

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

Anonymous Referee #1

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This manuscript presents a critical global ice water path (IWP) retrospective by comparing satellite derived IWP to two modern reanalysis (ERA5, MERRA) datasets. Global mean spatial and zonal analyses are provided, as well as a diurnal analysis. This study highlights major incongruities that, despite major research efforts, still exist between respective satellite derived IWP and reanalysis datasets. This manuscript underscores the need to develop more robust observational platforms to reduce IWP estimate uncertainties and should serve as a foundational reference for future IWP related work in both the observational and modeling worlds. The manuscript is exceptionally well written from both stylistic and organizational standpoints. I found the introduction section to be extremely effective. The authors distill the IWP research problem and outstanding

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unsolved issues in a succinct, yet thorough, manner.

After reading the manuscript for the first time, I will admit that I possessed the initial mindset that the authors should also try to solve the major discrepancies between the respective datasets. My initial review posed the following question to the authors: Can the authors provide any substantive and conclusive reasons to explain WHY the reanalysis and satellite IWP estimates are so grossly different? But after reading the manuscript a second and third time, I believe this is an unreasonable request. After digesting the manuscript further, I think that in its present form, it elegantly summarizes the current state of the IWP modeling and space-based remote sensing world as a much-needed update to the Waliser et al. (2009) study. The authors properly advertise the current manuscript as a Waliser et al. (2009) update, so I do not think it should be construed in any other way. The authors' message is quite simple based on the evidence collected – we still have much work ahead of us to solve the global IWP problem as a community. It will take concerted future research efforts to explain the dataset differences, but this current study serves as a new launching point.

Major comments:

1. This is a positive major comment. The authors chose to subsample the reanalysis datasets to mimic CloudSat's sampling. I think this is a wise choice that should be adopted in future studies by investigators evaluating these disparate datasets.
2. My only major methodological comment is related to the different time frames chosen for each dataset displayed in Figure 1. DARDAR uses 2008-2015, SI 2013, and the other datasets 2015. Can the authors surmise whether interannual variability diminishes the value of the comparisons? I'm not sure how to offer any constructive suggestions that can assuage any fears related to this issue. Could the reanalysis datasets be analyzed for the 2008-2015 period to illustrate the importance of IWP interannual variability?

Minor Comments:

1. Any particular reason why MERRA was chosen versus MERRA-2?
2. Page 5, Line 8: The authors state that GPM provides estimates of precipitating IWP. I would argue that IWP should theoretically be possible to retrieve using the high GPM radiometer frequencies under many non-precipitating circumstances. For example, thick tropical cirrus sometimes pose a radiometer precipitation retrieval problem due to a tangible scattering signature.
3. Page 6, lines 10-14: The authors state that A-Train co-locations were not used, but how large were the subsampled “swaths” in the reanalysis datasets to mimic A-Train orbital patterns? Were just the longitudinal belts at 13:30 local time progressively sampled in the reanalysis datasets? If so, were the longitudinal belts only one grid point, or many grid points wide? I’m slightly confused about the mechanics used to extract subsampled reanalysis grid points used in the analysis. Please provide further details so other investigators can replicate or adopt a similar strategy in future studies.
4. Page 6, Line 34: Was the 6 pixel DARDAR average chosen arbitrarily, or was there a physical reason that this averaging strategy was adopted? The MODIS, SI, and GPM-AMSR2 dataset probably all have different effective footprint sizes used for their products, so was 6 pixels a proxy average value that optimally satisfied the different passive footprint sizes?
5. Figure 1: I erroneously thought SI was a reanalysis dataset since it was in the same column as ERA5 and MERRA. I quickly found the SI acronym listing in the manuscript text, but I recommend somehow highlighting the satellite versus reanalysis datasets somewhere in the Figure 1 caption to quickly remind readers. A simple fix is to state that ERA5 and MERRA are reanalysis datasets, DARDAR, GPM-AMSR2, MODIS, and SI are satellite datasets, then refer the reader to Section 2 for further descriptions.

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