacier (in yellow on Figure 1).

I also have trouble seeing the connection between proposed physical mechanisms for ice-tongue stabilization and the analyses performed, particularly in regard to sea-ice extent. The area over which sea-ice extent is assessed is listed as being 4000 square kilometers, but the area chosen is never shown or justified. It is unclear to me how the authors determined the area over which sea-ice extent should matter to the behaviour of the ice tongue. Sea-ice concentration is taken from a pixel in a sea-ice product that covers the ice tongue, but this area is also not shown in the paper, and it is not clear whether it is centered on the ice tongue or whether all areas in the pixel are likely to affect the ice tongue. These decisions should be clearly justified and the areas shown in the paper.

We now present the extent of the 4,000 km^2 box and of the pixel on Figure 1 (and revised Figure 1). We choose the pixel to represent the sea-ice conditions at the glacier ice tongue location and, the 4,000 km^2 box to represent the sea ice conditions in a larger spatial extent which may be susceptible to buttress the Astrolabe ice tongue. Figure 2 of this response letter shows that the variations of sea ice extent [Fetterer and Windnagel, 2017] and landfast sea ice [Fraser et al., 2020] between the 4,000 km^2 box and the pixel is not significant in the first order.



Revised Figure 1.

Still, the fact that there is some correlation between these variables and ice-tongue behaviour is likely to be interesting. However, that correlation is not quantified. The authors claim that it is well-correlated, and there does seem to be some evidence of that in Figure 5, but it is very difficult to interpret the data from the very small panels in the figure. It would be helpful to perform the correlations, perhaps between sea-ice extent and the trend in ice-front position, for example, to better quantify the relationship.

First, we propose a new version of Figure 5 with to improve its readability, taking into account all reviewers' comments on this Figure.
Secondly, we estimated the correlation between the occurrence of calving larger than 0.25 km^2 (Figure 3a of this response letter) and sea ice area and concentration (Figure 3b of this response letter). The correlation is presented on Figure 3c (of this response letter). The Pearson correlation between calving event timing and sea-ice extent is 0.38 with a p-value of $1.5{*}10^{-10}$. The correlation is low but statically significant and confirm the trend observed between the two dataset. With a correlation coefficient of 0.13 and a p-value of $4.12{*}10^{-35}$, we conclude that there is a poor correlation between calving event and sea-ice concentration.

One reason for this low coefficient is likely due to the fact that calving events are "instantaneous" because iceberg detached in few days (see calving of November 2021 for example) while the free ice periods or low concentration of ice tend to last several months in the austral summer. We now integrate and discuss these values in the article.

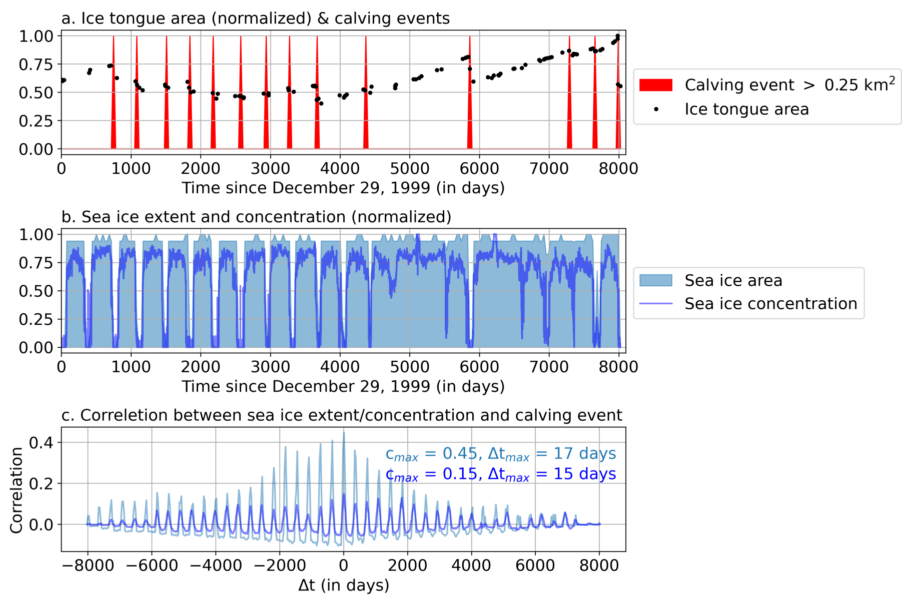


Figure 3: a) Evolution of the glacier area (black dots) and timing of calving events larger than 0.25 km^2. b) Evolution of sea ice concentration and extent at the vicinity of the Astrolabe glacier. c) The correlation between calving event and sea ice concentration and extent.

