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 oppose 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.

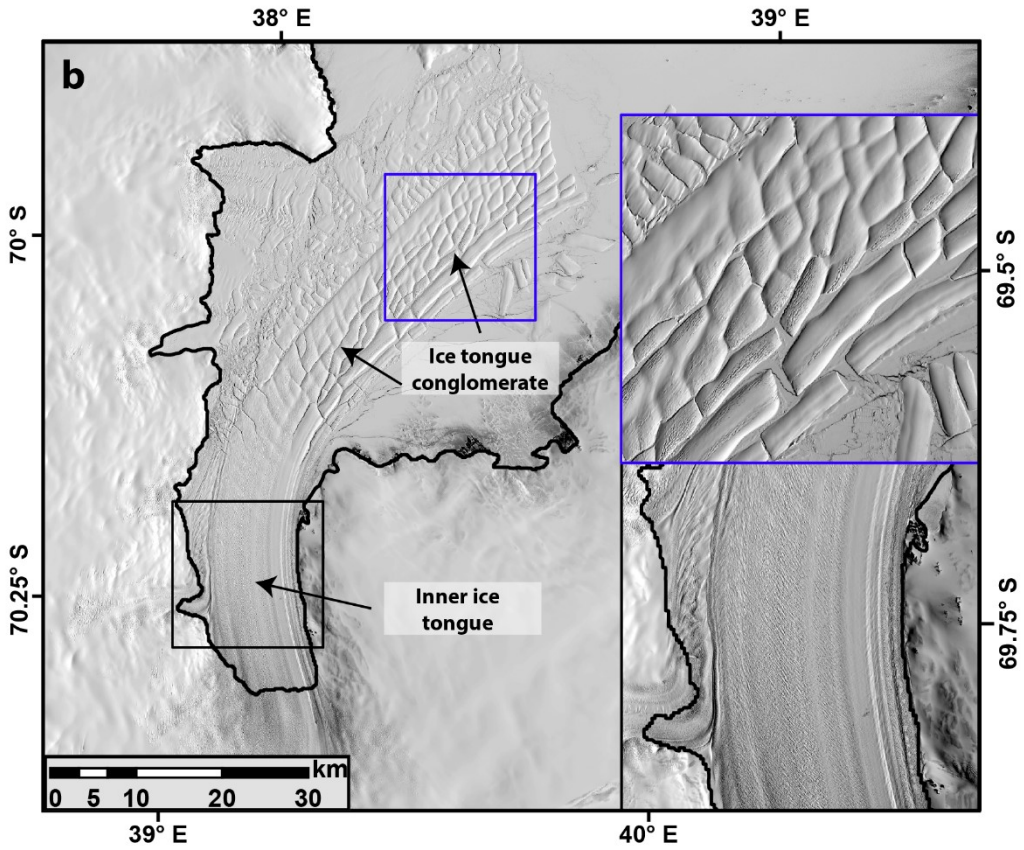
The authors claim that during some intervals the buttressing from fast ice and ice tongue mixture doesn't matter (line 249) and that in other time intervals it matters (270).

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 some austral summers it allows the loosely bound icebergs that for the conglomerate to drift away into the open ocean. But there is no evidence that fast ice or the conglomerate its self (see Fig. 5a) offers any significant buttressing to the inner ice tongue of Shirase Glacier, it merely controls the length of the group of loosely bound icebergs that form the Shirase conglomerate.

We have improved the manuscript by including the new figure 1b that highlights more clearly the two sections of the ice tongue and referred to it throughout the manuscript. We have also added the following sentence at Line 249:

'It is important to note that fast ice only helps control the length of the ice tongue conglomerate (Fig. 1b) and it is unlikely that the fast ice has any major role in providing buttressing for Shirase Glacier.'



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. The unconfined ice tongue conglomerate, where ice thickness changes will have no impact on buttressing. But there is also the highly confined section of the inner ice tongue that is more akin to a 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 output 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).

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). But 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 you mention above, from the experiment they highlight that fast ice does have a modulating effect on the warm water intrusions. But the key message here is 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.

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.

We would expect melt rates to correlate with wind speed, which it broadly does given inherent noisiness of the data. However, 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. Arguably, even taking all the above into account, the uptick in melt rate actually does actually coincide with brief cessation the slowdown at around 2010.

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. There is also perhaps a reverse argument, while this study's main point is that the recent slowdown has been caused by strengthening easterly winds. It also makes physical sense that the ice speed (which we have very high confidence in) broadly correlates with alongshore wind given our understanding of how we would expect the ocean to respond to changes in winds.