Reviewer 1

Review comments for "Slowdown of Shirase Glacier caused by strengthening alongshore winds" by Miles et al. (tc-2022-126)

General comments: This study estimated a time series of ice flow speed of Shirase Glacier for 47 years (from the early 1970s to the present) and showed that Shirase Glacier had experienced a long-term slowdown. While many Antarctic ice sheets/glaciers are losing their mass, the behavior of the glacier is very unique among Antarctic ice sheets/glaciers. The authors have collected several different datasets (position and thickness of ice sheets/glaciers, wind speeds, ice-shelf basal melt rate, and so on) and integrated them into their conclusion. The main conclusion of this study is that alongshore easterly wind plays an important role in regulating the glacier dynamics (speed and thickness) through the wind-driven CDW transport onto the continental shelf regions. Although I have put several comments below, the paper is nicely written and prepared, and thus I recommend publishing in The Cryosphere.

We thank the reviewers for their positive comments on our manuscript and the constructive suggestions listed below.

1. Abstract and Introduction.

Sentences about the mean field and the temporal variability are mixed up. It is very confusing. The present form gave me (readers) an impression that it is very natural for warm ice shelves to experience increases in ice-shelf basal melting over decades (and future). As this paper's subject, I think it is not obvious.

This is a good point. We have amended parts of the abstract and introduction accordingly. We confirm that mCDW inflow onto the continental shelf does not cause mass loss or gain, it is how the inflow changes through time that is important.

2. Figure 1: Please add information of longitude and latitude.

We have added longitude and latitude to figure plots

3. L97: ">10 km" Is the inequality sign orientation correct?

The sign is correct and we have changed the sign to text 'greater than' in response to a comment from reviewer 2.

4. L147 In my reading of the reference, they used ERA-Interim (not ERA5) to force the ocean model.

Amended

5. L162: I don't understand the equation. Alongshore wind can be calculated from the inner product between the defined unit vector and wind vector.

The equation calculates wind speed relative to the alongshore direction (80°) to give alongshore wind

6. L170: The expression "extent" should be "length" if the unit of some figures uses "km"/"m/a".

Amended

7. Figure2e: Which side is grounded/floated?

We have added a label confirming which side is grounded and floating

L189-203: How did you calculate the percentages (8% and 4%)? What is the reference speed?

The reference speed is the speed from the first year given in the date range. For example:

"Between 1988 and 1996 we observe a 2 ±7% slowdown" - refers to ice speed being 2 ±7% slower in 1996 relative to 1988.

9. Figure 3: Vertical axes for "Ice speeds" and "Alongshore Wind Speed" should be exchanged to place the explanatory variables on the right side. Please add short tic marks showing 1-year interval on the horizontal axis. Since data for the 1960s and 1970s are available in ERA5, please extend the black line for wind speed.

We have exchanged the axis and added the tic marks. We have not extended the ERA5 record back to the 1960s and 1970s. This is because large uncertainties have been documented in the ERA5 product in Antarctica before the 1980s. Please see Bell et al. 2021

Bell, B., Hersbach, H., Simmons, A., Berrisford, P., Dahlgren, P., Horányi, A., et al. (2021) The ERA5 global reanalysis: Preliminary extension to 1950. Q J R Meteorol Soc, 147(741, 4186– 4227. Available from: https://doi.org/10.1002/qj.4174

10. Section 4.2: It would be helpful for readers to insert a figure showing the linear trend of wind (e.g., 1979-2020 or the full length of your analysis 1960-2020), like Fig 2a in Hazel & Stewart (2019).

We have added a figure showing the linear trend in zonal wind over the wider region between 1979 and 2021. This shows a spatially widespread trend for increasing easterly winds over the continental shelf boundary in Enderby Land and more limited change in zonal wind in Dronning Maud Land.

11. 331-335: The sentence is just speculation and is unsuitable in conclusion, although it is ok in Discussion (4.2).

We have removed this sentence from the conclusion.

12. I feel that some figures/panes showing precipitation are missing. Please consider adding the panel in Fig. 3.

We have added this to figure 3. This shows that while precipitation has very strong interannual variability, there is no obvious trend nor any obvious link with the slowdown of Shirase Glacier.

Reviewer 2

This paper presents a relatively long time series of flow speeds and frontal positions of the Shirase glacier. It correlates those with along shore wind speeds from ERA5 and argues that it is these strengthening winds that, via decreased melt rates, drive the observed slowing of the glacier.