Finally, the term "melting" appears to be used incorrectly in regard to sea ice. It seems to be used synonymously with a decrease in sea-ice concentration or extent. While this can be due to melting, these variables may also change due to sea-ice advection. Since sea-ice melting does not appear to be assessed in the study, it would be better to use a more general term.

We agree. We removed the term "melting" and replaced it by sea-ice "decrease" or "disappearing" or "sea ice free conditions" as suggesting by the other reviewer.

Other comments:

The manuscript is generally fairly clearly written, but there are typos and grammar issues throughout the manuscript that should be addressed. For example, hyphens between compound nouns acting as adjectives are used inconsistently, and there are many spots where verb tenses don't match the noun form. In line 8, "lead" should be "led." I am also accustomed to the term "transverse" rather than "transversal" being used for strain rates, but that may just be a convention I'm not familiar with.

We reviewed thoroughly the manuscript for typos and grammar. We corrected L8 and "transveral" for "transverse" as suggested by the reviewer.

Section 2.1.2: It would be helpful to have some indication of estimated error in the velocity correlations

Estimating the error on the velocity derived from image correlation is not an easy task. First, we propose to estimate the precision of the yearly estimation as the standard deviation of the estimated monthly velocities (presented in Figure 3a of the article). The result is presented on Figure 4a below. Secondly, the GDM-OPT-ICE service provides the displacement time series with associated RMS error on the displacement inversion [Doin et al., 2011, Bontemps et al., 2018]. The RMS error quantify how reliable is the displacement estimate. We use this to compute the velocity uncertainty as: $2 * \mu_{RMS} i^{yj}/dt$ where $\mu_{RMS} i^{yj}$ is the mean RMS error for year yj and dt is the delay between interpolated estimations of the displacement. The result is presented on Figure 4b below.

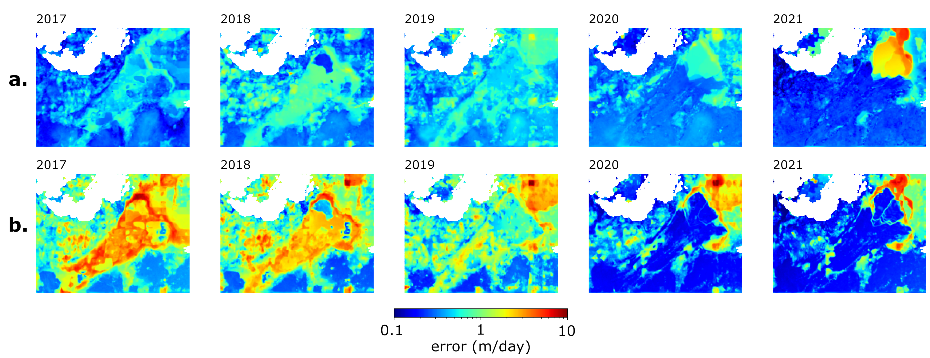


Figure 4: Estimation of the velocity precision as a) the standard deviation of the yearly velocity and b) the uncertainty of the yearly velocity from the RMS error on the displacement inversion.

The results show that both the standard deviation of the velocity and the uncertainty on the velocity decreases strongly after 2019 in the central part of the glacier, (Figure 4). One can observe that on the detaching part of the ice tongue, the standard deviation increases in 2020 and 2021 (Figure 4a). However, looking at the uncertainty on the velocity estimation (Figure 4b) this part

appear to have very low uncertainty (< 0.1 m.day^{-1}) which indicates that the estimation of the displacement is very precised in 2020 and 2021. The high standard deviation in this part of the glacier for year 2020 and 2021 is mostly due to the acceleration of the detachment.
The accuracy of the measurement is estimated from the in-situ measurement and already mentioned in the article. It is further detailed in the next comment.

Section 3.2: It would be helpful to have some more explanation in this section. I think that the GNSS measurements were averaged over the whole year to match the satellite-derived measurements, but that wouldn't be possible with the bamboo stakes, if I've understood the methods correctly. It doesn't necessarily make sense to compare a small-time slice, taken to be ground-truth, to measurements averaged over a longer period of time, but perhaps that's what the comments on lack of seasonal variation are trying to address. It's also not always reasonable to compare point measurements to those averaged over a large spatial area. Finally, I can't quite figure out what the last two sentences in this section are trying to say. I suspect all the analyses discussed in this section are reasonable, but I can't quite figure that out based on what is written.

