Reviewer #3

Review of tc-2020-31

This manuscript examines the current state of precipitation around the Greenland Ice Sheet (GrIS) using precipitation products from CloudSat (CS). It partitions the frequency of the snowfall into regions of the GrIS as well as looks at impacts based on elevation. The manuscript looks at both snow (moderate and light) and rain (light only) and compares to both CESM findings of current day and future projections. In general, CESM overestimates the rainfall frequency, but reproduces the spatial and seasonal variability when compared to CS. Under future warming conditions, the authors find that rainfall will increase at higher elevations of the GrIS, whereas snow only increases in the highest elevations (>2500 MASL).

Overall, this is a well-written and organized manuscript. I believe that the material is novel and will add to our understanding of future impacts of precipitation to the GrIS. I have only some minor comments and suggestions of added references in some areas where I think they would help broaden or support the manuscript. These are listed below:

We thank the reviewer for their positive assessment. We provide a point-by-point response below.

* The introduction could benefit with a little more background and citations (especially the first three paragraphs). For example, please cite: . . .”equivalent to 7.3 meter sea 15 level equivalent.” (P1, L15), . . .”driven by a progressively declining SMB.” (P1, L20). Also, could you add any comments on recent data from GRACE or IceSat2/IceBridge in constraining some of these measures of SMB somewhere in the Intro?

Thanks for this suggestion. We have added several new references to the intro.

Morlighem et al., 2017 - to support the 7.4 m sea level rise equivalent claim.

Shepherd et al., 2019: to support ‘progressively declining SMB’

Montgomery et al., 2020: Constraining SMB using IceBridge in SW Greenland

Fettweis et al., 2020: model intercomparison of Greenland SMB

Morlighem, M., Williams, C. N., Rignot, E., An, L., Arndt, J. E., Bamber, J. L., et al. (2017). BedMachine v3: Complete Bed Topography and Ocean Bathymetry Mapping of Greenland From Multibeam Echo Sounding Combined With Mass Conservation.

Geophysical Research Letters, *44*(21), 11,051-11,061. https://doi.org/10.1002/2017GL074954

Montgomery, L., Koenig, L., Lenaerts, J. T. M., & Kuipers Munneke, P. (2020). Accumulation rates (2009-2017) in Southeast Greenland derived from airborne snow radar and comparison with regional climate models. *Annals of Glaciology*, 1–9. https://doi.org/10.1017/aog.2020.8

Fettweis, X., Hofer, S., Krebs-Kanzow, U., Amory, C., Aoki, T., Berends, C. J., et al. (2020). GrSMBMIP: Intercomparison of the modelled 1980--2012 surface mass balance over the Greenland Ice sheet. *The Cryosphere Discussions*, *2020*, 1–35. https://doi.org/10.5194/tc-2019-321

Shepherd, A., Ivins, E., Rignot, E., Smith, B., van den Broeke, M., Velicogna, I., et al. (2020). Mass balance of the Greenland Ice Sheet from 1992 to 2018. *Nature*, *579*(7798), 233–239. https://doi.org/10.1038/s41586-019-1855-2

* Could you please add McIlhattan et al. (2019 TCD – in revisions) as well at “and GrIS precipitation rates (Bennartz et al., 2019)” (P2, L31)? McIlhattan et al. also ex- amines the frequency and rates of snowfall over the GrIS (https://www.the-cryosphere-discuss.net/tc-2019-223/tc-2019-223.pdf)

Done.

* This comment relates to what you say on P2, L33: “In particular, CloudSat radar reflectivity profiles are contaminated by ground clutter in the bottom kilometer of the atmosphere. . .” Both Bennartz et al. (2019) and McIlhattan et al. (2019) examine the impact of the ground clutter and the accuracy of the lowest available bin on snow rate information. McIlhattan et al. found that up to 25% of the light snow-producing mixed-phase clouds are likely being missed by CS, when compared to studies at Summit Station (Pettersen et al., 2018 (ACP)). It might be good to have some discussion of this in the data and methods section. I do not think that it fits in the introduction and I do not think it will detract from the overall narrative, but I think some discussion or inclusion of the ground clutter/detection issues in the Data and Methods section would be helpful. It might also be helpful to show the definitions of “light” versus “regular” snow and rain in the methods (I did find it in Kay et al., 2018, but it would be nice to include here as well).

Thanks for pointing that out. We added to the Data and methods, after the first sentence “In addition, CloudSat suffers from ground clutter, which leads to, for example, missing up to 25% of the light snow producing mixed-phase clouds over central Greenland (Bennartz et al., 2019; McIlhattan et al., 2019)."

We also added the thresholds to the text in the Data and methods.