We thank the reviewer for taking the time to comment on our manuscript and for the constructive suggestions listed below.

Major points are below and in text comments in the attached pdf:

It looks to me like there is no correspondence between the local measurement of wind speeds and the modeled ones from ERA5. (And that is probably why the local one is in the supplement and not in the main text) .

Unless there is a good reason to think that the data from the station are representative of the wider area, should these data be included here? And if yes, the disagreement between the datasets needs to be addressed.

This is somewhat important to clean up, as the correlation between the flow speed and wind strength is the main scientific result of this paper.

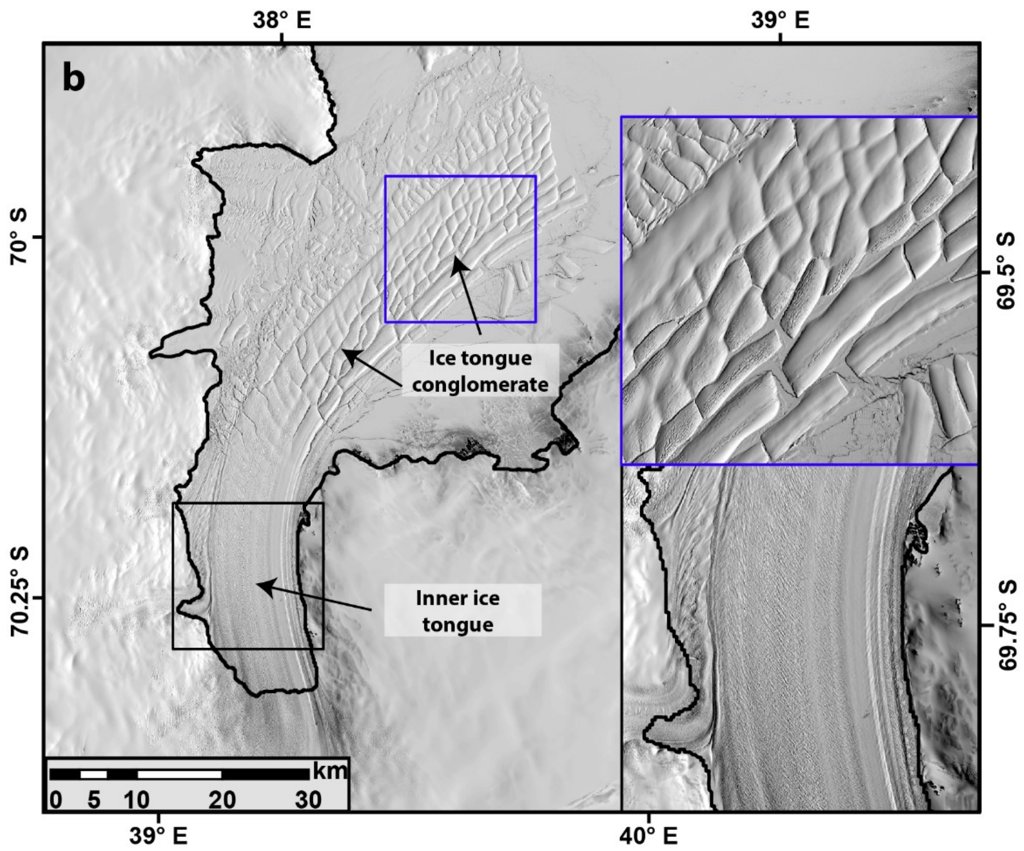
The observations at Syowa station are from one point in space; the wind data from the ERA5 data represents average wind conditions over a much larger area. Therefore, the ERA5 data is the preferred dataset when considering how changes in wind may influence ocean circulation and subsequently melt rates. We originally included the Syowa data because it is observational (as opposed to reanalysis) and located within the Lutzow-Holm Bay. We disagree that there is no correspondence between ERA5 and the Syowa observations. In our view, the timings of the peaks and troughs are very similar with the exception of a spike in the 1980s in the Syowa dataset that is not present in the ERA5 dataset. However, for clarity we have now removed the Syowa wind observations from any discussion in the manuscript and supplement.

The authors claim that during some intervals the buttressing from fast ice and ice tongue mixture doesn't matter (line 249) and that in other time intervals it matters (270).

