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

David Ian Duncan and Patrick Eriksson

david.duncan@chalmers.se

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Sincere thanks are extended to both reviewers for their thoughtful comments, which have undoubtedly improved this manuscript. The paper's conclusions have been augmented and strengthened, with other points in the paper addressed to improve the clarity and readability of the text. Figure 2 was expanded to two panels to show the significant influence of large IWP values on the mean values reported, which engendered some further discussion. All significant modifications to the text are detailed here, as specific reviewer comments are addressed below in order, with reviewer comments (offset by ***) followed by the responses.

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Referee #1:

*** 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?

A sentence was added to Section 3: "To ensure that 2015 is not an outlier for IWP and that interannual IWP variability is not a big concern, ERA5 gloal means were analysed and found to vary by about 1% year to year, with 2015 being a typical year." To elaborate on this slightly, attached is a plot of ERA5 zonal mean IWP for 7 recent years of data, sampled daytime-only and displayed as in Fig. 3 of the manuscript. This view shows that while there is some variability in the distribution of IWP zonally and globally from year to year, it is on order 1% for the global mean and varies by a few percent at most latitude bands, with greatest variability seen around the equator, presumably dependent on ITCZ fluctuations and ENSO. ERA5 mean IWP for 60N-60S in 2015 is 67.1g/m2, whereas for instance 2013, 2014, and 2016 are 66.4, 66.8, and 67.8, respectively.

Minor Comments:

*** 1. Any particular reason why MERRA was chosen versus MERRA-2?

1. MERRA-2 was indeed used throughout the study, though the style of referring to it as just 'MERRA' after the initial introduction as MERRA-2 was admittedly confusing. All mentions of MERRA have been amended to MERRA-2 in the revised manuscript.

*** 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.

2. It is true that the GPM radiometer (GMI) does have sensitivity to cloud ice given its 166GHz and 183GHz channels. However, as stated in Section 2.1, the GPM radiometer algorithm is a Bayesian retrieval driven by the sensitivities of the GPM radar, DPR. Effectively, if DPR does not see a hydrometeor, then the GPM radiometer algorithm will not see it either, because the a priori database is built upon what the radar sees. At least for rain, this is recognised as a shortcoming for the radiometer algorithm since valuable signal is wasted for light rain scenes below the detectability threshold of DPR, and efforts are underway to exploit this in subsequent product versions. To the authors' knowledge, no similar activity is currently underway to use GMI for cloud ice retrieval.

*** 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.

3. As mentioned in Section 4 on vertical profiles, for the global analysis the reanalyses were similarly sliced up longitudinally to better populate the fields for comparison with the satellite datasets, with widths of 45 degrees (or 3 hours). To clarify the initial description, the width of gridded data sampled has been inserted into the text: ". . .sampled daily at about the satellites' crossing time with a width of 45ậŮę longitude."

*** 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?

4. The reasoning here is a little convoluted. We followed the example used in the GPM V05 database creation, where CloudSat rain rate statistics were sampled using a running mean of 6 pixels because this both approximated the size of the GPM imager footprint and yielded rainfall statistics that were not dissimilar from those of other GPM estimates (see GPM V05 passive algorithm ATBD). For the purposes of this study, the reviewer is correct in that these datasets do have differing footprint sizes, so it is a problematic decision to make. The choice of a 6 pixel running mean was better than using none, and also found to not yield significantly different results than say a 5 or 7 pixel running mean. The MODIS data were excluded from this analysis because the 1-degree gridded product was too dissimilar to the native/L2 data to warrant inclusion.

*** 5. Figure 1: I erroneously though 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.

5. Figure 1 has been amended to make this delineation between satellite and reanalysis datasets more explicit, both in the figure and caption: "Global mean IWP maps from six datasets: 4 satellite datasets (DARDAR, GPM, MODIS, SI) and 2 reanalyses (ERA5, MERRA-2)."

Referee #2:

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

A few of the reviewer's comments caused further investigation that has added to the conclusions of the manuscript. This was especially true with Figure 2 and the importance of high IWP values, which prompted the figure to be expanded and a paragraph added to the discussion of IWP frequencies in Section 3. The conclusions section has been sharpened slightly and expanded. There is now a paragraph to sum up conclusions about the reanalyses, and statements about precipitating ice and comparisons to in situ data. The additions are intended to better convey which datasets to trust and where the uncertainties lie. But as no dataset holds claim to the truth, the conclusions are not forcefully presented.

The specific comments are addressed in order below:

*** 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.

1. 'Homologous' was changed to 'similar.'

