**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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Dear Authors, I went through the article and have a few suggestions to improve the article. I hope that my comments will help to improve the article a little bit and wish the best for your future researches.

Dear Editor, I am not sure that I, as a non-official reviewer, have the right to give any suggestion about publishing the article but anyway my suggestion to the editor is as follows. As it is not possible to perform my comments in a few weeks and I believe that some of them are very critical then I suggest to reject the article to give more time to the authors to completely re-write the article and submit it as a new manuscript. There is also another review from someone who has published a similar article. He has focused on details and individual statements. His review seems to be more or

less a comparison between his paper which has just been published and the current article. He has given very good comments but I believe that this paper needs a big revision, in addition to the some minor and structural comments. I am not going to give comments on the details but will just post my major comments. I believe that the current version is somehow misleading for people who are not familiar with either satellite data or climate models. If someone does not know that microwave sensors are not affected by thin clouds then will assume that satellite observations from microwave sensors underestimate IWP. They have mentioned this in several parts which is completely wrong. Because these sensors are there to be able to work under cloudy conditions. So they are designed to work in frequencies which are not sensitive to most clouds but just very thick clouds. This does not mean that they underestimate IWP but it actually means that work very well in most conditions. That's why we use microwave data to investigate the distribution of deep convective clouds. One can have some similar conclusions for IR data.

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Comments:

1.My first suggestion is to remove models from the article and just focus on the satellite data, I wrote in my previous comments that models can not generate correct values and have some problems in spatial domain and when you choose a small area this problems become more important. It is not suggested to compare IWP from models to the satellites. The authors has some discussions about these problems and I suggest to remove the models to have a fair comparison. This has also been discussed in a comment from the other reviewer as "The models only have cloud ice. The obs as used here have all components of ice". I think this reason and also the geo-spatial problem of models are enough to suggest to remove the models. There are some other problems as well and if you want to get an overview then I propose to have a look at related articles.

2.I suggest to just compare satellite IWP values which are consistent together, IR to IR and microwave to microwave. Please note that these sensors have different sensitivity and also weighting functions. Each of them senses a different part of the atmosphere. They are also sensitive to different clouds (optical thicknesses). If you do not have several datasets from for e.g. microwave observations then either skip them or try to just explain them. I do not suggest to compare data from different sensors as we know in advance that for example microwave data just detect deep convective clouds but not thin clouds and IR data are saturated under thin clouds. That is why they underestimate IWP compared to cloudsat. Please be aware that CloudSat gives the IWC profile from the ground to the top, at least to some extents, but other sensors just detect a small part of ice water path.

3.The structure of the article does not fit to standard styles. Everywhere results and methodology are mixed and there are a lot of statements without any reference. It seems that they are putting their understandings without giving any references. If something is not obvious then you have to either prove it or give a good reference.

I have some specific comments which are repeated through the article. I would be happy to re-read this article after new submission.

Specific comments 4.Introduction Ice clouds are an important part of the earth's climate system. Knowledge of the distribution and properties of ice clouds is central to understanding the atmospheric water budget, as their distribution strongly affects precipitation and the water cycle. Ice clouds also have 5 a strong effect on the radiation budget of the atmosphere. They cool the atmosphere by reflecting incoming solar radiation, but also heat the atmosphere by absorbing and re-emitting outgoing terrestrial radiation. The magnitude of both processes, hence the net sum radiative impact of ice clouds, depends on macro-physical properties such as cloud top temperature, ice particle density, and vertical and horizontal extent, and on micro-physical properties such as ice crystal shape (Ramanathan et al., 1989). You have one reference for the whole paragraph. Has everything been mentioned in (Ramanathan et al., 1989). If so, why

C5133

are you copying such a big part from one old reference. If not, then you have to add references for different statements. As you have shown neither the importance nor the impact of the distribution of ice clouds on precipitation, therefore you need to put some good references here.

2.second paragraph, you have to add references about the importance of in situ data and etc. This has to be done everywhere you mention something which is not clear.

3."Models make a clear distinction between precipitating-ice and cloud-ice, whereas observed IWP will contain a mixture of both. This must be taken into account in comparison studies such as this one. " Reference? 5.Page 3 Line 15 please add reference for the following explanation? "The lack of adequate and abundant IWP measurements is the main problem in constraining climate models." 6.Remove figure and all other comparison figures. These figures are misleading and would cause mis-understanding. Do not try to make comparison between different datasets, instead try to explain them and their nature. 7.I completely understand that you have done a lot of works and calculation for this comparison but if you put such comparison figures then this would be a challenge for people who are not familiar with the sensors.