To clarify there are two parts to the ice tongue the unconstrained conglomerate and the constrained inner part of the ice shelf. We have highlighted this on the new figure 1b (copied below).

Fast ice is important in determining the length of the unconstrained part of the ice tongue. When fast ice breaks-out in some austral summers it allows the loosely bound icebergs that form the conglomerate to drift away into the open ocean. However, there is no evidence that fast ice or the conglomerate itself (see Fig. 5a) offers any significant buttressing to the inner ice tongue of Shirase Glacier. Rather, it merely controls the length of the group of loosely bound icebergs that form the Shirase conglomerate.

We have clarified these points in the manuscript by including a new figure (1b) that highlights more clearly the two sections of the ice tongue and referred to it throughout the manuscript and in the text at Lines 274-278:



New Figure 1b highlighting the different sections of the ice tongue

I couldn't find in the paper where the authors quantify how thick the Shirase ice tongue needs to be to provide sufficient buttressing (the provided reference of Reese et al is not relevant to this claim as that paper doesn't address the buttressing evolution through time, only instantaneous change). In both cases the ice tongue is largely unconfined so supposedly its thickness changes would not have a significant dynamical impact?

As clarified above, the ice tongue has two sections. Firstly, there is unconfined ice tongue conglomerate, where ice thickness changes will have no impact on buttressing. Secondly, there is also the highly confined section of the inner ice tongue that is more akin to an Antarctic peninsula/Greenlandic outlet glacier. Intuitively, any changes in ice tongue thickness here would be important for buttressing because it will directly alter the level of contact between the ice tongue and the fjord walls. The modelling outputs from Reese et al. show this clearly. Changes in the thickness of the inner constrained section of the ice tongue are important for buttressing (purple square; Fig 4c). We do not feel there is the need for transient experiments because we already know that the inner ice tongue has thickened (Fig. 4b) and ice speed is sensitive to changes in ice tongue thickness (Reese et al., 2018; Fig. 4c).

We have improved the manuscript by again clarifying that it is thickening in the inner ice tongue that will have increased buttressing and contributed to the slowdown. Please see lines 298-301.

Also, Kusahara et al highlight the role of fast ice for modulating the strength of warm water intrusions. How does that effect fit in with the story presented here? Do you see a correlation between fast ice cover and flow speeds, as flow speeds are inversely correlated to melt rates?

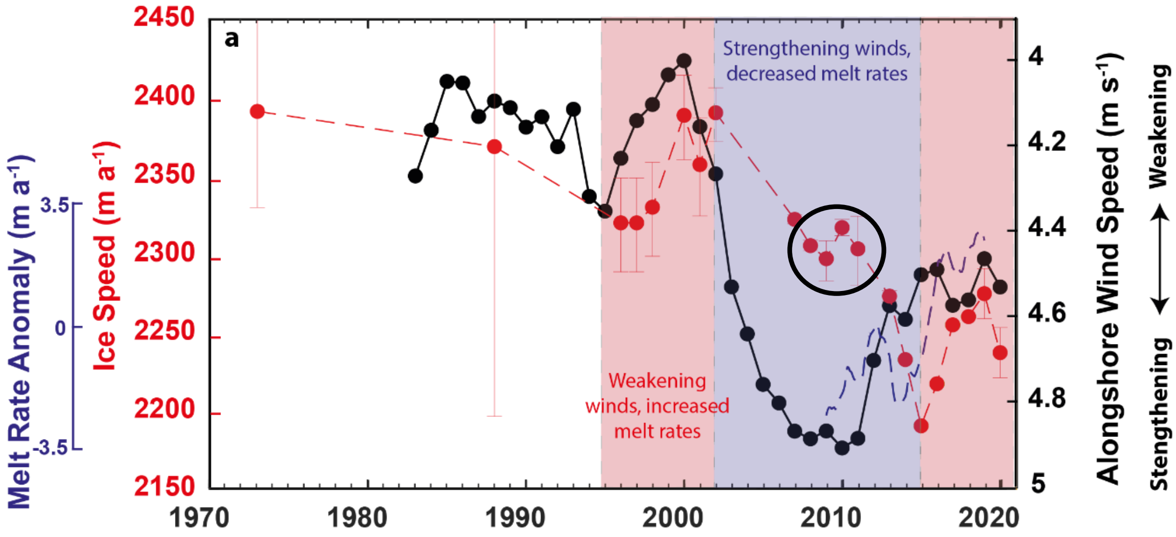
The background here is that the Lutzow-Holm bay is semi-permanently filled with fast ice. There is the occasional partial break-out in some austral summers where the fast ice vacates the bay for a few weeks (Please see Fig S1 for an indication of the consistency of fast ice coverage). Aside from these occasional fast ice breakouts, the fast ice is always there and has been for many decades (entire observational record).