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

2. This is a very tough problem to address, because the systematic differences for a given retrieval would be more regional or regime-dependent than something simple like a 20% low bias across the board. To a degree, we skirted this problem because it is too complex for this study to tackle head on. Even studies such as Heymsfield et al. (2017) that are relatively exhaustive also tend to skirt these issues, because in-situ data are by definition not global in scope and so narrow conclusions can be drawn. Ultimately, this particular study endeavours to present the results as they are, as others have gone into detail on how important the microphysical assumptions are in ice cloud retrieval.

However, to the authors' knowledge none has really examined how these assumptions scale up to global uncertainties in ice cloud mass.

*** 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.

3. This prompted the strengthening of this paragraph by including this sentence: "Ice clouds are the most significant emitter of longwave radiation out to space, and thus uncertainty about their properties impacts the global energy balance."

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

4. Changed to 'prognostic.'

*** 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 wavelenghts to a simulated version of these quantities).

5. To address this viewpoint, similar to the following comment about satellite simulators, it is a well-taken point that harping on IWC is somewhat immaterial if the radiative properties are relatively invariant within the error bounds. However, we posit that comparison of physically understandable/intuitive quantities has some inherent value rather than discussing extinction at a given wavelength, say, and makes comparison between retrievals and models more readily manageable. This is a worthwhile point to stress, though IWC still seems the best way to compare different datasets in this manner.

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

6. In response, the following sentence was added to the 3rd to last paragraph in the introduction: "Satellite simulators offer an alternative perspective Masunaga et al.

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(2010), in that retrieval of physical quantities may be viewed with secondary importance if signals at various wavelengths are well simulated; but, in the context of developing further physical understanding, this study focuses on the retrieved quantities."

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

7. This section has been reworded to hopefully provide better clarity, seen below as #8.

*** 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.

8. The section describing the reanalyses has been modified to include discussion of what is assimilated by both MERRA-2 and ERA5, and how independent they are from the satellite datasets analysed. Additionally, a new citation (Geer et al., in press) was added to mention that ERA5 IWP lacks the convective ice component, which the authors were previously unaware of, but was pointed out at a conference presentation. Because this subsection was becoming lengthy, its paragraphs were divided up to improve readability, seen in full below:

"Two reanalysis datasets are used in this study. The first is the European Centre for Medium-range Weather Forecasts (ECMWF) Reanalysis 5, known as ERA5. The other is the Modern Era Retrospective Analysis for Research and Applications version 2 (MERRA-2), a dataset produced by NASA's Global Modeling and Assimilation Office (Gelaro et al., 2017). Both reanalyses provide profiles of ice water content, and both were downloaded at 0.5âŬę resolution.

Differentiating between precipitating and non-precipitating ice is problematic for models and observations alike, as noted in previous studies (Waliser et al., 2009; Jiang et al., 2012). ERA5 is different from all other datasets used in this study, as it differentiates between precipitating and non-precipitating ice as snow water and cloud ice water,

respectively. Because all other datasets queried treat all ice together, ERA5 values reported here are combined unless otherwise stated. Additionally, a caveat to ERA5 IWP values discussed herein is that ERA5 does not output convective ice (Geer et al., 2018), which is parametrized in the model as a convective flux, and thus not included in total IWP here. In contrast, MERRA-2 parametrizes all precipitating ice and outputs only non-precipitating ice.

A noted difference between ERA5 and MERRA-2 is that ERA5 assimilates cloud- and precipitation-affected radiances from microwave sensors at a higher rate and for more channels (Geer et al., 2017; McCarty et al., 2016; Geer et al., 2018). Since these are the channels most sensitive to columnar atmospheric ice, it is hypothesized that ERA5 should represent a better estimate than MERRA-2. ERA5 assimilates a portion of all-sky radiances from both the AMSR2 sensor and the NOAA-18 satellite, and MERRA-2 assimilates MHS radiances from NOAA-18, so the reanalysis estimates are not entirely independent of the GPM and SI estimates given."

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

9. The authors considered moving the zonal means plot ahead in the manuscript, but prefer having the global view in exponential space shown first, before giving the zonal means in linear space to accent the quantitative differences. However, to aid the quantitative interpretation, the near-global mean values have been added to Fig. 3 (attached).

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

10. It does not change qualitatively, but a single year of CloudSat/CALIPSO data provides noisy statistics, especially when just using daytime-only observations, which is why we used as many years of DARDAR data as possible to make, for instance, the

DARDAR panel in Fig. 1 a coherent image instead of a very noisy one.

