Review of '***Seasonality in Terminus Ablation Rates for the Glaciers in Kalaallit Nunaat (Greenland)***' by Aman, KC et al., submitted for consideration to *The Cryosphere*

Summary
This manuscript combines terminus change and ice discharge time series to derive monthly terminus ablation time series for nearly 50 large outlet glaciers distributed across most regions of the Greenland ice sheet. The authors find that most of the sample exhibits coincident seasonal terminus change and ice discharge variability, with a summertime peak in ablation. On seasonal timescales, terminus change contributes to the majority of total ablation, often far exceeding the magnitude of intra-annual variations in flux gate discharge. In light of this, the study concludes that incorporating terminus change is an important component of seasonal and interannual ablation that is excluded from time series using ice discharge time series alone. The manuscript is well written and arranged in a comprehensive and logical structure, with appropriate figures that complement the main results in the text. The methodologies are appropriate for the study and the discussion/conclusions are aligned with the scope of the work presented. This manuscript is therefore nearly suitable for publication in TC in its current form, but there are several aspects of the manuscript that could benefit from additional context and/or clarity, which I detail item by item below:

Main
I think it could be worth including a brief discussion to address types of science questions that can be refined by incorporating a total ablation time series (like the one presented in this study) vs. applications where discharge-only, or similar time series, may be more appropriate. For example, for ensemble mass change studies that often compare Input-Output based methods to altimetry and GRACE, it is useful to derive changed in sea-level contributing mass fluxes. These variations in mass would precede terminus ablation (in conditions where the ablated terminus was floating or near-flotation) because that sea level volume has already been displaced. The manuscript does a good job of describing circumstances (specifically w.r.t fjord conditions and freshening) why total ablation is a refinement over ice flux alone, but does not mention that other mass change related studies may not necessarily benefit from this additional term.

Line 60, On filtering based on BedMachine source: Can the authors provide how many glaciers were excluded due to not meeting the BedMachine source criteria? My understanding was that for the majority of outlets near the margins, mass conservation was a common method for deriving bathymetry estimates (as compared to further inland where kriging is more common). Additionally, how close to the terminus do a direct radar observation hold as applicable to that glacier? For example, do direct observations need to fall within a certain length threshold to be considered robust for the downstream flux ate and terminus thickness calculations?

Line 113, Glacier speed-based filtering threshold: How was the 2x averaged speed threshold (used for filtering erroneous terminus advance observations determined? Was this an empirical threshold?

Line 150, on unaccounted mass change between terminus and ice flux: I understand that past studies have made similar assumptions given the small overall uncertainty this component would add in to the total ablation. However, for glaciers where persistent retreat occurred throughout the study period, resulting in a terminus much closer to the gate than the beginning of the time period, it could be a useful metric to provide the maximum bias this assumption could possibly impose on the final time series. While the number I likely to be small, providing bounds of uncertainty for at least several glaciers where its impact is likely to be the largest, would help support the decision to exclude mass change over this intermediate region.

Line 181, positive mass change from terminus advance: I did not follow the attribution here that negative terminus ablation was due to an underestimation of bias-induced terminus mass loss. Can the authors provide more explanation here? From my understanding, the fact that seasonal signals present in the Fourier analyses necessitate positive terminus change (or "Negative terminus ablation", i.e., advance) in addition to retreat to exhibit seasonal-scale variability. Can the authors clarify whether all instances of terminus advance are considered a result of bias in their analyses, or whether this refers to a specific treatment of terminus change with respect toa. Reference position?

Table 2: Consider adding in variance or STD in paratheses beside the mean values for discharge and ablation in each season. This would provide readers with a sense of interannual variability across the regions and how discharge amplitude and seasonality scale with total ablation.