The Kusahara et al. experiment simulates the difference in melt rates at Shirase Glacier between 100% fast ice conditions (close to actual conditions) and hypothetical 0% fast ice conditions and strong sea-ice production in the bay. As the reviewer notes above, Kusahara et al. highlight that fast ice does have a modulating effect on the warm water intrusions in a hypothetical sense. However, throughout the observational record there have been no major changes in fast ice conditions (remained close to 100%), so there is no need to investigate this as a possible explanation.

We have improved the manuscript by further justifying our use of the fast ice basal melt modelling output at Lines 161-165 and clarify that there have been no major changes in fast ice coverage throughout our observational time period at Lines 274-276.

There is a really nice correlation between observed ice speed and modelled melt rates from about 2012 on. However, prior to that ~2008-2012 the sign of the correlation is opposite, high flow speeds associated with low modelled melt rates. Can you explain the full time series? At the moment the story is only consistent with the post 2012 period.

Firstly, a general consideration is that these are noisy datasets, particularly when looking at very short inter-annual time periods. Arguably, the uptick in melt rates in 2008-2011 does actually coincide with cessation in the longer-term slowdown (highlighted in black circle below), which is broadly what we would expect. Can we explain the full time series? The full time series stretches back to the 1970s and we have regular speed measurements from the mid-1990s. We think the general pattern that alongshore wind is driving multi-year patterns in ice speed is robust.



There are a number reasons as to why one would not expect a perfect relationship between melt rates and ice speed, particularly over very short timescales. Melt rates do not necessarily have to correlate with ice speed. For example, melt rates could increase, but the ice tongue could continue to thicken at a lower rate. In this scenario, a slowdown in ice speed could be expected. A second consideration is that we would not necessarily expect a linear relationship between ice tongue thickness and ice speed because much depends on the local geometries. Some form of lag time (months) could also be expected.

We have amend the text to better reflect this at Lines 311-315

I have checked a few of the many references and found some of them to be incorrect or inaccurate, some examples are in the pdf. Mainly, the authors should cite observational references for observational claims, and clarify when a cited paper shows a result, hypothesizes about it, or cites that claim from elsewhere (in which case that other cited paper should really be referenced).

We have been through the comments in the PDF which are useful and help improve the manuscript, so thank-you for this. We have carried out several amendments on the basis of these comments and can be seen in the tracked changes.

Would it be possible to also plot precipitation time series on Figure 3 and analyze the relative importance of the precipitation vs melt rate changes? This would be useful, as it seems from the way the paper is set up (at least at the beginning), that the authors discovered winds to be the main driver of flow speed changes while before it was thought to be precipitation. It is probably not exactly like that but that is the feeling the paper passes on at first.

We have added this to figure 3 in a new panel. This shows that while precipitation has strong interannual variability, there is no obvious trend nor any obvious link with the slowdown of Shirase Glacier.

The authors claim that their results of wind driven-basal melt induced-speed control mechanism extend to the whole of Queen Maud Land. While that is very reasonable hypothesis it is something that was not shown in the manuscript, so this should probably remain as a hypothesis in the abstract and other places.

We have removed these sentences from the abstract/conclusion.

Is there some evidence that ERA5 provides reliable wind info over the studied time period at Shirase or at least over QML?

ERA5 is generally considered the most appropriate reanalysis dataset for Antarctica and has been used in many studies. The mechanism in which winds at the continental shelf boundary can modulate mCDW inflow onto the continental shelf is relatively well understood. That is, when winds strengthen we would expect the thermocline to deepen, melt rates to decrease and the glacier to slowdown. The fact the observed slowdown (which we have very high confidence in) coincides with a strengthening in winds is arguably, in its self, a form of validation in the wind ERA5 wind product i.e. the reverse argument. This is because it is consistent with our understanding of the wider physical mechanisms involved.