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The Cryosphere Discussions

Interactive comment on "Trends and spatial variation in rain-on-snow events over the Arctic Ocean during the early melt season" *by* Tingfeng Dou et al.

Tingfeng Dou et al.

doutf@ucas.ac.cn

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Response letter Dear Editor, We have studied the valuable comments from yourself and the reviewers carefully, and made further revisions in the manuscript that address your and the reviewers' concerns. Our detailed response to the reviewers' comments follows below. - Response to Reviewer #1's comments: This study uses several reanalyses to study the trends and spatial variation of rainfall on snow-covered sea ice in March-June over the Arctic Ocean and how these events relate to early melt onset. They also compare reanalysis results to observations at several coastal weather stations to assess the validity of reanalysis output. The key points, as I interpret them, are:

Rain-on-snow events are often a trigger for early melt onset on sea ice. Rain events on snow-covered sea ice are occurring earlier in the season. This is partly because of a shift from solid to liquid precip phase. The shift in rainfall-to-precipitation ratio is a measurable cause of declining snow depth on sea ice (-0.5 cm/decade averaged over Arctic Ocean). The ERA products (ERAInterim and ERA5) are generally better matches with the observations, and ERA5 is the most consistent for rainfall-precipitation ratios. I have several comments about the structure of the text and points of clarification, but most of the writing is already clear. The figures are clear and illustrate the points well. There is one aspect of the methods that I think needs to be better described, but assuming I've interpreted it correctly, the methods seem sensible and appropriate. I think this will be an excellent fit for The Cryosphere journal after some revisions to improve the clarity, the discussion, and the textual organization. Section 1 Comments Line 38: I believe the Rennert piece cited here was actually published in 2009 – the May 1 issue of Journal of Climate. I think this section has a good cross-section of papers on ROS, precipitation phase, and sea ice. However, I think it is worth including some more literature on sea ice melt onset. Much of this literature appears in the results section already. There are other good papers to cite for the impact of warm, moist air transport leading to melt onset for sea ice, such as Kapsch et al. (2016) and Liu & Schweiger (2017). Response: Thank you for your reminder. Yes, the Rennert piece cited here was published in 2009, and we have corrected this reference in the revised MS. According to your suggestion, several additional references are added in the discussion of the impact of warm, moist air transport on sea ice melt onset. Please see details at L225 in the revised MS with traces of changes. Kapsch M. L, Graversen RG, Tjernström M, Bintanja R (2016). The effect of downwelling longwave and shortwave radiation on Arctic summer sea ice. Journal of Climate, 29,1143–1159. https://doi.org/10.1175/JCLI-D-15-0238.1. Liu, Z. , and Schweiger, A. (2017). Synoptic conditions, clouds, and sea ice melt onset in the beaufort and chukchi seasonal ice zone. Journal of Climate, 30(17), 6999-7016. Persson, P. O. G. (2012), Onset and end of the summer melt season over sea ice: Thermal structure and surface energy perspective from SHEBA, Clim. Dyn., 39(6), 1349– 1371,

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Discussion paper

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Section 2 Comments Line 77-78: There is assimilation of land-surface precipitation in MERRA-2, although is tapers off to 0 weighting at 62.5 N (Reichle et al., 2017). Therefore, it would be more accurate to specify that for the location of interest in the Arctic Ocean there is no direct assimilation of precipitation data. Response: Thank you for your suggestion. We have revised the statement here (L102). "There is no direct assimilation of precipitation data in the Arctic Ocean (Dee et al., 2011; Reichle et al., 2017)."

