We would like to thank the two anonymous referees for their time and effort in reviewing and providing feedback on our manuscript.

Before addressing individual comments, we would like to make clear that the general topic of our manuscript is snowfall. While we motivate our study of Greenland snowfall by talking about the importance of the mass balance of the Greenland ice sheet, our datasets, methods, and results are all from the perspective of precipitation.

The reviewer comments are in red and our responses are in black. Responses to both reviewers are in one document.

Authors' Response to Anonymous Referee #1

1. (Referee #1 Comment - major) At the highest level, the article would benefit by being shortened, streamlined to emphasize whatever new it brings, avoid re-stating what is already well known, condense text where possible. I have pointed out some novelties I recommend get more emphasis.
(Authors' Response) Thank you for the comment. The results that you point out in your later comments (listed as xxiv,xxv,xxix under comment 6) have been highlighted in the conclusion section. In addition, the conclusion section has been condensed to focus more clearly on the novel findings of this work.

2. (Referee #1 Comment - major) The main value of the study is to "look beyond Summit station", so please 1.) streamline the Summit text 2.) add more discussion emphasis to other regions that goes beyond synoptic climatology (discussion of obvious and already documented precipitation being associated with troughs)... There has to be something about the satellite perspective taht brings much more than synoptic climatology does; for example, why not use a heavy precipitation event case to make some new points?
(Authors' Response) The emphasis of this study is looking at GrIS precipitation from a satellite. To our knowledge, there is no published work using satellite or remote sensing to look at precipitation regimes over Greenland and their connection to large scale atmospheric circulations, other than Pettersen et al., 2018 which looks only at snowfall at Summit Station. We do reference theoretical and modeling studies of GrIS snowfall (e.g. Chen at al. 1997), reanalysis studies (e.g. Schuenemann et al., 2009), and snowfall implied from ice core studies (e.g. Alley et al., 1993; Kapsner et al., 1995), but the point of this paper is to add a remote sensing perspective. Both Lenaerts et al., 2019 and Bennartz et al., 2019 looked at GrIS snowfall using the same satellites we use, however they did not connect their findings to the synoptic patterns or look at snowfall regimes.

While the satellite approach does allow us to look over the full GrIS, we argue that it is important to focus on Summit Station at the beginning of Section 4.3 in order assess the satellite snowfall retrievals and assure that they are physically realistic before moving on the larger region.

We agree with you that it is well documented and accepted that GrIS precipitation is generally associated with troughs, however, our regional analysis of IC snowfall shows that troughs have differing locations/orientations when producing snow over a given part of the ice sheet. The troughs we show align well with the theories presented in the Chen et al. 1997 paper, which lends confidence to both studies since the remote sensing perspective is independent from the modeling/theory. The ridging that we found to be associated with CLW snowfall is important to record in detail as well, since the connection between ridging and precipitation has been previously documented only in Hanna et al., 2016 (reanalysis based) and Pettersen et al., 2018 (looking only at Summit Station). We believe this is the first documentation of the synoptic patterns associated with GrIS snowfall from the remote sensing perspective, however, if the editor or reviewer have suggestions of additional studies that we should include, we are happy to do so.

Looking at an isolated heavy snowfall event would be anomalous, and we are not sure what new points would be added. If there are particular interesting results that the reviewer or editor would be interested in seeing from a case study of heavy snowfall, it could be the basis of a follow on study, however here we would like to keep the focus on the typical circulation patterns that favor GrIS snowfall. Again, the fact that our observational results agree well with previous theoretical and modeling estimates brings confidence to both perspectives.

3. (Referee Comment #1 - major) I don't entirely agree with the statement "Snowfall accumulation is the only significant, positive term in the surface mass balance of the GIS" ... surface water vapor deposition can be, as Box and Steffen (2001) abstract: "At high-elevation sites; the annual water vapor flux is positive, up to +32 ±9 mm at the North Greenland Ice core Project (NGRIP) and +6 ±2 mm at Summit." or above 25 mm near Summit which is roughly 10% of the accumulation rate (see Fig. 6 2 Level method). ... so the issue of the remote sensing technique not observing? water vapor deposition needs some treatment in this study if not just clear recognition water vapor deposition is one of the underestimates of precipitation. similarly, I am not that comfortable with "evaporation over the snowand ice-covered regions of the Arctic is negligible"... the issue of how much moisture is recycled from daytime sublimation and nighttime deposition deserves at least some mention.

