

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2022-55

Ted Scambos (Referee)

Referee comment on "Seasonal land ice-flow variability in the Antarctic Peninsula" by Karla Boxall et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-55-RC2>, 2022

Review of Boxall et al., The Cryosphere;

Seasonal land ice-flow variability in the Antarctic Peninsula

The paper uses a combination of Sentinel 1 A/B velocity data and ITS_LIVE Landsat-8 velocity data, along with a careful mapping of the grounding line, to assess the scale and extent of a clear seasonal variation in flow speed for glaciers inflowing on both sides of the George VI ice shelf. The paper makes a strong case for the validity of the signal they see, and the seasonality is quite sharp and clear, albeit not large. The authors attribute this to variation in ocean forcing.

This is a well-written, well illustrated and described study that breaks new ground on sensitive detection of seasonal velocity signals (a -few- other studies are out there now for some other regions). However, the attribution of this signal to ocean forcing is untenable. While this means that the paper **absolutely** needs to be revised, in fact 80% of the paper is ready to go. **It is necessary** that the paper revise the attribution to discuss the pros and cons of ocean forcing and surface melt percolation to the bed equally. That is, if the following considerations do not convince the authors that surface melting has in fact a far stronger case for this speed-up. I would like to point out that such a conclusion, or preferred but qualified causal process (surface meltwater reaching the glacier bed), would still make this paper a significant contribution to Antarctic glaciology.

The sharpness and regularity of the signal, spanning the entire GVIIS cavity within one or two months (Figure 4 and 5), is the first indication that this is related to summertime melt rather than ocean flow. Peaks in ice flow in December and January are timed closely with peaks in surface melting. Moreover, these timings occur sharply year after year (Figure

A1). Nearly all of the glaciers showed a significant spike in 2019-2020, a major melt year for the region. As the paper notes, the -potential- for surface water to induce glacier acceleration is well-proven. It is not essential that the water be visible on the surface as pools (see papers by Harper, Humphrey, Pfeffer; by Koenig, O. Miller, Miégè, Forster.)

On the other hand, oceanographic signals along the Antarctic coast are rarely so sharply seasonal. The cited papers do not (-can- not) discuss seasonal variations in cavity currents or changes in the depth of the CDW layer. The authors infer and favor ocean forcing, but don't discuss how it would occur – would it be related to sea ice losses? (far more variable and uncertain than the surface melt season) Or wind patterns moving the polar water layer and changing isopycnals in that fashion? (also not reliable enough to provide a signal like Flowlines 2, 3, 4, 5, 17, 18, 19, 20, and 21 in Figure A1). Note that if the change in CDW depth or flux is related to a south-to-north current, the speed required would be an order of magnitude faster than that discussed in Jenkins and Jacobs, 2008 (and it would have to be a continuous laminar flow or wave in the isopycnal).

At the very least the authors need to discuss the two possibilities as equally likely. Personally, the case for summer melt influence is far stronger in my view. However, there are data that might save the ocean discussion: instrumented seals. Data collected by instrumented seals and analyzed by, e.g., Lori Padman or Lars Boheme, might be able to show strong seasonal ocean variations. Have a look at Padman et al., 2012, JGR-Oceans – perhaps in the data used in that paper there is an indication of seasonality (but I don't think it is mentioned in the paper).

The sharp downturns in the ice velocity just before, or just after, the seasonal speed-up pulse are not easily explained in the ocean scenario.

Also – the authors missed something really cool in the data shown in Figure A1. Look carefully at the signal of Flowline 3 and Flowline 21, and their geographic position. *These glaciers are influencing each other across the ice shelf.* The earlier acceleration of Flowline 21 (Grotto Glacier, west side, Alexander Island) -slows- the outflow of Flowline 3; then Flowline 3 (Millet Glacier, east side, with a later melt-season peak, perhaps?) accelerates and forces Flowline 21 to slow down. You can see a similar but less clear influence in Flowline 2 and 4, and then Flowline 20 and 19. Like an angry uncle at Thanksgiving, one glacier is shoving the ice shelf table, turkey and all, towards the unsuspecting nephew; the nephew then makes his final point, and shoves the table back toward the uncle. (I suppose it starts with the aunt dumping her drink, meltwater, on each of their heads in succession.)

The last parts of the paper should be re-written with these considerations in mind, but the majority of the paper is publishable as is. I suggest moving some of the figures to the appendices or supplemental information, but overall this is a very well done study, that needs to revise the attribution to a wider perspective at least; if not outright favor surface melt-driven acceleration.

I would like to review the revised paper.

Also, the authors are invited to Thanksgiving at my house.

Detailed comments: Many comments are embedded in the annotated .pdf of the paper, submitted with this review.

Please also note the supplement to this comment:

<https://tc.copernicus.org/preprints/tc-2022-55/tc-2022-55-RC2-supplement.pdf>