

The Cryosphere Discuss., author comment AC2
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Reply on RC2

Sarah S. Thompson et al.

Author comment on "Glaciological history and structural evolution of the Shackleton Ice Shelf system, East Antarctica, over the past 60 years" by Sarah S. Thompson et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-265-AC2>, 2022

We thank Anonymous Referee #2 for their considered review of our manuscript. Below we respond to each comment, with the reviews original comment shown in italic text and our response in bold text.

Review: This manuscript uses a suite of remote sensing data to map changes in ice extent, structure and velocity across the wider Shackleton system. The manuscript compliments recently published work showing recent grounding line retreat and acceleration of Denman Glacier, but also includes detailed and novel observations across the wider understudied Shackleton system. The authors use these observations to conclude that there has been limited change across the Shackleton system across the observational time period. They also then simulate the response the Shackleton system to a hypothetical loss of floating ice to demonstrate the systems sensitivity to any future ice shelf loss.

Overall, I think the manuscript contains some interesting and novel observations of the Shackleton system that are worthy of publication. In particular I think the 50 year record of rifting evolution across the Shackleton Ice Shelf is a very nice contribution. The background here is while there has been some recent studies focussing on Denman Glacier, we know very little about the recent behaviour of the many other glaciers that feed the wider Shackleton system, so these results are valuable. However, at the moment I think these interesting results are somewhat lost in the manuscript. It seems like quite a jump and a distraction from discussing these detailed annual scale observations, to discussing the response of the Shackleton system to the hypothetical loss of all floating ice 400 years in the future, which then turns into a discussion as to how the deep trough of Denman may favour vigorous channelization of the subglacial meltwater system close to the grounding line. At the moment I am not sure what the main focus of the manuscript is. I think the manuscript would benefit from being more streamlined, with a greater focus on the novel observations. I have included some more detailed comments below.

These comments echo those of Anonymous Referee #1 and have inspired us to remove the modelling part of the manuscript and make it the focus on a follow-up manuscript instead. In making appropriate revisions to the manuscript as a whole this will allow us to re-focus the manuscript on the novel observations, as suggested by the Anonymous Referee #2.

Observations: The authors state that there have been no significant annual variations in

ice flow speed across the Shackleton system. I would argue that the use of 'significant' is not appropriate, what is 'significant' variations greater than 50 m yr⁻¹ 100 m yr⁻¹ etc?.

In this context we are using significant to mean greater than the uncertainty and in this case, we do not find that any changes are greater than the uncertainty. We will clarify this use in the text.

The plots in Figure 9 give a good overview of the longer-term changes in ice flow speed across the region. However, because they are in m/day and the scales are somewhat stretched it is difficult to determine if there has or has not been any annual variations in ice flow speed. A variation of 0.3 m/day equates to around 100m yr, which would be larger than the uncertainty of the velocity products and would be an interesting result. Are there similar scale variations, particularly in the faster flowing sections of the Shackleton system, to my eyes it looks that there could be, but I could be wrong, I really cannot tell from the plot alone?

May we refer to our corresponding answer to Anonymous Referee #1. The use of both m/year and m/day is deliberate as it appropriately reflects the temporal resolution of the available data. With all due respect, in our experience it is usually incorrect to state that "0.3 m/day equates to around 100m yr" because our experience with tracking glacier flow speeds over several decades shows that velocities can change on a daily basis.

In Figure 8 the authors plot an ice speed difference map between 2019 and 2020, I think to illustrate the acceleration of parts of the Scott Glacier. I think these plots are useful in giving a broad overview of the changes in ice speed. Could they also do this over a longer time period, maybe 2000-2010 and 2010-2020 (brackets whatever the availability of velocity data allows)? This would probably be the best visualization of the speed changes over the observational period.

We would be happy to add time periods, however this time-period was carefully chosen as it is the period where the multiple velocity maps allow the uncertainties to be much lower than the difference. Other time-periods are generally rather less clear. We are however happy to add such requested figures if desirable.

One of the most striking observations is the migration of the shear margin near Chungunov Island over just a few years. This is a somewhat unique observation. The authors have collated this wide range of velocity data, while they have tended to focus of ice speed, could they also focus on the velocity directional data? Is this change in shear margin caused by the whole Denman ice tongue 'wobbling' or a more localised change to do with Scott?

We can see from Figure 3b that Denman tongue does not appear to be 'wobbling' and all of the evidence we see indicates that changes are confined to Scott, likely a combination of ice front calving close to Mill Island, the rift opening from Chugunov Island towards Mill Island and the speed increases. While we are happy to calculate a flow direction difference between 2019 and 2020, we know from previous experience plotting directional arrows onto individual ice speed maps would provide a much clearer illustration of any changes in velocity direction.

Modelling: My expertise lies in remote sensing, so I am not in a position to comment on the methodological details of the modelling. But I did find the description of the modelling experiment carried out to be lacking. In the discussion it is stated that: 'The upper limit scenario of forcing in our BISICLES model runs suggests that noticeable grounding line

retreat occurs in the Denman Glacier over the simulated 400-year time period' But in the methods section there is no mention of an upper or lower limit scenario, nor any mention of the timescales of the simulation. Aside from the basic description of model, there is only a very limited description of simulation. It is essential that the details here are expanded.

