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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of cloud fraction and its radiative effect simulated by IPCC AR4 global models against ARM surface observations" *by* Y. Qian et al.

Anonymous Referee #2

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Review of paper: Evaluation of cloud fraction and its radiative effect simulated by IPCC AR4 global models against ARM surface observations.

This study evaluates the representation cloud fraction and related radiative effects in a number of climate models used in the IPCC fourth assessment report (AR4). Clouds pose a major source of uncertainty in climate models and their projections of future climate evolution, and therefore a comprehensive assessment of the ability of the models to reproduce clouds under present day conditions is a worthwhile (and necessary) endeavor. While many studies use satellite data to assess clouds in climate models, here the authors focus on surface based cloud observations, by including 3 observa-

tion sits from ARM in different climate regimes with comprehensive instrumentation. The authors make extensive use of the information on cloud characteristics available at the ARM sites compiled by 3 different observational methods. The comparison of these different methods as done by the authors is useful as it gives a good idea on the uncertainty in cloud fraction estimates as seen from the surface observation. I would have liked if also the classical synop observations of cloud fraction by human observers could have been added, as this is by far the most widespread and abundant information on clouds from the surface. In case such observations would be available at the ARM site, it would be interesting to get an idea how they perform compared to these more sophisticated methods, as a means to better interpret the many studies that have used synop cloud data before.

On the modeling side of course one can argue that the models used in this study from the 4th assessment report are by now becoming somewhat outdated, reflecting the state of climate modeling at least 5-10 years ago, which somewhat limits the usefulness of this study. But as we experience such delays with the runs for the 5th IPCC assessment report, which are only now gradually becoming available, we can expect publications based on the 4th IPCC report models still to be published for a while. While I think it is too much to ask the authors to repeat their study with the new CMIP5/IPCC AR5 models for the present paper, I still would encourage them to do so as soon as a comprehensive set of model simulations will become available, to provide an assessment of more up to date models, which may then have a more immediate impact on model development. If already now the authors would be in a position to make any statements with respect to the applicability of their conclusions to the AR5 models, this would enhance the visibility of this study.

Specific comments:

p14939, L4. “more than a dozen”. I would say it is rather “two dozen GCMs” that provide data to PCMDI. All of them provide cloud fraction and all sky solar fluxes, most of them also clear sky solar fluxes. Therefore I slightly wonder why the authors only

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used 11 GCMs.

P14940, L21 SkyRad, GndRad, and QCRad are not generally known acronyms and should be explained.

Section 2.2: The observational methods used in this study could benefit form a more elaborate description, e.g. the TSI method is not explained at all.

Section 4: How did you compare the gridded simulated fields with the point observations: e.g., taking the surrounding gridpoints, or the nearest gridpoint? For example at the coastal site Barrow, has it been ensured that only simulated land points are taken into account? Manus is a pure ocean point in the model, while observed cloud formation may be affected by the island. Is this taken into account in the comparison?

P14947 L27ff: why is this comparison only done for Manus, it would be interesting to have similar information from the other sites.

There are a number of figures which could profit from a redesign:

Figure 1: Horizontal Axis (years) is not readable

Figure 3: Vertical axis should not exceed one, which would also allow to better differentiate between the different models.

Figure 4: Why not put all 3 methods into one figure. The values for TSK are repetitive in the top and bottom figure.

Figure 6: The curve representing the GCM means is not well discernible, it could be represented as a thick line for example. Again the vertical axis should only go to one, which would allow a better separation of the different GCM results. Legends may then be outside the figure.

Figure 7: Why is only one type of observational method (TSK) shown here?

Figure 9: Figure caption not detailed enough, it should be explained what is shown in

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the left and right panel, respectively. The number of years that went into the averages should also be mentioned. This applies also to some of the other figures.

Typos/small changes:

P14935 L25 should be clouds, similarly P14936 L1 Clouds are also. . .

P14937 L19 replace “difficulty” by “impossibility”

P14940 L7 clouds and their radiative forcings

P14945 L 14, should be (Fig. 3, middle), or add a), b) c) into the Figure, if you refer to them in this way.

P 14948 L2: one of TWP: something missing

P 14948 L10 Fig. 5a should be 5 (top), or add a) and b) into the Figure, if you refer to them in this way.

P14949 L8, should be Fig. 5, not 4, and bottom, not a)

P14951 L16: “too frequent overprediction”, is too much. Better “too frequent large cloud cover”

P14951 L25 has a similar

P14958 L21 lower levels

P14959 L20, should be Fig. 14, not 13.

P14961 L 7, with “transient” do you mean “daily”?

P14962 L10 near the surface

P14963 L12 should be “from the 1990 at most of the sites”

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 14933, 2011.