

Editor Decision: Publish subject to minor revisions (review by editor) (30 Aug 2019) by [Mark Flanner](#)

Comments to the Author:

Dear Dr. Riihelä and co-authors -

As you will see from the new referee reports, both reviewers find your revised manuscript substantially improved over the earlier version. At this point, only minor revisions are needed prior to publication. In particular, please:

- take Reviewer #2's advice to remove "ice discharge" from the title, section 3.4 and conclusions, as ice discharge is no longer a focus of the paper.

Title revised; regarding section 3.4., we propose to keep the present short description and results, as they are believed to be useful for the discharge study community, even though the result in question is negative in nature.

- consider reviewer #2's proposal to eliminate section 3.4 (along with comments from Reviewer #1), and make your own decision based on what you think is best for the paper.

Please see above for our reasoning to keep section 3.4., and the associated short mention in the conclusions.

- address Reviewer #1's question about Stroeve [2001, JGR].

Addressed through addition in discussion section, please see the point below for details.

- satisfy Reviewer #1's request to include some discussion of Tedesco et al [2016]

Addressed through addition in discussion section, please see the point below for details.

- consider Reviewer #1's advice for improving the paper by "further emphasizing the main points of the article, perhaps at the expense of un-necessary text"

Restructured discussion and rewrote the final paragraph of conclusions to highlight main findings of study.

- consider Reviewer #1's comments pertaining to specific lines in the text

Each comment taken into account; see below for details.

Thanks, and I look forward to seeing your revised manuscript. -

Mark

[Referee #1](#)

For final publication, the manuscript should be

accepted subject to **minor revisions**

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

Some of the litigious results from Sect 3.4 have been removed in this revised version as well as some clear warnings have been added in Sect 2.7.4.

I'm happy now with this version that I suggest to accept for publication after some minor corrections:

- As the main interest of this paper is the presentation of a new satellite data set, I suggest to add at the end of the title "using the new CLARA satellite-based product"
- I suggest also to remove "and ice discharge" in the title as it is no more presented in the manuscript.

Title revised, "and ice discharge" removed and added "using the CLARA-A2 dataset". As CLARA-A2 is already 2 years old with several existing use cases, though, we prefer not to use the word "new".

- I'm not sure about the usefulness of keeping Sect 3.4 as most of analysis has been removed. Idem for the sentences about this in the conclusion.

We prefer keeping section 3.4 and a note in conclusions; a negative result is a result nonetheless, though not in the paper's main focus, and the bit of knowledge gained in this short analysis is still believed to be of use to the discharge modeling community.

Referee #2 (J. Box)

For final publication, the manuscript should be

reconsidered after **major revisions**

I am **not** willing to review the revised paper.

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

Overall synopsis

Another revision further emphasizing the main points of the article, perhaps at the expense of unnecessary text, such as the very last paragraph, would increase the readability of the article.

In the first review, I questioned “Stroeve [2001, JGR] found NAO resonance, as one would expect. What about this study?”... I think that useful question remains unanswered. The summer NAO remains a likely strong predictor of albedo variability. Given the differences in albedo data employed here (black sky) vs blue sky in Stroeve (2001) and thus this topic should be more clearly addressed.

We further analyzed the SAL data against the NAO index (Hurrell et al., 2009) and found a generally similar relationship as did Stroeve (2001); a positive correlation ($r=0.47$) between NAO and ice-sheet averaged surface albedo. Our correlation was weaker than Stroeve’s over the overlap period 1982-1998 ($r=0.29$), reflecting perhaps the impact of variability in cloud cover in the APP-x blue-sky albedo used in the earlier study. The relationship between GBI and CLARA albedo is stronger, in line with the reasoning in Rajewicz and Marshall (2014). The appropriate paragraph in the discussion section has been expanded to reflect this consideration.

Hurrell, J. W., and C. Deser, 2009: North Atlantic climate variability: The role of the North Atlantic Oscillation. *J. Mar. Syst.*, 78, No. 1, 28-41

Sorry I didn’t pick this up earlier but, some solid justification for excluding September from the analysis is justified. The month of May is included. Why not September? The melt season extends into September for at least the southern half of the ice sheet.

CLARA coverage over the ice sheet is more uncertain and only partial during September, because large Sun zenith angles (>70 deg.) prevent reliable estimate calculation. We thus focused only on the months with full coverage. Issue noted in section 2.1.

Sorry I didn’t pick this up earlier but, some discussion of the following article is warranted because they also use an AVHRR Greenland product.

Tedesco, M., Doherty, S., Fettweis, X., Alexander, P., Jeyaratnam, J., and Stroeve, J.: The darkening of the Greenland ice sheet: trends, drivers, and projections (1981–2100), *The Cryosphere*, 10, 477-496, <https://doi.org/10.5194/tc-10-477-2016>, 2016.

A new paragraph has been added to the discussion section analyzing our findings in relation to Tedesco et al. (2016). The principal results are consistent between GLASS and CLARA, although the role of older impurity exposure is likely less dominant than atmospheric circulation and cloudiness changes, as seen by the effects of the 2013-2015 melt seasons.

Page 17 line 3 “outermost” relative to what?

Removed.

Page 17 line 7, Conclusions ... reference is made to “northeastern and eastern margins” then sentence two focuses on western ice sheet driver of albedo variability. So, the sentence could be split to be more clear. Then if no proposed candidate drivers for “northeastern and eastern margins” , state so.

Sentence split and clarified.

Page 17 line 10, Conclusions ...”the western and northern ice sheet margins intensified after 2000”... 1998 (and 1995) were warm summers too... I guess MERRA-2 data time series would show that? See Fettweis et al 2007 GRL Fig 2.

Added “primarily” to indirectly acknowledge the 95/98 warm summers.

17 14 “where we propose...” is not a conclusion, is a hypothesis or otherwise speculative, suggest remove from Conclusions and place in discussion.

Done.

17 18 remove the un-needed “state of the art” and “latest” list MAR version number.

Done.

17 24 remove “This...” is speculative and not a conclusion.

Done.

17 28 explain in quantitative terms what is meant rather than “duress” which has a definition of “noun...threats, violence, constraints, or other action brought to bear on someone to do something against their will or better judgment.

See below.

17 30 “precipitation intensity and phase are most likely the strongest drivers of change manifested through...” speculative, remove and stick to firm conclusions of the study. ... This whole paragraph seems un-necessary... Rather, I suggest focusing on the main points of the study which are extending the albedo record and what is the value of the AVHRR record, what does it tell us with that many more years of data?... how reliable is the AVHRR period before 2000? Is it possible to make conclusions on how reliable is the AVHRR period before 2000 or does the multi-satellite, vicarious calibration prevent that?

Last paragraph rewritten to focus more on the added value of the record and the consistency of results obtained.