

In their manuscript *From Heinrich events to cyclic ice streaming, the grow-and-surge instability in the Parallel Ice Sheet model*, Johannes Feldmann and Anders Levermann nicely describe their findings from surge-type experiments with the Parallel Ice Sheet Model. In this respect, this manuscript is very similar to the Van Pelt and Oerlemans 2012 publication. There is nothing fundamentally wrong with this. Testing previously obtained results is an important process in science, and it is interesting to see that and under which parameters the current version of the Parallel Ice Sheet Model shows surge-type behavior. However, it then needs to be made very clear, where previous results were (not) confirmed and where new results are obtained. In contrast to the Author’s claim, Van Pelt and Oerlemans also used a model version that included modeled pore water as enthalpy (page 348, second paragraph). The friction laws in the two publications look very similar to me. Please point out where the “simpler friction law” was improved. The visible main differences between the publications are the investigation of a water-terminating glacier instead of a land-terminating one (this has no obvious effect on the surge cycle as drawn in Figure 2), the parameters that were varied, and slight changes in the treatment of basal water.

The manuscript left me a bit concerned regarding the appreciation of the authors for previous work, and a tendency towards over-selling their own results. This begins in the title where Heinrich events are mentioned right at the start, while the manuscript deals with cyclic surging in a pretty generic ocean-terminating glacier, that is not obviously set up to resemble the situation in the Hudson Bay / Hudson Strait area, the source region of Heinrich events. I would suggest cutting the title to *The grow-and-surge instability in the Parallel Ice Sheet model* or something similar. While continuing through the manuscript, I would have enjoyed seeing more references to previous publications on glacier surging, and the similarities and differences in the underlying theory and results. This has already substantially improved since the first version of the manuscript, but I think it can be improved further. One example is the introduction of Figure 2 in Section 3.1, where a comparison of the model presented here to the binge-purge cycles of MacAyeal 1993 (and subsequent publications, e.g., Roberts et al, 2016) could make it easier for the reader to appreciate the additions presented in this manuscript. There also is a substantial body of research on growth-and-surge cycles in the context of mountain glaciers that could be referenced.

The newly introduced comparison of different values for the basal sliding exponent q suffers from an ill-chosen reference velocity $u_c = 1 \text{ m s}^{-1}$. This is the velocity for which the basal friction τ_b is independent of the exponent q . For comparing different flow law exponents, this value has to be in the range of typical sliding velocities. Using numbers obtained from the surge phase in Figure 3, the basal shears stress for ice sliding at $u_b = 1000 \text{ m a}^{-1}$, and a τ_c value of 300 kPa varies over more than six orders of magnitude for the different values of q . It is highest for the plastic flow law with $\tau_b = 300 \text{ kPa}$ ($q=0$) and decreases via $\tau_b = 9500 \text{ Pa}$ ($q=\frac{1}{3}$) to $\tau_b = 9.5 \text{ Pa}$ ($q=1$). For lower velocities relevant for surge initiation, this contrast is even more extreme. Van Pelt and Oerlemans chose $u_c = 100 \text{ m a}^{-1}$, a much more realistic value. This might explain some of the differences in the results of the two studies.

I am not convinced by averaging the values for Figs. 3, 6-9, A1, considering that at least two thirds of the grounded domain are not affected by the surge at all. I would probably prefer one or two point measurements in the center line, or a line-average of this line.