We thank the reviewer for the feedback given. Please see below for a point-to-point response.

albedo can be only used, scientifically speaking, as a proxy of melt extent and melt amount. Not as proxy of ice discharge (as shown by the authors) and not as proxy of summer SMB (also in part driven by summer snowfall anomalies, as also mentioned by the authors). Due to the delay between production of meltwater (highly correlated with albedo I agree) and runoff (depending of the snowpack meltwater retention), runoff not be correlated with albedo at the monthly time scale. Using MAR outputs, at the summer time scale, it is true that MJJA SMB is correlated at -0.96 (-0.88 over 1980-1999) with runoff and 0.66 with snowfall over 1980-2015. But in May for example, SMB is correlated at 0.98 with snowfall and -0.13 with runoff, showing well that albedo cannot be used as proxy of monthly SMB, althrough MAR is not "the true".

The reviewer is correct in that snowfall impacts MAR SMB variably, and substantially during the early summer (May). However, MAR model results equally suggest that the impact of May SMB on the overall summer-summed (MJJA) SMB is small, as the mean ice sheet SMB in May is on the order of ~5 Gt, in comparison to the -50…-200 Gt seen in the MJJA sums. Thus, we maintain that while the use of the proxy method to attempt to estimate any single month's SMB is indeed an uncertain venture and cannot be recommended, the proxy method can be useful in its original intended use: to test, on observational basis, whether or not runoff is indeed the dominant driver of the whole summer's summed SMB variability.

In consideration of the reviewer's feedback, we propose to revise the manuscript text in sections 1, 2.7.4., and 3.3 to further clarify and emphasize the intended use of the proxy determination as a test case for runoff dominance at the whole summer scale, to caution the reader against using the proxy to attempt inversion of ice sheet SMB for any single summer month, and to relegate the regression equations to supplementary material to further discourage their careless use.

The albedo data was not used as a proxy for ice discharge in any part of the manuscript. Temporal lag correlation analysis was performed for independent (seasonal) estimates of ice discharge and albedo, as a proxy for meltwater production. However, with regard to the comments here and from the other reviewer, we have removed the time-lag analysis with its associated result figure from the manuscript.

- the role of the albedo decrease and the bare ice expansion to the recent melt increase has already been shown a lot of time in previous publications (e.g. Box et al., 2013).

The reviewer appears to refer to this publication: Greenland ice sheet albedo feedback: thermodynamics and atmospheric drivers, Box et al., 2012. These results, and nearly all others present in literature, are obtained from various versions of the MODIS data record and thus covering 2000->. Recent recalibrations of the MODIS instruments have resulted in a significant update of the referred-to findings (Casey et al., 2017), which the CLARA record is now quite consistent with. Also, as mentioned, this is the first study to leverage the full 1982-2015 coverage in CLARA-A2 albedo, based on intercalibrated AVHRR observations whose stability we demonstrate in the manuscript.

The results also clearly indicate that the significance of albedo trends along the GrIS margins is affected by the length of the observational record being investigated; using CLARA allows us to see the relatively stable 1980s-mid-1990s period, which place the subsequent changes during the MODIS era in larger

temporal context. The manuscript is not a simple replication of past investigations, but rather seeks to extend the viewpoint and contribute to the ongoing discussion.

- the correlation between GBI and melt (approximated with albedo here) is also some-thing which is known from a long time (see the Hanna et al.,....).

The connection between albedo changes and GBI was presented as part of the discussion and not results precisely because it is intended as supporting evidence for what is revealed in the full CLARA period, also for the pre-MODIS era. The discussion around this point has been revised and expanded to provide further viewpoints into the consistency of the albedo decrease with changes in atmospheric circulation.

- the discussion about the cloud cover seem to be out of the purpose of this paper...and using MAR for this is certainly not a robust basis of validation.

MAR cloud cover is not intended as a validation basis for the satellite record, the comparison is presented to highlight the differences between observations and model, and in the context of commentary regarding other recent progress in the field. Cloud cover and cloud optical properties are highly relevant to surface melt and thus albedo changes, the authors do not quite see why their inclusion into the discussion section would be out of purpose.

To conclude, the authors try to correlate their "new" data with several previous stud-ies/estimates but there is no new interesting scientific message in this paper deserving to accept this paper in TC. However, using this new satellite product deserves to be published, but I recommend to the authors to limit their correlations/comparisons to melt extent (from satellite) or modelled melt amount (as model validation data set). A comparison with the albedo/bare ice extent MODIS based product (notably used asbare ice albedo in RACMO/HIRHAM) will also be more interesting.

The authors disagree with the reviewer on the lack of novelty value in the manuscript. We show that:

1) albedo changes are significantly negative along many parts of the ice sheet margins, are consistent with MODIS for the overlap, display substantial regional variability in the pre-MODIS era, and are consistent with a variety of atmospheric circulation, air temperature, and cloudiness drivers,
2) bare ice extent has most likely expanded and reaches further up the ice sheet,
3) snowfall and atmospheric regime anomalies are potentially strong inhibitors of enhanced surface melt,
4) the year-to-year albedo changes and discharge changes are not strongly connected, supporting recent modeling findings.

Finally, the runoff-dominance test for summer GrIS SMB is the final new piece of information offered.

The reviewer's suggestion to focus on melt extent/amount determination is naturally worthwhile as well, but publications in said fields do already exist for the more commonly used microwave observations (Mernild et al, 2011; Välisuo et al, 2018). Extension to the optical domain is certainly possible, but requiring and deserving a separate study in the authors' view.