We propose to detail this analysis in the supplementary information of the manuscript, with the following explanations. The GNSS campaigns available for this study are years 2018 (points 1 to 8, Figure 5a) and 2021 (points 9 to 12, Figure 5a). There is a gap of measurement in 2019 and 2020 due to the COVID crisis. Figure 5b presents the annual time series of the GNSS velocity. The first observation is that the time series do not exhibit particular seasonal variations in this part of the glacier (Figure 5b) for these two years. The second observation is that the velocity seems to be constant over time. Indeed, points 5 and 9 are located in the same area and exhibit a velocity of 1.27 ± 0.14 m.day^{-1} in 2018 and 1.28 ± 0.11 m.day^{-1} in 2021 (Figure 5b). Similarly, points 8 and 12 exhibit the same range of velocity, as well as points 7 and 10 between the two campaigns (Figure 5b).

The second set of in-situ measurements are 16 bambou sticks installed for one week between January 31, 2020 and February 7, 2020. The velocity of the bambou sticks is derived from the measured positions at the beginning and end of the campaign. Although this campaign is relatively short in time, it provides an interesting profile from the glacier limits (point A, Figure 6b) to the center (point A', Figure 6a). As the GNSS time series do not exhibit seasonal variations nor major variation from 2018 to 2020 (considering neighboring) points, we assume the velocity measured by the bambou sticks is representative of the glacier velocity. To confirm this assumption, we compare the bambou sticks velocity to the GNSS velocity of 2018 and 2021 projecting the GNSS position along the bambou profile. The results are plotted in Figure 6b, and we observe that the GNSS velocity are in agreement with the bambou stick velocity (Figure 6b). To explore further the variation of the velocity through time we also extracted the velocity from the MEaSUREs dataset available from 2000 to 2018

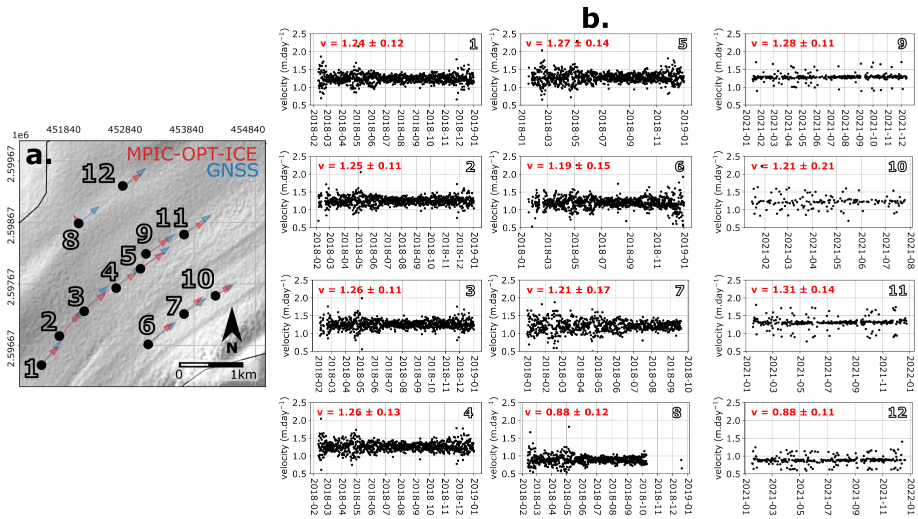


Figure 5: a. Location of GNSS permanent stations: 1 to 8, stations installed in 2018 and 9 to 12, stations installed in 2021. For each measurement point, one year of date is available in 2018 or in 2021. The arrows show the total displacement of the point measured with the GNSS stations (blue) and with image correlation (blue) for years 2018 and 2021. b. Evolution of the velocity for the year of acquisition (2018 or 2021) for each of the 12 GNSS station.

[Gardner et al., 2018]. Except for years 2000, 2006 and 2012, the two datasets are in very good agreement (Figure 6c).

Finally, we compare the velocity derived from the GDM-OPT-ICE dataset and the bambou sticks (Figure 6d). The RMS error between GNSS measurement and 2017 and 2018 GDM-OPT-ICE estimation is 0.76 m.day-1. The GDM-OPT-ICE estimation is particularly poor on the edge of the glacier, while it significantly improves toward the center (Figure 6d). From year 2018, the GDM-OPT-ICE results slightly improve toward the center of the glacier tongue (Figure 6d). From 2019, the accuracy of the GDM-OPT-ICE improves (RMS < 0.25 m.day^{-1}) and one can observe that the two datasets are in good agreement. In 2020 and 2021, the GDM-OPT-ICE velocities tend to be in agreement with the bambou velocity only on the edge of the glacier (Figure 6d, toward point A). In the center of the glacier tongue, GDM-OPT-ICE velocity have larger magnitudes (1.25-1.5 m.day^{-1}) than the bambou stick velocity (1.20-1.25 m.day^{-1}). However, comparing the early months of 2020 (January to March 2020), the derived velocity is in agreement with the bambou sticks velocity measured during this period.