(Authors' Response) We agree that at high elevation and/or low precipitation locations, water vapor deposition can be an important contribution to surface accumulation. However, our statement *"Snowfall accumulation is the only significant, positive term in the surface mass balance of the GrIS"* refered to the entire GrIS and it is supported by the references we included:

- "Precipitation is the only significant source term for the mass balance of the Greenland ice sheet and smaller ice caps in the Arctic." Jakobson and Vihma, 2010, p2175
- "The mass budget of the ice sheet as a whole is driven by precipitation at the surface of the ice sheet, which is balanced at the surface by ice melt and runoff." Mottram et al 2019, p1407

For clarity and to address vapor deposition, we changed the statement to read "Snowfall accumulation is the **largest** positive term in the surface mass balance of the GrIS" and have added the following modification to the introduction: "While vapor deposition can be locally important, snowfall is the major source term for the mass of the GrIS…" The satellite instruments that we use don't see the surface, so observing/quantifying the moisture recycling you refer to would be outside the capabilities of our technique.

4. (Referee #1 Comment – major) Move discussion text (for example page 17 lines 3-22) out of conclusions section. In conclusion section, limit text to what new this study finds, little more please. How to accomplish is to make a list of new insights in this section, simple as that!
(Authors' Response) Thank you for the suggestion. We have modified the conclusion to focus on the new insights from our work, including the additional results you suggested that we highlight. We have removed the majority of the discussion text out of this section, leaving only three sentences at the end to provide context to the readers on the broader importance of our results.

5. (Referee #1 Comment) By the way, I think you probably agree, it would be helpful to have all the fancy equipment lower on the ice sheet, for example, near DYE-2 and much closer to the airport! You're welcome to pass my comment to your science foundation.
(Authors' Response) The instrument suite at Summit Station is funded by the US National Science Foundation and will run through 2020. We are unsure of the future location of the instruments, but will pass along your suggestion.

6. (Referee #1 Comment – minor revisions) about my comments... io means instead of comments, if two numbers, first number is page number, second is line number

i. *throughout consider "study" io "paper"*
(Authors' Response) We have made this change.

ii. *page 1 abstract "due to increasing surface melt" io "due to surface melt"*
(Authors' Response) We have made this change.

iii. *After it is soon obvious the region is the Greenland ice sheet, use "ice sheet" io "GIS"*
(Authors' Response) We have updated GIS to GrIS throughout the paper. GrIS avoids the confusion of the previous acronym and is becoming the standard among the atmospheric science community.

iv. *11-12 "Overall, most CPR observations of snowfall over the GIS come from IC events (70 %), however, during the summer months, close to half of the snow observed is produced in CLW events (45 %)." ... really depends where and as the next sentence puts it, when, i.e. "summer", so what is the point?*
(Authors' Response) To our knowledge, this is the first study to partition GrIS snowfall events based on cloud phase, so these numbers are new to the community. We go into more detail in the analysis section regarding the importance of these numbers, the main

idea being that while CLW events only make up 30% of the total GrIS snowfall observations, they make up nearly half (45%) in summer and thus are important for increasing surface brightness during the months where solar radiation is present.

v. *17 "growth" of ___? be clearer*
(Authors' Response) Since "growth" covers multiple processes that would clutter the abstract (but are detailed in Section 3, page 8, lines 13-17), we have simplified the introduction to say "IC events demonstrate consistently increasing reflectivity toward the surface" rather than "growth toward the surface".

vi. *18 how is "large scale anomalous high pressure" different from "large scale high pressure"? The latter is less ambiguous IMO*
(Authors' Response) Anomalous high pressure indicates a deviation from the mean in a given region, rather than the actual pressure level. To reduce confusion, we have updated the text to say: "…CLW events generally occur under large scale anomalously high geopotential heights over the GrIS."