Line 83-85: I am not convinced that 0.5 mm/day is sufficient to remove all spurious precipitation events for MERRA-2. Excessive precipitation is a worse problem in MERRA-2 than ERA-Interim or JRA-55 (Boisvert et al., 2018; Figure 10). For ROS detection at an hourly scale, Crawford et al. (2020) used 0.1 in./event (2.54 mm/event) as a threshold with MERRA-2 (Figure 2). I know Bieniek et al. (2018) used 0.254 mm/day, but they were also using ERA-Interim, not MERRA-2. I'm not sure what the impact changing the threshold would have on results, but the fact that MERRA-2 has relatively poor matches with observations makes me think a higher threshold would help make them align better. That is what Crawford et al. (2020) concluded, and it might work here. Given that a) the authors have other reanalyses to work with and b) I don't think other additional analysis is necessary, my advice is to acknowledge this possibility in the text rather than re-do the analysis with a higher threshold. Response: Thank you for your comment. We rechecked the data used in this work, and found that we were actually looking at MERRA and not MERRA-2 in this study. We mistakenly wrote it as MERRA-2 in the original MS. As show in Figure R1, MERRA-2 gives a lower RPR because it overestimates the snowfall too much and underestimated the rainfall in the Arctic Ocean. We corrected this in the revised MS and added a discussion about the results of Boisvert et al. (2018). Please see details at L84-91 in the revised MS with trace of changes. Figure R1ïijŽTime series of monthly rainfall precipitation ratio (RPR) averaged over the Arctic Ocean for MERRA and MERRA-2.

As you have mentioned, changing the threshold of precipitation events may affect the determination of the date of the first rainfall to a certain extent, especially for the reanalysis with more frequent trace precipitation, such as MERRA and MERRA-2 (Boisvert et al. al., 2018). Therefore, a lower precipitation threshold may result in an earlier ROS event in these reanalysis datasets, while a higher threshold may result in a later ROS event. However, this will not change the pattern of the trend of ROS occurrence time. We have acknowledged this possibility and added a discussion in the revised MS (L181-186). "Changing the threshold of precipitation events may affect the determination of the date of the first rainfall to a certain extent, especially for the reanalysis with more frequent trace precipitation, such as MERRA and MERRA-2 (Boisvert et al. al., 2018). Therefore, a lower precipitation threshold may result in an earlier ROS event in these reanalysis datasets, while a higher threshold may result in a later ROS event. However, different thresholds will not have a fundamental impact on the spatial distribution of the trend of the first rainfall timing."

Line 106-111: I'm not entirely sure how the station data from Environment Canada would be acquired by another researcher to reproduce that results of this study. It is stated these data are part of the national archive, but also that they were acquired via "personal communication". Is there no link or DOI or citable information for the national archive data? Response: Thank you for your suggestion. We have included a link for the station precipitation data across the Canadian Arctic Archipelago (http://climate.weather.gc.ca/index_e.html) in the revised MS.

Line 131 – 132: The method described by Bintanja (2018) involves a discrete difference between two periods. It's not stated what two periods are being used for that differencing in this study. Response: The statement: "......more than 80% of the increase in early melt season precipitation occurring as rainfall can be attributed to the solid-to-liquid precipitation phase transition...." has been removed in the revised MS since it will not affect the main conclusion of this study.

Line 133–142: This description also merits some clarification. For example, the meaning of the word “original” in Line 139 is unclear to me. Do the authors mean “original” with respect to time (i.e., “original” means the early part of the time series)? Do the authors mean the time series of precipitation occurring as ROS prior to detrending? I would think they mean the latter because of previous statements about detrending. Another thing that is unclear is that “decrease in snowfall” sounds like an overall rate (mm of snow/yr), whereas saying “differences” (pluralized) implies subtracting two time series element-wise. The latter appears more in line with the rest of the description. Response: Thank you for your suggestions. The “original” here means the time series prior to detrending. We have clarified and revised this in the revised MS. “. . . the differences in the snowfall amount due to the change in precipitation phase was derived by the difference of the time series of precipitation occurring as ROS events before and after detrending.”

Section 3 Comments Lines 148-150: I’m not convinced that difference is a problem. ROS events can lead to percolation and re-freezing in the snowpack during the cold season, so it seems likely the first ROS detected sometimes pre-dates melt onset, leading to lower average values for first ROS event day than for early melt onset day. Line 184: Since the Gimeno reference doesn’t seem to focus on ROS events, I wonder if Bieniek et al. (2018) might be a better reference here. It may be focused on terrestrial Alaska, but it explicitly links ROS events to moisture transport like atmospheric rivers. Given the author list here, I assume there is some familiarity. Response: Thank you for your comment. The reference here has been changed to Bieniek et al. (2018) in the revised MS.

