My co-authors and I would like to thank Reviewer #2 for their input and comments on our submitted manuscript. They bring up topics and concerns that will lead to refinement and overall improvement of the manuscript.

This manuscript was originally slated for inclusion in a special PAGES2k issue focusing in climate change in the last 2000 years, hence the more specific focus on the single ice cap and production of the associated climate records. We agree that comparison of our record to other records in the Arctic would provide a broader (both spatial and temporal) context for our study. This would shift the main focus of the paper slightly, but, as suggested, a title change could pair well with that shift.

Below I address the additional points (1-7) brought up by the reviewer:

1. Movement of the modeling detail from the SI to the manuscript was also mentioned by Reviewer #1, a change that may clarify the purpose of the model, it's set up and the assumptions involved. Although it is simple model designed to capture the primary mechanisms at work, as mentioned by both reviewers, there are several assumptions that are important to lay out clearly. Again, we defer to the editor's judgement on whether to move the modeling SI section into the manuscript.
2. The Reviewer brings up two important assumptions dealing with wind redistribution of snow and precipitation over the late Holocene, both of with are important for determining the mass balance. Field observations from our sampling campaigns have suggested that wind redistribution may be a factor in glacier mass balance, however, without local, long-term wind records accounting for this is difficult. Additionally, the complexity of accurately modeling wind redistribution is highly localized and beyond the scope of this model. Furthermore, the available records of dominant wind direction cannot explain the N-S asymmetry in the LIA ice cap trimlines, suggesting that another factor (e.g., solar radiation) is the dominant control on glacier shape at this location. As for precipitation, in addition to the data from Greenland, and older study from Devon Ice Cap on Ellesmere Island also suggest that precipitation was steady over at least the past ~1300 ka (Paterson and Waddington, 1984). Without more detailed records of precipitation, I believe that assuming steady precipitation (and thus accumulation) throughout the model run is the most accurate and reasonable approach.
3. In the initial manuscript submission, the authors kept the broader comparisons (section 6.1 Regional Comparison) focused on pertinent ones in the immediate area around the study site, to maintain the focus of the manuscript. We agree with Reviewer #2 here that a more pan-Arctic comparison could provide new information on connections across the Arctic. Several other studies from Baffin and Greenland, including both glacial and lacustrine records (e.g., Briner et al., 2009; Schweinsberg et al., 2017; Thomas et al., 2010) provide different perspectives on Holocene climate and provide interesting comparison. The Reviewer is correct that work on Holocene climate and glacier records has been ongoing for several decades around the North Atlantic (nicely summarized by Solomina et al., 2015), and a concise comparison would be worthwhile.
   The mention of threshold lakes from Briner et al. (2010) (as well as CRN moraine chronologies) is used to highlight the often-discontinuous nature of glacial records compared to the plant kill age transect method used here. The transect method provides

tighter spatial and temporal constraints on the exact position of the ice margin, whereas threshold lakes may have good temporal resolution, but only provide data on when the ice in in the catchment or not (no additional information on glacier dimensions). Conversely, moraine CRN records can provide good constraint on glacier margin dimensions, but many issues plague boulder dating and due to the destructive nature successive glacier advances, the records are often discontinuous. All of these examples set the stage for the transect method used in this study, which provides more continuous records of actual glacier dimensions through time.

It is well known that individual glaciers can deviate significantly from regional climate forcings, due to localized climate effects or individual catchment features, the authors want to take care with respect to this when comparing our study. We believe comparison to broader regional trends may be more useful.

The reviewer also recommends comparison to Reusche et al. (2014), a CRN study of a late Holocene moraine on Svalbard where the authors suggest moraine abandonment at ~1.6 ka, in apparent contradiction to our chronology. There are two aspects of this study that raise concern with us. First, the reported age of 1.6±0.2 ka is the mean of 16 individual CRN boulder ages from a single moraine (with multiple crests) that range from 0.5-3.6 ka, which does not seem to be an appropriate representative of moraine activity. It is unclear if the spread in ages is due to continuous occupation of this moraine limit (with pulses of glacial advance), degradation of the moraine over time due to an ice core, or a combination of both. This moraine record is also discontinuous by nature and makes comparison to our more continuous record difficult (in additiona to the limited temporal overlap). Secondly, the proximity of Linnébreen to the Arctic ocean might suggest that it's has a strong maritime influence, including fluctuations in sea ice. Our location is also close to the ocean and would be influenced by ocean temperature and sea ice, but likely in different ways at different times. Given the concerning nature of age calculation and local climate effects, we are hesitant to compare these two records explicitly, but agree that broader comparisons to the Northern North Atlantic may be worthwhile.

4. As mentioned to Reviewer #1, upload issues created fuzzy images for Figures 1 and 2. High resolution versions will be submitted during the revision process. Does the Reviewer have any specific comments on how to improve the figures? Or what is troubling about them?
5. Per Reviewer #2's comment regarding the dashed line in figure 3, we defer to the response given to Reviewer #1. Obviously, the true trajectory of the ice margin is unconstrained between our plant kill ages, but the line represents the best representation of the episodic advances imprinted on overall cooling and ice expansion over the past ~1000 years. We would reiterate that since plant kill ages represent the time when ice likely advanced *through* that location, the inflections in the dashed line are necessary in order to fit the kill age chronology.
6. High resolution temperature records of the last ~2000 years for this region are difficult to come by, however, we agree that comparison to records from farther afield (i.e. Greenland) or from more regional compilations (e.g., Marcott et al., 2013) would improve figure 4 greatly.
7. With regards to the CESM climate simulation, we refer to the response given to Reviewer #1, that our submission deadline of Feb 11 was a hard deadline and we were not able to

finish the mode run in time. The model has since run up to 2005 CE, enabling us to use the full simulation and remove the text regarding the compositing of multiple records.

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