vii. *18 "Ground-based data" be more specific; location, sensor*
(Authors' Response) We have updated the abstract text to say "Ground-based data from an instrument suite at Summit Station is used to estimate…"

viii. *22 "key role in both the global energy budget (e.g. Box et al., 2012)" that study is not exemplary of global energy budget and "key" here is vague, pls rephrase*
(Authors' Response) Thank you for the comment. We have updated that sentence for clarity and replaced the Box et al., 2012 study with Flanner et al., 2011: "The Greenland Ice Sheet (GrIS) is important globally because of its influence on both the energy budget (e.g. Flanner et al., 2011) and water cycle (e.g. Church et al., 2001, Enderlin et al., 2014)."

ix. *24 "year-round" actually not in winter or at night*
(Authors' Response) We have removed "year-round".

x. *page 2 Bamber et al 2013 have a more accurate number than "7.2 m (Church et al., 2001)"*
(Authors' Response) Thank you for pointing out this reference, we have updated the number and citation accordingly.

xi. *7 "Recent mass loss" add time interval(s)*
(Authors' Response) We have added the time interval specific to the referenced study: "Between 1972 and 2018, the GrIS contributed 13.7 mm to global sea level rise…"

xii. *"shortwave" io "SW"*
(Authors' Response) We have removed the abbreviation SW.

xiii. *"summer" is not the right word, non-summer months can matter. More meaningful can be to write of "sunlit periods" or "period of positive net radiation"*
(Authors' Response) Thank you for the suggestion, we have updated the text to say "sunlit periods".

xiv. *21 "surface height" worth having a look at PROMICE.org data and associated publications*
(Authors' Response) Thank you for the suggestion, we have looked at the PROMICE website/publications and included a sentence on ground penetrating radar accumulation estimates in the introduction: "Both airborne and ground-based radars have looked below the surface of the GrIS to provide historical accumulation values (Miege et al., 2013; Lewis et al., 2017), but are limited by the specific location of the transects, complications from melt events, and accumulation estimates are for annual or longer periods."

xv. *24-25 "wide range of GIS snowfall estimates" see work of Lewis in Cryosphere, a paper from 2018? and another now in review 2019 TCD*
(Authors' Response) In looking at the two Lewis papers in the Cryosphere ("Regional Greenland accumulation variability from Operation IceBridge airborne accumulation radar," 2017; and "Recent precipitation decrease across the western Greenland ice sheet percolation zone," 2019), they provide regional estimates using airborne and ground-based radars and ice cores. While these are interesting studies, neither provides an estimate for the total annual GrIS snowfall accumulation, which is what we are referring to in this statement. To clarify, we have updated the sentence to say "wide range of estimates for total GrIS snowfall"

xvi. *26 "ground-based snow observations" io "ground-based observations"*
(Authors' Response) We have made this change.

xvii. *26 "can be useful to examine" io "are needed to look at"*
(Authors' Response) Modified to say "satellites are useful tools for looking at…".

xviii. *27 "from space"... "remotely" o*
(Authors' Response) In this case, the method/reference we mention is space-based, and it would not be appropriate to say "remotely", which could include many possible platforms (ship, aircraft, ground-based, etc.).

xix. *31 "an attractive" io "currently the best "*
(Authors' Response) We have modified the sentence to remove "currently the best", it now reads: "Satellite-borne active sensors are an advantageous platform for measuring the annual cycle of snowfall over the full GrIS because they can provide both information on falling snow as well as insight into the coincident clouds."

xx. *6 19 "explore the contents of clouds" rephrase to be more explicit re: "contents" "examine" may be better than "explore"]*
(Authors' Response) We have modified this paragraph to be more explicit, and in the process, have removed an additional acronym (IWP) as it was no longer needed. It now reads: "In this work we use column-integrated reflectivity (Z_{path} , mm^6 m^{-2}) as a proxy for the ice mass characteristics of the cloud. Z_{path} is a relatively simple measurement related to the amount of hydrometeor backscatter (defined as Z_{int} in Kulie et al., 2010; Pettersen et al., 2016).

xxi. *8 15 and throughout the paper, I suggest replacing "look" with "examine" ..."examined" io "looked specifically at"*
(Authors' Response) We have made this change.

xxii. *10 30 remove "(~1.2 million)" un-needed and arbitrary*
(Authors' Response) We have made this change.