Line 186: I think that statement about emissivity alteration merits a reference, however logical. I know it’s described in Markus and Cavalieri (2000), but there may be a better reference. Also Line 186: Rather than “On the other hand”, I think “Additionally” is a more appropriate phrase here. Response: Thank you for your suggestion. We have added two references to support this statement. Ferraro, R. R., Peters-Lidard, C. D., Hernandez, C., Turk, F. J., Aires, F., Prigent, C., Lin, X., Boukabara, S.-A., Furuzawa, F. A., Gopalan, K., Harrison, K. W., Karbou, F., Li, L., Liu, C., Masunaga, H., Moy, L., Ringerud, S., Skofronick-Jackson, G. M., Tian, Y., Wang, N.-Y.: An Evaluation of Microwave Land Surface Emissivities Over the Continental United States to Benefit GPM-Era Precipitation Algorithms, IEEE Transactions on Geoscience and Remote Sensing, 2013(51), 378-398, 2013. Markus, T., and Cavalieri, D. J.: An enhancement of the NASA Team sea ice algorithm, IEEE Transactions on Geoscience and Remote Sensing, 38(3), 1387–1398, 2000.

We have also changed “On the other hand” to “Additionally” in the revised MS.

Organization of Section 3.2 and Line 205: I think Line 205 (the latent heat argument) would be more effective in the explanation for why ROS matters to melt onset, which is explored in the prior section. Additionally, Section 3.2 is short enough, that I think the authors would be better off by basically flipping the order of the first two paragraphs. Paragraphs 1 and 3 cover similar ground and could be condensed a little if consecutive. Another reason for this is that the last line of Section 3.3 makes sense more immediately following the current paragraph 1 (179-187) than following paragraph 2 (189-196). Response: Done.

Line 251: “Canadian Basin” is used here, but “Canada Basin” is used elsewhere. Response: We have used ‘Canada Basin’ uniformly throughout the revised text.

Lines 260-261: I think the authors can use stronger language here – in all eight cases, the trends are predominantly positive, and that’s not clear from the current statement. Response: We have modified the statement here based on your suggestion. “Overall, the RPR trends are predominantly positive over the Arctic Ocean in May and June, although there are large spatial differences among the products in both months.”

Lines 269-272: This statement goes by fast, and the authors don’t fully explain what “further analysis” was done. Looking back at the methods section, this must be the Bintanja (2018) method. A brief reminder here would go a long way. Response: This statement has been removed in the revised MS since it will not affect the main conclusion of this study, as in the reply to your previous comment.

Line 280: I am not sure what the phrasing "got contributions of the largest decreasing trend" means. Do the authors simply mean these regions "exhibit" the largest decreasing trend"? Response: This has been revised to: "The Kara/Barents Seas and Canadian Arctic Archipelago exhibit the largest decreasing trend (more than -2.0cm/decade)."

Line 285: A follow-up of sorts to the Webster et al. (2014) paper also explored the importance of cyclone activity in driving snow depth (Webster et al., 2019). This might be a good place to discuss that. Here or perhaps in the introduction. Response: Thank you for your reminder, we did not notice this reference in previous research. We have added a statement of the long-term changes in the surface snow of sea ice and its causes in the "Introduction" section (L326-327) in the revised MS and also revised the statement here (L339). "Webster et al. (2019) investigated the inter-decadal changes in snow depth over Arctic sea ice, and attributed its variability and trends mainly to cyclone activity and accompanying precipitation, followed by the sea-ice freeze-up." "This study suggests that the interdecadal decrease in snow depth on sea ice in spring is enhanced by the change in precipitation phase (solid to liquid) during the initial ablation period, in addition to the impacts from variability in cyclone snowfall over the snow accumulation season (Webster et al., 2019) and delayed sea ice freeze-up during autumn (Webster et al., 2014)."

Section 4 Comments Lines 291-292: In isolation, this statement is not true because other studies have combined coastal station observations and renalyses in North America to detect rain-on-snow events (e.g., Rennert et al., 2009; Bieniek et al., 2018; Crawford et al., 2020). I believe the spirit of this statement is that this is the first study to synthesize these datasets to examine rain on snow-covered sea ice. I agree that the application to sea ice is novel, but tweaking the language slightly would be best. Response: Thank you for your comment. We have made clarification here to emphasize that the innovation of this work lies in the application to sea ice. "This study, for the first time, synthesizes station observations at coastal sites in Arctic North America and multiple atmospheric reanalysis datasets, to examine rain on snow events over sea ice."