Lines 158-169 says: “It can be noted that compressional strain rates are measured from 2017 to 2020 at the terminus of the glacier tongue with strain rate

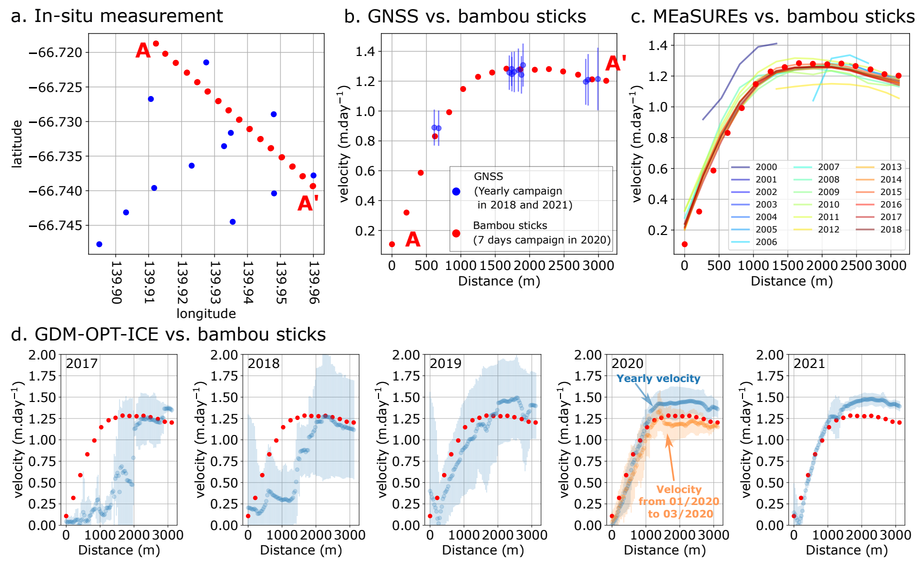


Figure 6: Comparison between in-situ data (i.e. GNSS measurement of 2018 and 2021 and bambou sticks campaign of 2020) with the location of the measurement points in (a) and comparison of the derived mean velocity in (b). The bambou measurements are compared with MEaSUREs yearly velocity [Gardner et al., 2018] from 2000 to 2018 (c) and to the yearly estimation of the velocity from the GDM-OPT-ICE products from 2017 to 2021 (d).

larger than 0.001 day^{-1} while it is not observed anymore in 2021." Strain rates are usually positive in extension, so compressional strain rates could not, by definition, be larger than 0. Is this an absolute value, or is there a different convention being used here?

This is an absolute value as we refer to "compressional strain rate" we then mentioned the absolute value of the strain rate.

Figures:

There are several spots where figures are not referenced correctly. E.g. in section 2.1.1, it seems like the references to Fig. 1 should refer to Fig. 2, and in section 3.4, Fig. ?? should be corrected to the figure number.

We apologize for this and corrected accordingly.

Figure 2: It would be helpful if the y-axes were the same in panels e-g to facilitate comparison between panels.

We corrected the figure accordingly.

Figure 4: I don't find Fig. 4a very helpful, because the panels are too small to clearly see what is discussed in the text. Consider showing just a few panels that are necessary to the analysis, and shifting the rest of the panels in larger form to supplementary information. I think Fig. 4b is a very clever way to display the information.

We prefer to keep the figure 4a as it is, we do think the information is readable. We modified figure 4c in order to point out the fissures appearing in June 2021.

In general, it would be helpful to have more labels in the figures that correspond to what is discussed in the text. For example, the discussion talks about the "main rift" and refers us to Fig. 3 on line 194. I can make some guesses based on Figure 3 about what the main rift is, but I would rather have a label or two that helps me know exactly what the authors are referring to. It would be helpful to have those labels in Figure 4, as well.

Labels/boxes are already present on Figure 3c corresponding to "A: the main rift" and "B: secondary rift". However, we tried to improve this on all figures. We added those label on all sub-figures when the rifts are visible on Figure 3, we added the location of the rift on the revised version of Figure 1. We also indicated the network of fissures in Fig. 4c. We hope this help the reader.

References

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[Fetterer and Windnagel, 2017] Fetterer, F., K. K. W. N. M. M. S. and Windnagel, A. K. (2017). Sea ice index, version 3.

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[Gardner et al., 2018] Gardner, A. S., Moholdt, G., Scambos, T., Fahnstock, M., Ligtenberg, S., Van Den Broeke, M., and Nilsson, J. (2018). Increased west antarctic and unchanged east antarctic ice discharge over the last 7 years. *The Cryosphere*, 12(2):521–547.