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Atmospheric Chemistry and Physics Discussions

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

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[a4paper,10pt]article

enumerate

Overall

Thank you for your comments. I'll address your comments point by point. But first, ultimately, the point of the article is to survey the IWP observational data sets available, and to compare them, especially in certain geographical regions where differences are

expected to be large/ important. This is done baring in mind the inherent strengths and weaknesses of the retrievals and the instruments used. The large-scale differences between the GCMs are also assessed to highlight their differences, confirm the findings of Waliser et al., (2009), and to see where they stand by comparison. The goal is also to get a better understanding of the true IWP distribution, and to have a chance of constraining the GCMs in terms of this, we must look at several different datasets, deliberately picked to cover a wide range of different retrieval techniques. This is explained further in answer 4. Now to answer your specific questions:

1 Figure and table references in abstract

I agree that they have no place in an article abstract. These will be removed.

2 Abbreviations in abstract

I will explain the abbreviations, all of which are the names of data sets, in the abstract, as you are right in that there may be many readers who don't know them. This will make the abstract much longer, but so be it.

3 Cited: "This is well known because IWP is not the main product in climate models. They have to tune IWP to get correct OLR "

I understand your comment as implying that discrepancies between the models in terms of IWP, are justified and uninteresting, as it is not their main product and they need the leverage that IWP offers in order to achieve the right levels of TOA OLR.

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My answer to that is that, despite this the article shows the large-scale differences between the models, because we believe that IWP is an important atmospheric parameter. IWP along with r_e and τ, one of the most important cloud ice parameter in terms of radiative impact and the hydrological cycle [e.g., Zhang et al., in press, JGR], and measurements of this quantity (at least by name) exist. The article also explains that the models are different from each other due to the different microphysical assumptions that are made. It is also clearly explained that GCMs cannot be directly compared to observations (agreeing with waliser et al., 2009). Models easy and artificially distinguish between ice particle types, and discard precipitating ice at each time step, making them impossible to directly compare to any observations. It is further explained in the article that satellite observations of cloud ice cannot make the particle distinctions the models do. For this reason the article focuses on the discrepancies of the models in terms of spatial distribution and correlations rather than focusing on the IWP magnitude.

4 The are inherent differences in the observational data sets and they should not be compared

We are naturally aware that different sensors retrieve ice cloud information from different portions of the atmospheric column, especially due to their different instrument sensitivities. The article carefully explains this, and its consequence is manifested throughout the results section. Basically, in order get have a long enough time series and the most complete atmospheric column ice content information, in the long run, we must carefully combine the ice particle information from several datasets.

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5 The title: "modelled spatial distributions"

I think I agree with you here. I will probably change this to "spatial distributions of IWP from models"

6 Finally

Thank you for your input, and I really recommend to read further than the abstract in order to get a better feel for what issues have been dealt with and what limitations have been considered.

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