xxiii. *9 16 "The frequency of detected snowfall events" io "The frequency of snowfall" 9*
(Authors' Response) We have made this change.

xxiv. *22-23 "more CLW cases along the western side of the GIS than the eastern" a point may be worth emphasizing as a new result this study brings forward. The previous information in the paragraph is already common knowledge in the field*
(Authors' Response) Thank you for the comment, we have included this as a point in the conclusions.

xxv. *9 25 winter "stronger north-south gradient compared to the annual distribution" also deserving some highlight, perhaps buried here, put it in conclusions if not also in abstract*
(Authors' Response) We have included this as a point in the conclusions.

xxvi. *10 5 "~83 % of its annual incoming solar insolation." according to what info?*
(Authors' Response) This was calculated from the 2B-FLXHR-lidar data product. We have added this to the text and included the relevant citation.

xxvii. *10 8 not in full agreement with "Snowfall accumulation is the only significant, positive term in the surface mass balance of the GIS" see major comments*
(Authors' Response) Addressed in major comments.

xxviii. *10 34 "hydrometeor" mean rain? be clear or use less jargon*
(Authors' Response) In this instance we were referring to ice particles, so have replaced "hydrometeor formation and/or growth" with "ice particle formation and/or growth".

xxix. *11 1-2 "while the common summer events are snowing at slightly higher rates on average, it is in winter that the less frequent, highest-intensity snowfall occurs." interesting point worth emphasizing in conclusions*

(Authors' Response) We have included this as a point in the conclusions.

xxx. *11 13 "determine" io "see", modify throughout*
(Authors' Response) We have made this change.

xxxi. *13 31-32 "Excluding a minor moisture recycling in the surface boundary layer that delivers frost during net surface radiative cooling events, the moisture required to produce GIS snowfall is not produced locally" io "The moisture required to produce GIS snowfall is not produced locally" ... actually the first sentence in 4.3 can be removed, the following sentence makes the point and there you might as well mention temperature inversion and PBD moisture recycling*
(Authors' Response) We have done as you suggest and removed the first sentence of Section 4.3, and modified the following sentence's beginning to smooth the new transition: "While many local factors influence when and where snowfall occurs over the GrIS (topography, surface type, temperature, etc.)…"

Authors' Response to Anonymous Referee #2

1. (Referee #2 Comment – minor revisions) There are only a couple of very minor comments that the authors could address

i. *Page 3 Line 14: "been" is repeated.*
(Authors' Response) We have made this change.

ii. *Page 12 Line 15: "have" is repeated.*
(Authors' Response) We have made this change.

iii. *Page 12 Line 30-35: you are talking here about opposite skewness of the distribution between winter and summer, but I don't actually see a negative skew in summer (with the peak to the right and the tail to the left). I would actually still see a tail to the right for summer even if steeper. I'll leave to the authors if considering this very minor comment or not.*
(Authors' Response) We see what you mean. The difference we were trying to emphasize was the movement of the peak in the distribution (from between 2-4 km in winter to between 3-6 km in summer) while the range of the distribution remained much the same. In light or your comment, we changed the text of page 12 line 30-32 to read: "There is also a change in the skewness of the distribution, with a positive skew (peak to the left, tail to the right) in the winter and a little to no skew (peak centered in the range of measurements) in summer."

iv. *Page 13 Line 1: reading "a distinct nature of the two regimes" makes me think that I should be able to separate the two regimes from the histograms in fig. 9 and this actually not the case since the two distributions are quite totally overlapping. I then understand that these plots tell the story of the IWP behavior for the two regimes, but I would try to describe it better not to lead the reader to a wrong conclusion (being able to*

distinguish between the regimes thanks to the dBZpath).

(Authors' Response) Thank you for the comment. You are correct that the annual and winter distributions of dB(Zpath) are largely overlapping for the two regimes. The 'distinct nature' that we were referring to was in the peak locations and the seasonal behavior – in summer (Fig. 9c) CLW maintains the same shape as winter, while the IC events lean to larger values. We have changed the line beginning on page 13 line 1 to be clearer: "The histograms of dB(Zpath) (Fig. 9) highlight distinct seasonal behavior for the two regimes."