* P4, L9-11 I would add some citations of previous precipitation studies that agree with these findings. For example, "to >30% over Southeast Greenland" is con- sistent with previous studies, such as: Schuenemann et al., 2009; Hakuba et al., 2012; Berdahl et al., 2018. And "The interior experiences snowfall most fre- quently in the summer (JJA, >20%)," is in line with ground-based studies from Summit Station, so I suggest noting that and adding the citations: Castellani et al., 2015 (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2015JD023072) and Pettersen et al., 2018 (https://www.atmos-chem-phys.net/18/4715/2018/acp-18-4715- 2018.pdf). Throughout this paragraph, it would be helpful to note previous work that is consistent with these findings (similar with the rain frequencies).

Thanks once again for providing these references, these are useful to put our results into a perspective. To clearly separate our own results and the discussion, we have added a brief discussion on the comparison/agreement with existing studies to the discussion (first paragraph), adding the references suggested by the reviewer: "Our CloudSat results align well with previous studies. The snowfall frequency maximum of >30% over Southeast Greenland is consistent with various modeling results (Schuenemann et al., 2009; Hakuba et al., 2012; Berdahl et al., 2018). The summer maximum in snowfall in the GrIS interior is confirmed by ground observations at Summit station (Castellani et al., 2015; Pettersen et al, 2018)."

* Figure 3 caption implies there should be dashed lines, but they are not shown. It does say "not shown" in the text (P5, L7). I think it would be nice to show these. Also, this is in agreement with what McIllhattan et al. found (see figure 7).

We revised Figure 3 accordingly.

* P6, L1: You say "In contrast, interior GrIS summer snowfall frequency is slightly lower in CESM than in CloudSat." Both Pettersen et al. (2018) and McIlhattan et al. found that mixed-phase clouds were the dominate cloud type producing snowfall in the summer (as opposed to deep, frontal clouds). CS misses many of these lightly precipitating mixed-phase clouds (especially over the interior where CS was compared to Summit Station instrumentation). Is it worth noting this point? Either here or in the discussion? It could be that CS is missing some of this summertime precipitation that is actually being modeling correctly?

That is a fair point, but if we understand correctly, this would actually aggravate the CESM bias, since the model produces even less light snowfall than CloudSat (which - as the reviewer indicates - misses a fraction of these events). Since we already mentioned that CloudSat potentially fails to detect such events in the Data and methods section, we would argue that this topic has been addressed sufficiently.

* Figure 6: just a comment that not only does the heavier snow seem to have less of a seasonal cycle, it seems to be completely missing the uptick in SON that is due to the firing up of the NA storm track. Just a comment – but does CESM not accurately capture the NA storm tracks impinging the GrIS?

We are not aware of a study that analyzes the CESM1 storm track seasonality in/around the North Atlantic region, but this is potentially the case. Since this topic is out of the scope of our study, and would require substantial additional analysis, we prefer to refrain from mentioning it.

* P8, L3-4: "A part of these discrepancies between CESM and CloudSat may be ascribed to CESM (at its horizontal resolution of 1 degree) not resolving the steep topog- raphy and related surface climate and precipitation gradients of the marginal GrIS" – also, Bennartz et al., (2019) showed that CS additionally has a very difficult time re- solving the precipitation accurately in the steep topographic regions (as well as other studies focused on CS and GPM orographic impacts). Could it also be that both CESM and CS have difficulties here? Might be worth noting – I am not sure I would say it is all CESM.

Good point. We added to the discussion: "The differences between CESM and CloudSat are, at least partly, ascribed by the limited horizontal resolution (around 1 degree) of both products. Here we show that topography smoothing in CESM leads to underestimated precipitation frequency along the GrIS edges, but we should note that CloudSat also struggles to accurately represent precipitation in steep topographic regions (Bennartz et al., 2019)."

* P9, L9 -10: "The increase in GrIS interior snow frequency is consistent throughout all seasons, and most prominent in winter (DJF)" – any speculation as to why? Is it temperature driven, moisture? (either here or in the discussion).

We added in the discussion (second paragraph): "The strongest increase in snow frequency occurs in winter, which is the season with the strongest simulated temperature increase in CESM (Peings et al., 2017). Snowfall and temperature are strongly correlated at low temperatures, since the Clausius-Clapeyron relationship dictates that the atmospheric saturation vapor pressure exponentially increases with temperature."

Peings, Y., Cattiaux, J., Vavrus, S., & Magnusdottir, G. (2017). Late Twenty-First-Century Changes in the Midlatitude Atmospheric Circulation in the CESM Large Ensemble. *Journal of Climate*, *30*(15), 5943–5960. https://doi.org/10.1175/JCLI-D-16-0340.1

* Figure A1 is not really in an appendix. Is it worth just adding it as a regular figure? Or adding a proper Appendix with some verbiage?

We have added this figure as a regular figure.

* Final comment – Much of the above comments/citations could be added either where I noted or in the discussion. I think adding some of the above gives the paper more context.