In wider point, while I think it could be a useful contribution to repeat the experiment in Martin et al., but with BedMachine, I did feel the modelling appeared as somewhat left field in the manuscript and appears as a bit of a jump in the discussion from the main body of observations. The general tone of the manuscript is that very detailed remote sensing observations have shown limited changes in the floating ice in the Shackleton system... But then the manuscript jumps to.. 'now we simulate the unrealistic loss of all floating ice in the Shackleton system'... The scientific rationale for this is unclear to me? Of course, it is entirely up to the authors, but I would point out that I think the detailed observations have the potential to be a nice contribution alone.

The term upper limit was used to indicate the upper limit of change in the system by removing all of the floating ice, rather than the upper limit of a series of scenarios. May we refer Anonymous Referee #2 to our response to Anonymous Referee #1 for an explanation of the anticipated logic of connection between observations and modelling in our original manuscript. Following up on these matching anonymous referee comments, and also explained in that response, we consequently propose to split the observational and modelling components into two manuscripts, removing the modelling from this manuscript which already largely focuses on observations.

Specific comments:

Line 59: I would not describe the acceleration of Denman Glacier since the 1970s as a short- term fluctuation

Whilst an increase in ice flow speed of the Denman Glacier clearly occurred between the 1970s and 2017 detailed examination of the timing of this change (as matched by our own feature-tracking based inferences) reveals that almost all of it happened sometime between 1972 and 1989, with very little change since. We will review this aspect of the manuscript and make appropriate revisions to emphasise the more recent stability in ice flow speed.

Line 62: 'the glacier' – please clarify in the text that you are presumably referring to Denman Glacier.

Yes, we are referring to the Denman Glacier.

Line 113: Landsat 1 was not mentioned in the above paragraph? Was it used?

Landsat 1 MMS acquired 27th February 1974 was used in Figure 5b and will be added to the paragraph describing the data used.

Line 118-129: What software was used for the feature tracking?

The standard Gamma software was used, and this information will be added to the manuscript.

Line 164: That of Rignot (2011) – Do you mean the MEASURES ice velocity mosaic?

This section will be removed from the manuscript as outlined above.

Line 173-178: I think much more detail is needed here (see comment above)

This section will be removed from the manuscript as outlined above.

Line 189: Not sure 'remarkable' would be the word I would use here

Will remove remarkably from the sentence.

Line 243-245: Its nice to see the changes between 2019 and 2020, but I think you could also expand to include 2000-2010 and 2010-2020, maybe even 2000-2020.

As outlined above in response to general comments, this time-period was carefully chosen as it is the period where the multiple velocity maps allow the uncertainties to be much lower than the difference. Other time-periods are generally rather less clear. We are however happy to add such requested figures if desirable.

Line 250: Please use the correct citations for the Measures and ITS LIVE velocity products. I think the correct details can be found on each respective website.

The correct citations will be included for both.

Line 267: Is d Chugunov Island?

C is Chugunov Island and has been relabelled accordingly.

Line 269: The paragraph is discussing modelling results. Therefore, I think it needs a new appropriate subsection, currently it is under 'ice flow speed'.

This section will be removed from the manuscript as outlined above.

Line 285: 'Not changed significantly' What is 'significantly'? Observations of Denman have shown that its grounding line has been retreating and that it has been accelerating and losing mass, this is contradictory.

As explained in our response to Anonymous Referee #1 it is natural for systems such as the Shackleton Ice Shelf to undergo some variability. Within the framework of our regional observations of system structural evolution (and indeed modelling experiments) previous authors' inferences point to relatively minor dynamic changes that do not necessarily point to a system that is on its path to instability. We will clarify this point in further detail in the revised manuscript.

Line 285-302: I think you could be more detailed in this section. One of the key results here is the lack of propagation of the rifting across the Shakleton over the past 50 years, with the exception of some small activity over the past few years. This is a key result. What does this mean and how does it tie into the wider body of literature? In other regions rifting evolution has been key to ice shelf stability? How important could the melange that fills these rifts be (see recent Larour Larsen C paper in PNAS)?, etc..

There have indeed been a series of manuscripts that examine the importance of suture zones and their mélangé fills on rift propagation, not least those on Larsen C cited here, as led by some of the current authorship team (Kulesa et al., 2014, 2019, both in Nature Communications), that pre-date the Larour et al. manuscript quoted here. So we completely agree, rift mélangé is indeed critical to ice shelf stability and we are very happy to add an expanded explanation to

the revised manuscript.

Line 321-338: I struggle to see the relevance of this section to the manuscript. Fig 1a: The scale is a little difficult to make out because of the transparency

This paragraph left both anonymous referees confused. We had included it to set the scene for processes that may control the evolution of the ice shelf and for possible future investigations of key properties and processes that we do not understand well at present. It is clear that we need to much better clarify its connection with our observations reported earlier and we will do this by comprehensively revising the paragraph and the parts of the manuscript that it connects to.

Fig 10: The high strain rate band going across the Denman ice tongue – presume there is no evidence of any recent rifting in this location?

No there is not any evidence of rifting in the area, we have examined all of the imagery in detail, but it is an artifact. We will ensure this is described clearly in the figure description.