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

11. This comment prompted Fig. 2 to be expanded (attached), as noted in the general comments above. The new view of a mass-weighted PDF accentuates this difference on the high end of the IWP spectrum, with ERA5 exhibiting much greater prevalence of very high IWP values in comparison to DARDAR or 2C-ICE.

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

12. To clarify this in the text, the sentence now reads: "Zeros are accounted for in the calculated frequencies but not shown in Fig. 2 due to the logarithmic scale." In other words, the zero values would be difficult to show on this plot, but summing the curves shown will yield 100% only if also adding the true zero values quoted in the text. This is why SI has a smaller area under the curve when compared to the other datasets, as it has a lot of true zeros.

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

13. As the sensitivities depend on regime and microphysics, we added a short statement on sensitivity limits to guide readers: "For perspective, the detectability limit of CloudSat/CALIPSO is roughly 1g/m2 whereas for passive microwave less than 190GHz the detectability limit is more like 10 to 100g/m2 (Holl et al. 2014)." The CloudSat/CALIPSO detectability limit is close to 1mg/m3 in a given range gate, which is of order 1g/m2 for a cloud of any significant thickness.

*** 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?

14. Precipitating vs. non-precipitating IWC is now addressed more directly in

manuscript, via citing the recently published Deng et al. (2018). This is addressed primarily in the 3rd paragraph of section 7:

"Differentiating between cloud and precipitating ice remains an issue when comparing model output with observations. Deng et al. (2018) argues that the majority of IWC at all atmospheric levels measured by 2C-ICE is particles of maximum diameter less than 800 μm and thus presumably non-precipitating, which might indicate that ERA5 has too much precipitating ice, since SWP dominates over CIWP in most regimes for ERA5. Such differentiation is important, but depends on a fall speed to designate precipitating ice, and even CloudSat/CALIPSO had very limited information on this. If the GPM estimate is taken to represent a lower bound for precipitating ice due to its sensitivity primarily to large hydrometeors, this is not too far off the magnitude of SWP in ERA5. It is troubling that the CloudSat/CALIPSO estimates' global means are driven by high IWP cases (Fig. 2), where attenuation and partitioning into ice and mixed-phase are significant potential error sources for CloudSat. If there were a systematic high bias in retrievals of very high IWP from CloudSat, then the spread between estimates would shrink considerably."

*** Page 13, L8:what percent of variance is explained by the second principle component and is this significantly different than PC1and PC3?

15. This is now addressed directly as a follow-on to the original statement: "...the second PC represents 23, 23, 14, and 27% of the variability for the same, but is not significantly different from the third PC for the observational datasets, as noise seems to dominate the signal."

*** 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.

16. The reviewer is correct that it was the latter. The text has been amended to clarify this point: "...the gridded data were smoothed before differencing."

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

17. This comment dovetails with the response for point #2 above. It may seem like dodging the question to say that some datasets perform better than others in some places, but that is the truth as investigated by the Heymsfield et al. (2017) study.

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

18. The phenomenon mentioned in the Gong et al. study is now given in the text: "For instance, Gong et al. (2018) found that ice particle axis ratio displays a distinct diurnal cycle over land and thus impacts polarimetric microwave measurements."

*** 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 diurnal cycle.

19. In conjunction with point #11 and #14, this comment prompted reexamination and highlighting in both Section 3 and the conclusions. As the relevant part of the conclusions is quoted above for the response to #14, below is the relevant paragraph from Section 3:

"The right panel of Fig. 2 features mass-weighted frequencies, showing which IWP bins contribute most to each dataset's mean. Integrating each curve in the right panel would approximate the near-global mean values presented in Fig. 3, absent lati- tudinal weighting. This view shows that the main cause for discrepancies in mean IWP comes from the high endâĂŤmagnitudes in excess of 800 g m-2âĂŤwith the peak contribution for 2C-ICE and DARDAR coming from IWP values of 1 to 16 kg m-2. Notably, 2C-ICE and DARDAR diverge for very high IWP values, indicating the importance of

microphysical or retrieval assumptions for these cases, since these observations use the same data. ERA5 and SI overestimate the prevalence of very high IWP values relative to the CloudSat/CALIPSO-based estimates, with ERA5 reporting IWP values greater than 16 kg m-2 significantly more often than either GPM or DARDAR. These large IWP values have outsize influence on the means despite their low frequencies of occurrence. For instance, DARDAR retrieves >4 kg m-2 less than 0.5% of the time but this accounts for almost a fourth of the global mean IWP; in contrast, ERA5 estimates such cases at less than 0.1% frequency, contributing about a ninth of its global mean.

