

Interactive comment on “An update on global atmospheric ice estimates from satellite observations and reanalyses” by David Ian Duncan and Patrick Eriksson

Anonymous Referee #2

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Review of "An update on global atmospheric ice estimates from satellite observations and reanalyses" by David Ian Duncan and Patrick Eriksson

This manuscript is a nice analysis of atmospheric ice from observations and reanalysis simulations. In general this is a nice and careful analysis, I applaud the authors for care with co-location and other data analysis methods which are well done and avoid common problems. I think this should be publishable in ACP with several important modifications. Most importantly the conclusions could be significantly sharpened and improved to make this more useful.

This paper could be a really excellent contribution with sharper conclusions and a better

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summary. I think the summary does need a bit of a rewrite to sharpen the conclusions from the analysis. As noted below, the summary and conclusions could note what the analysis says about where differences lie (noted in specific comments). Also the relative merits of the reanalyses (models) and review what they assimilate and how this might affect the results.

Page 1, L8: homologous ? The word is a bit obtuse. Not good in an abstract, I think 'similar' would be fine here and easier on the reader.

Page 1, L14: can you bound uncertainty? Can you attribute systematic differences to microphysical assumptions?

Page 2, L3: the motivation here could be stronger. Ice clouds are generally the radiating layer to space. Ice cloud properties are highly uncertain, which makes this a difficult problem.

Page 2, L4: I would probably suggest ice content is 'prognostic'. It is part of the hydrologic cycle.

Page 2, L24: the difficulties you have accurately spelled out here beg the question of whether we should look at IWP or IWC at all, and instead work in a space where we understand the measurements (e.g. just compare attenuation due to ice for different wavelengths to a simulated version of these quantities).

Page 4, L2: I would like to see a discussion of satellite simulators here. At least acknowledge there are other ways forward.

Page 5, L33: isn't ice just precipitating (snow) and non precipitating? What is MERRA missing? Maybe you should use consistent terms here.

Page 6, L4: you might need to be specific about what is assimilated and whether it is independent data you are comparing the reanalyses to.

Page 6, L6: a zonal mean plot of all 6 would help make this quantitative. It might be

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better unless there are pattern differences you want to point out. I guess this is figure 3, but I would like to see this mentioned here. Possibly even discussed here.

Page 6, L14: I like the detail on co-location. This is helpful. Does the Dardar picture qualitatively change if you only use 2015?

Page 6, L24: should this skewness be obvious in figure 2? It does not look like it.

Page 7, L5: what does not shown in figure 2 mean if zero values are included mean?

Page 7, L11: might be better to describe the sensitivity thresholds of each instrument specifically.

Page 9, L7: here or later it would be useful to understand what data sets you trust. The fact that the models seem to put a large mass as precipitating (snow) is worth mentioning. Is this realistic?

Page 13, L8: what percent of variance is explained by the second principle component and is this significantly different than PC1 and PC3?

Page 14, L27: for figure 8, was the diurnal difference calculated before smoothing on each point? Or were day time and night time smoothed separately and then differences? I'm guessing the latter. Please clarify.

Page 16, L4: relative to the insitu data from Heymsfield 2017, what satellite data do you think compares best with insitu. Why?

Page 16, L14: can you explain the mechanism for the diurnally dependent microphysics in another sentence please? What was the Gong et al 2018 mechanism?

Page 16, L24: the discrepancy for large IWP probably deserves highlighting. There attenuation and microphysics with large particles may matter a lot. That they would also affect total mass disproportionately. This might be worth highlighting.

It may also enable some comments on why the seasonal cycles look similar but not the

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diurnal cycle.

Page 16, L25: what about the reanalyses? Can you please summarize what they do or do not assimilate, how that reflects the results, and comparisons between MERRA and era5?

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