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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Assessing modelled spatial distributions of ice water path using satellite data" *by* S. Eliasson et al.

D. Waliser (Referee)

duane.waliser@jpl.nasa.gov

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Dear Authors,

I have had the opportunity to review your manuscript. It is in an area I've worked in closely in recent years and am happy to see the direction you've taken and efforts you've made in regards to more thorough examinations of cloud-ice and their inter-observation and inter-model comparisons. However, I find a few things that could use improvement. These include the representation of this paper in context of others, the issue of normalization and its discussion possibly problematic, the comparisons between the model and obs despite them representing different quantities and what I would say is loose language regarding comparison terminology that bears more careful attention. Unfortunately, except for the first one above, I am on the fence about whether to categorize the others as “minor” because they are so important to clarify and fix – although it may not take a tremendous amount of work or analysis. Because of the latter and because these comments will apparently be available for others to read and consider, I’ve note the revisions needed as “minor” to the editor.

You were very gracious in your referencing of our paper (Waliser et al. 2009), although I will admit I think the set up of the interplay between the two could be improved. When I read the opening two paragraphs, the motivation and notions seemed to be redundantly similar to our paper, yet the latter wasn’t referenced as such. For example, our paper outlined the problem of too few obs to constrain model IWP/IWC, the model-data comparison difficulties, the large model biases and the importance of getting this feature/process right of significant importance...and we even discussed and compared a number of different observed data sets e.g. MODIS, CERES, CLOUDSAT, ISCCP and NOAA/AMSU. What I think you could do to make for a more precise objective of the paper for the reader and make it more clear how this builds on earlier work is to indicate how while our paper illustrated the large-level of disagreement and a qualitative and very cursory level of comparison between the available data sets, this paper provides a quantitative more comprehensive comparison between the observed data sets and then in turn uses this information to compare to the models - with an intent on evaluating both the variability and the mean. However, see my comments below about to what extent you can really make these comparisons.

At least twice in the paper, there is mention that CloudSat data are not yet adequate to provide validation information due to the short record and relatively sparse sampling (e.g. 12194; 12207 – patmos is better. . .because of its long record but since it is worse in the mean, it isn’t a “better” data set in this regard; line 5 line 12209 is arguable). I would argue that given the means of the GCMs differ by two orders of magnitude when considering the time average maps (our figure 3), that even having the mean information provided by CloudSat is extremely worthy and valuable – and the interannual variability is not big enough to produce a problem. Moreover, it there is also mention

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along the way about the sparseness of some of the data sets and the need to retain the high resolution data (e.g. 12195) but yet when averaged to 5 deg grids or taking the zonal mean (you do both with success), there is significant information to be yielded. Apart from this, I agree that it is useful to use other data sets for the interannual variability - although I think it could be argued that if the means are of by an order or two in magnitude the variability is a second order issue.

Then in regards to the issue of comparisons of the means and the difference between what the models (usually cloud ice) and observations (total ice) represents – it seems worth mentioning that Waliser et al. made an estimate of the ice in clouds from the total provided by the cloudsat observations. Thus there is at least an observed estimate for modelled cloud ice, and while it may have considerable uncertainty, even if it was a factor of two, it is much smaller uncertainty than the one to two orders of magnitude between the models being compared.

In the 2nd paragraph where Eriksson et al. is referenced, the paper you reference by Wu et al. would be appropriate to cite.

It seems worth mentioning at least once, and probably more than once (e.g., paragraph 2 on page 12189 and summary Line 1 on 12206) that the group of 6 models is a subset of N (I think 18-20?) models so the reader knows how much you subsampled the CMIP3 database of models. Moreover, your selection of models seems qualitatively well founded but seems was not chosen based on quantitative metrics. This is okay, but explicit mention of this e.g. “somewhat ad hoc selection” would be appropriate.

You use the word “cloud-ice fraction” a number of times. I know what you mean but it is so tempting to read as maybe the fractional coverage of ice clouds, meaning some frequency or coverage in space of clouds. I would recommend upon first mention of this terminology to be very specific and say . . . In this article, we refer to the fraction of ice that is contained in clouds as the cloud-ice fraction and the total amount of ice in the atmosphere which also includes any precipitating ice as the “total ice”. Or something like this – just make it very explicit some place.

