Atmos. Chem. Phys. