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Interactive comment on "Seasonal dynamics of Totten Ice Shelf controlled by sea ice buttressing" *by* Chad A. Greene et al.

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Review of: Seasonal dynamics of Totten Ice Shelf controlled by sea ice Buttressing by Greene et al.

Paper Summary: In this study the authors examine intra-annual changes in the surface velocity of the Totten Ice Shelf (TIS). Velocity measurements are acquired from feature tracking of Landsat-8 (GoLIVE, 2013-2018, 12-112 day separation) and MODIS (ImGRAFT, 2003-2017, 92-182 day separation) image pairs. Fitting a sinusoid to the MODIS velocities, by means of least squares, the authors identify a 106 m/yr fluctuation in surface velocity. From the Landsat image pairs they determine an average spring to fall speedup of 0.8 m/yr. per day. Mapped differences between spring and fall

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velocities indicate that the summer speedup is concentrated towards the terminus of the ice shelf.

The authors then explore 3 likely causes for the summer speedup (surface melt, basal melt, and changes in sea ice backstress). Examining melt days determined from passive microwave data, the authors conclude the speedup precedes melt onset and therefore surface melt is unlikely to be the trigger for springtime speedup but they acknowledge that it may play a role later in the season. Through a combination of ocean modeling within the ice shelf cavity and simplified ice shelf mechanics the authors demonstrate that seasonal change in basal met rates, that have seasonal amplitudes of >8m/yr. at the grounding line and 3 m/yr. near the terminus, have little impact on rates of ice flow (several orders of magnitude below the observed signal). Lastly the authors explore changes in sea ice concentration and sea ice thickness and postulate that the breakup of fast ice in spring is the most likely trigger for the summer speedup.

Overall Opinion: The paper is well written, has a logical layout, and the analysis is transparent and easy to follow. The subject matter is appropriate for TC and will be well received by its audience. Despite the overall good quality of the manuscript I was left with a few concerns on the conclusions as drawn from the data. I see no barriers to the authors addressing these concerns in a revised manuscript.

1. My most pressing concern is the characterization of the intra-annual variability of ice shelf surface flow given the limitation in deriving surface velocities from the Landsat and MODIS images; low SNR, observations limited to polar day, and large/variable image-pair time separations. All of these conditions make it challenging to characterize intra-annual fluctuations in surface velocities. To this end I think it would be very valuable if the authors could explore the sensitivity of the least squares parameter fits to the velocity fields. For example: what is the implication of using large image-pair separations? Using bootstrapping can you better quantify the uncertainty in the fit? What does the phase and amplitude look like if you derive parameters on a pixel by pixel basis? How much do fits to the Landsat and MODIS data differ when constrained

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to the period of overlap? Is a sinusoidal fit justified by the data or should the authors solely focus on the spring to fall speedup?

2. It would be very valuable if the authors could provide uncertainties with their estimates. What is the uncertainty of the estimated annual amplitude in velocity? What is the uncertainty in the modeled melt rate and respective response in modeled ice shelf velocity? What are the uncertainties in the estimated velocities and how do these propagate into the model fits (the authors could use bootstrapping to answer this)?

3. One of the 3 environmental forcings examined as a potential trigger for spring-time speedup is surface melt. Given the very low number of days that experience any liquid water at the surface, I am suspect that there is any liquid water that does not re-freeze within the first few meters of the firn column. Can the authors provide any support that this is not the case? If not I would suggest removing this section from the paper and simply state that the vast majority of meltwater will refreeze within the firn and therefore it will not impact ice shelf flow.

4. There are a few places in the manuscript, including the introduction, Section 6.3 and the conclusions, where variability in discharge and its potential aliasing in mass change estimates are presented as the motivation for this work. I don't think this is an appropriate justification. Maybe the authors could simply us the justification that improving understating of glacier mechanics/response to intra-annual changes in boundary conditions is relevant to improving glacier models and thus future projections of sea level rise.

5. The authors clearly demonstrate that seasonal changes in ice shelf thickness on the order of 0.3 to 1 m are unimportant for seasonal fluctuations in ice shelf velocity. This is well proven through their combined ocean and ice shelf modeling. They go on to conclude that changes in sea ice thickness of the same magnitude ($\sim$1 – 1.5 m) are the cause of seasonal ice-shelf acceleration. They come to this conclusion primarily through the coincident removal of fast ice and ice shelf speedup. While I think this is

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a plausible conclusion it would be helpful for the authors to discuss the mechanisms by which sea ice is able to exert such an influence. Do the authors see seasonal fluctuations in the position of the ice shelf front that could suggest a modification in the calving rate? I would think that the backstress from 1 m of sea ice would not be sufficient in itself and instead it there would need to be some mechanism by which a small force at the front of the ice shelf could disproportionately modify the fontal stress regime

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