Lines 295 – 320: These paragraphs are all strictly summary of the results and add little or no new discussion. They are therefore less effective than the first paragraph or last three paragraphs of this section. I encourage the authors to condense them into one paragraph that highlights one or two key points in a more generalized way (i.e., shifting closer to the language of the abstract). Response: Done.

Lines 305-306: In Lines 227-228, the authors mention a "lack of significant trends" for March and April, so it seems incongruous to mention them here as increasing trends. I would restate this as "May and June" instead of "March-June". Response: Thank you for your comment. We have corrected the statement here. "The rain-precipitation-ratio (RPR) averaged over the Arctic Ocean shows a significant increasing trend in May and June in all reanalysis datasets, although there are differences in the magnitude of the trend among the datasets."

It also might make sense to remove "The RPR value and its increasing trend averaged over the Arctic Ocean are strongest in May and June in all datasets," if only May and June are discussed. Response: Done.

Lines 322-346: I think these paragraphs are stronger than the earlier part of this section. The first sentence of the penultimate paragraph in particular is a clear, useful summative sentence. But the length of this discussion/conclusions section buries that statement. Response: We have adjusted the structure of Section 4 according to your suggestions, and deleted and summarized several detailed and specific conclusions. The revised section is more compact, which should be helpful to highlight the main conclusions.

Works Cited in this Review: Bieniek, P. A., Bhatt, U. S., Walsh, J. E., Lader, R., Griffith, B., Roach, J. K., & Thoman, R. L. (2018). Assessment of Alaska rain-on-snow events using dynamical downscaling. Journal of Applied Meteorology and Climatology, 57(8), 1847–1863. http://doi.org/10.1175/JAMC-D-17-0276.1 Boisvert, L. N., Webster, M. A., Petty, A. A., Markus, T., Bromwich, D. H., & Cullather, R. I. (2018). Intercomparison of Precipitation Estimates over the Arctic Ocean and Its Peripheral Seas from Reanalyses. Journal of Climate, 31(20), 8441–8462. http://doi.org/10.1175/JCLI-D-18-0125.1 Crawford, A. D., Alley, K. E., Cooke, A. M., & Serreze, M. C. (2020). Synoptic Climatology of Rain-on-Snow Events in Alaska. Monthly Weather Review, 148, 1275–1295. http://doi.org/10.1175/MWR-D-19-0311.s1 Kapsch, M.-L., Graversen, R. G., Tjernström, M., & Bintanja, R. (2016). The effect of downwelling longwave and shortwave radiation on Arctic summer sea ice. Journal of Climate, 29(3), 1143–1159. http://doi.org/10.1175/JCLI-D-15-0238.1 Liu, Z., & Schweiger, A. (2017). Synoptic conditions, clouds, and sea ice melt-onset in the Beaufort and Chukchi Seasonal Ice Zone. Journal of Climate, 30, 6999–7016. http://doi.org/10.1175/JCLI-D-16-0887.1 Markus, T., & Cavalieri, D. J. (2000). An enhancement of the NASA Team sea ice algorithm. IEEE Transactions on Geoscience and Remote Sensing, 38(3), 1387–1398. http://doi.org/10.1109/36.843033 Reichle, R. H. Q. Liu, R. D. Koster, C. S. Draper, S. P. P. Mahanama, and G. S. Partyka, (2017). Land surface precipitation in MERRA-2. Journal of Climate, 30, 1643–1664, https://doi.org/10.1175/JCLI-D- 16-0570.1. Rennert, K. J., Roe, G., Putkonen, J., & Bitz, C. M. (2009). Soil thermal and ecological impacts of rain on snow events in the circumpolar Arctic. Journal of Climate, 22(9), 2302–2315. http://doi.org/10.1175/2008JCLI2117.1 Webster, M. A., Parker, C., Boisvert, L., & Kwok, R. (2019). The role of cyclone activity in snow accumulation on Arctic sea ice. Nature Communications, 10(1), 1–12. http://doi.org/10.1038/s41467-019-13299-8.

Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2020-214/tc-2020-214-AC1-supplement.pdf

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