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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.

Anonymous Referee #1

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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: âĂć Rain-on-snow events are often a trigger for early melt onset on sea ice. âĂć 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 âĂć 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). âĂć The ERA products (ERA-Interim 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).

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. 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. 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? 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. 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.

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. 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. 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). Line 251: "Canadian Basin" is used here, but "Canada Basin" is used elsewhere. 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. 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. 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"? 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.

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. 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). 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”. 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. 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. 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

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