*** 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?

20. As with point #8, the introduction of the reanalyses was amended to include discussion of what is assimilated. The conclusions have also been adjusted in line with this comment, hypothesizing that the relatively poor representation of IWC profiles in MERRA-2 may be a symptom of limited use of all-sky microwave radiances:

"For the reanalyses, MERRA-2 seems to underestimate ice mass systematically, since non-precipitating ice should dominate total IWP (Deng et al., 2018). MERRA-2 also distributes cloud ice markedly differently from both DARDAR and ERA5, which may be a consequence of assimilating few all-sky channels sensitive to scattering. ERA5 exhibits greater magnitudes of non-precipitating ice, but seems to possess both too much precipitating ice overall and high frequencies of very large IWP values (Fig. 2) that may not be physical. But ERA5 provides what appears to be a reasonable estimate of atmospheric ice at all but near-instantaneous scales (Fig. 8), especially when considering the caveat that convective ice flux is not included in this analysis. ERA5 captures large-scale variability well in comparison with satellite estimates, and matches reasonably well on the vertical distribution of mean ice mass, if not the magnitude."

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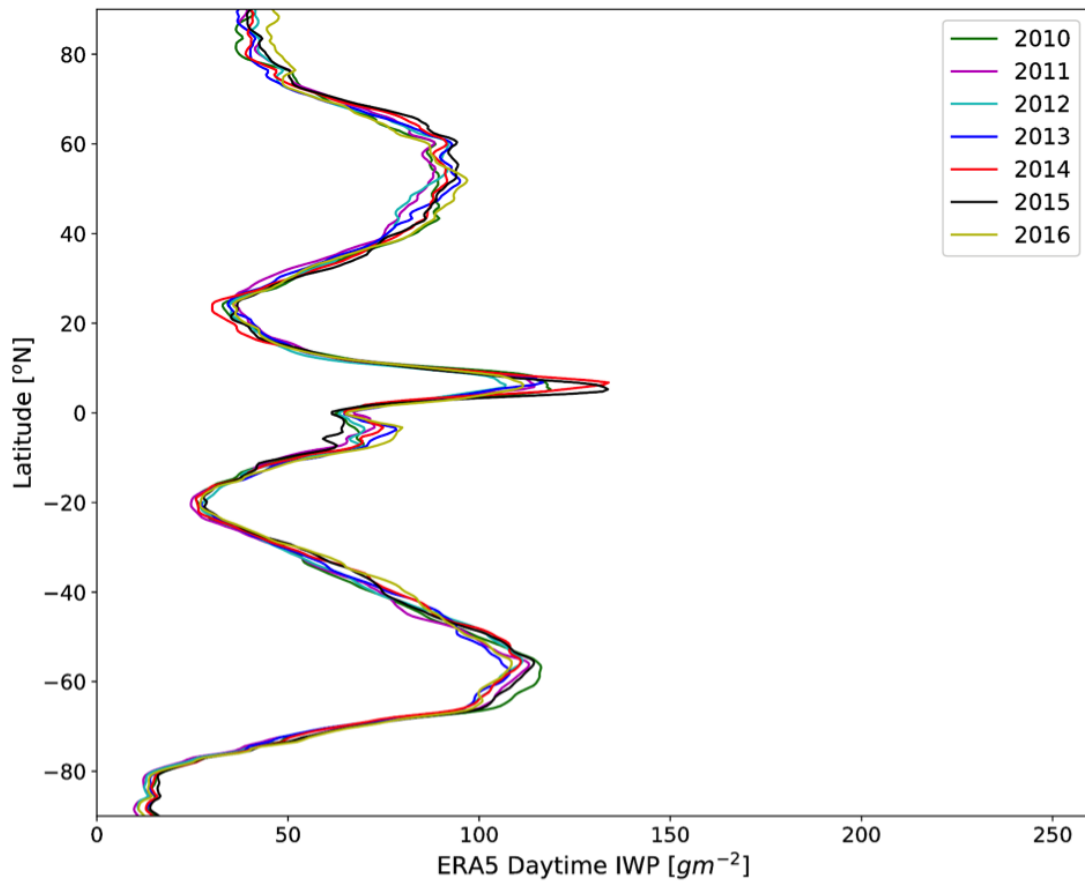
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Fig. 1. 2010 to 2016 ERA5 zonal mean IWP

Fig. 2. Figure 2 (updated)

C15

Means ($60^oN - 60^oS$):

DARDAR   126.8
ERA5 CIWP   21.7
ERA5 IWP   67.1
GPM (AMSR2)   29.4
MERRA-2 CIWP   17.0
MODIS (Aqua)   44.6
SI (NOAA-18)   97.2

Fig. 3. Figure 3 (updated)