Reviewer 1

This is the second review of the paper. I appreciated the authors for carefully responding to my comments in the first round. Now, I recommend publishing the manuscript for the TC. Here are some technical comments that can be easily modified.

We again thank the reviewer for taking the time to comment on our manuscript.

1. Thank you for adding longitude and latitude information on the axes. It would be nice to add thin or dashed lines on the figures.

In Fig.2, there are duplications of 37E on the upper axis and 40E on the bottom axis. Please correct them.

We have added thin lines of longitude and latitude and corrected Figure 2.

2. Figure 3: The unit of the precipitation with m/a would be better (to be consistent with panels a and b).

As we understand it, this comment is requesting to convert the units of precipitation from kg m^{-2} a^{-1} to m a^{-1}. This is somewhat tricky because the elevation data has been corrected for firn compaction and penetration. Therefore, simply converting precipitation to m a^{-1} is not comparing like for like.

Reviewer 2

This is my second time reviewing this paper. The paper has improved but a few issues still remain. Comments in chronological order follow here and minor/typos are in attached pdf.

We thank the reviewer again for taking the time to provide constructive comments on our manuscript and we confirm that we have also addressed the comments/typos in the attachment.

The tracked changes pdf was not too useful as it only showed what was added, but not what was originally in the paper and removed.

We have made sure to include both the added and removed sections of the track changes in this revised version

1) Figure references are sometimes wrong - I corrected some in the attached pdf but please check the rest too

We have corrected all figure references

2) Fig 2: panel 2d not referenced in text, is it perhaps referring to some part of the methods? If not maybe remove it or explain why it is shown

Thank-you for spotting this, the panel 2d is used in the methods and we have now referenced it correctly.

panel 2c shows surface elevation and is used to argue about grounding line migration (Line 209). It appears to me that there is primarily a vertical offset. I think what the plot should show instead would be there derivative of surface elevation (the surface slope) and that is the plot that should be used to assess the grounding line advance

The consistent seaward migration of the surface slope profiles can either be explained by a very large and consistent increase in precipitation e.g. vertical offset, or the seaward migration of the grounding line e.g. horizontal offset. There is no evidence of any consistent increase in precipitation. This combined with the observed slowdown of the glacier and inland thickening would be highly indicative that the elevation profiles are largely reflecting a horizontal offset and grounding line advance.

This is now reflected in lines 213-217.

We note that plotting the surface slope with these DEM's at Shirase is challenging. The surface of the Shirase glacier is highly crevassed, meaning the slope is very noisy. As far as we can tell this method of estimating grounding line location via the inflection point has only been successfully used on very smooth and relatively slow flowing ice shelves.

Fig 3: panel 3b reads thickness change but it is not specified over what time interval this change occurred/or with respect to which thickness reference. Same on line 214.

We have corrected the figure caption to state that the thickness changes reported are in respect to the reference epoch of 2009/10. The text on line 214 (now line 209) is correct because it reports on the trend between two dates.

panel 3a - the melt rate time series changes color from blue to purple, probably caused bu shading on top of line rather than line on top of shading.

This has been amended.

panel 3c

There is maybe a comment missing here.

Fig 4: It would be great if you could add wind speed vectors (mean over the time period, for example) on top of the trend, so that we know what the winds look like now and have a sense of what Ekman convergence is doing at the moment.

We have added mean wind vectors (1979-2021) on top of the zonal trend.

3) 275: "there is no evidence of any major changes in fast ice 275 coverage over the course of our observational time period"

This is a bit of a contradiction to a statement few lines above: "we note that the observed fluctuations in ice tongue extent are 270 correlated with landfast sea-ice" and the fact that there is quite a bit of ice tongue extent change over the course of the observations (Fig 2b)

We have removed this sentence from the manuscript to avoid any confusion.

4) The authors still give 1988 as a single supporting evidence of the absence of fast ice influence on the glacier flow speed, but this year has giant uncertainties, so can hardly be used as such. I already commented on this the last round of reviews but this hasn't been addressed. At the very least it has to be explicitly highlighted on line 279 how large the uncertainness are.

We have highlighted the uncertainties of the ice speed measurement on line 279 (now 283). We also reference a new study published after we first submitted the manuscript that uses a higher temporal velocity measurements to show no changes in ice speed at the Shirase grounding line immediately after a fast-ice breakout event in 2017 (Nakamura et al., 2022).

5)*283-292: The authors discuss alongshore wind speed in this paragraph without referring to the direction, which seems to be a key ingredient

*easterly vs westerly and alongshore also needs to be made consistent throughout the manuscript as there is about 20-30 degree offset around Shirase in those two quantites

We have clarified "alongshore easterly winds".

6) Precipitation

Reading this paper gives a feel that there was this increased precipitation hypothesis for explaining the Shirase slowdown, and now a new hypothesis is presented (strengthening easterlies). This is why I asked for the precipitation plot earlier which is now shown (are there any error bars/uncertainties on that one available btw?).

There are no available error bars and we have noted this on the figure caption.

It is interesting that there actually isn't an increasing precipitation trend, and so it would be worth highlighting that discrepancy in comparison to the study that did state that hypothesis. Did the previous study focus on a shorter time period? If so, mark that time period. Did they use a different data set? If so, then shot timeseries from that dataset too.

We have added further discussion on this interesting point on Lines 308 – 312. While there is no longer-term trend for increasing precipitation, there was a well documented series of well documented extreme anomalous precipitation events between 2009 and 2011. This might be the driver of the inland thickening.

7) trend vs higher frequency variability

312-315. I think I asked in the previous review for an explanation why melt rate changes seem to precede changes in winds when the relationship should be reversed. The correct relationship hold for the overall trend, but not for the variability. That has not been addressed. I think these lines might be trying to do that but they don't do the job. I don't know what authors mean by a "perfect" relationship. It is ok to state that there is a discrepancy in terms of the variability, and that it might be too much to ask for because of reasons a, b, c.

I think the previous comment was on the relationship between modelled melt rates and ice speed (not wind). For modelled melt rates and wind we would expect a correlation and this is what we see in Figure 3a. When wind strengthens melt rates go down and when wind weakens melt rates go up. We assume that this comment (and perhaps the previous comment) is referring to the slight uptick in melt rate slightly preceding the sharp increase in alongshore wind by 1-2 years between 2008 and 2011. An important consideration here is that we present alongshore wind as a rolling 5 year mean, the modelled melt rate is plotted as a 1 year running mean because it is a shorter time series. We suspect this is the reason for the apparent slight discrepancies in timing.

We have removed reference to "perfect relationship". Instead we highlight the difference surrounding the smoothing of both datasets in the manuscripts at lines 321-324