On pages 12195 near line 10, you speak of an "uncertainty" level – is this precision or accuracy and for what type of sampling/averaging, and what is the reference for this value? Then on line 5 of 12196, there is mention of the cloudsat "error" – what is this number and where did it come from? It took me a while to understand how this was used to develop the uncertainty values in figure 2. If the text could be more precise that would be great – even so far as to say. "For example, at 60 deg S, the value of the fraction is. . .and the uncertainty/error is. . .so upon multiplication . . .one gets. . ..and this is what is plotted as an uncertainty". This should include a more explicit discussion of how you obtainerd the "cloud-ice fraction" from the figures 10 and 11 in the Waliser et al. paper – was it just one mean value estimated from the line plots or did you use the latitudinal variation that is shown? Why not plot figure 1 and 2 on the same y scale?

I found the discussion on lines 5-15 on 12197 a bit hard to follow. Suggest improving and being more clear and precise.

Normalization. While I appreciate what is being done here and why, I think the text is fraught with a number of statements that are misleading based on this normalization. For example, from the normalized figures, there follows numerous statements about a given model being too low or too high. Take the following example that I've outlined in the table – for a hypothetical little Earth that only has 3 values – northern subtropics, tropics and southern subtropics. See table as graphic. In this case it looks like model-1 is way too "high" in the subtropics when viewed through this normalization and model-2 is perfect. But in absolute terms, model-2 is way too low and model-1 is just right in the subtropics. I know you understand this but from the maps made, there are many statements made about this or that model or satellite product being "too low" or "too high", with the reader's assumption that this means relative to observations which is not the case (e.g., "clear negative bias", "distinct dry zone" – but this may not be dry at all but only appear so because you took out a large mean, "some models (e.g., UKMO) have low IWP" – maybe not because again you took out its mean which could be high).

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Please exhibit extreme caution as you go through the manuscript again and keep in mind these are relative values only within the given model/obs themselves. I think the normalization values should be in a table rather than embedded in the figure that are harder to pick out and compare. This is even more perilous when considering the *** comment below.

In regards to the above, I would also recommend moving 3.2 after 3.3 and 3.4 since the latter two don't deal with normalized values and the flow of the paper wouldn't be broken and as the reader goes into the 3.3 and 3.4 material they wouldn't wonder if you are still using normalized values.

I found the discussion in 3.3 a little terse in its set up and discussion and I don't think the messages intended were conveyed very effectively.

Is Figure 18 in the Waliser et al. paper relevant and/or similar to the discussion in 3.4?

***As the discussion continues from 3.2 onwards, there are references of comparison between the model and observations where the notion of whether these two quantities can even be compared is lost – and in many cases cannot be compared in absolute terms. The models only have cloud ice. The obs as used here have all components of ice. However, the discussion and analysis often draws conclusions about a give model being high or low. While the beginning of the article spells the challenge out very nicely, it seems to get diffuse as the comparison are discussed. Where does that leave a reader when you've remarked a given model is "high" or "low" or the ECHAM model is "best" when it is being compared to an observation it is not representing (e.g. 12205 – line 10 - just one example "the models are in agreement with the satellite data in terms of absolute IWP in the oceanic subsidence regions" – if they agree then by definition their cloud-IWP is actually biased high). Could it be summarized a little more clearly why the ECHAM model is the "best" – particularly after taking into account the above remarks. 12206 line 15-20; 12209.

Based on a number of remarks above, the discussion and conclusions need to be

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reconsidered and clarified. 12209, line 3. I think it is arguable that long data sets are the key to understanding model deficiencies. Multi-sensor, multi-variable constraints over a few months or couple years can be more much more valuable. For example, of all the things that characterize cloud development –aerosols, wind, temperature, moisture, ice, liquid, updraft speed, microphysics, surface fluxes, etc; I'd rather have a relatively complete description of these for a short period than only a few of these over a long period.

I hope these remarks are helpful and constructive.

Best wishes Duane Waliser duane.waliser@jpl.nasa.gov

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 12185, 2010.

	obs	norm-obs	model-1	norm-model-1	model-2	norm-model-2
20N	4	-0.5773503	4	0.57735027	1	-0.5773503
EQ	7	1.15470054	2	-1.1547005	10	1.15470054
20S	4	-0.5773503	4	0.57735027	1	-0.5773503
mean	5		3.33333333		4	
stddev	1.73205081		1.15470054		5.19615242	

Fig. 1. table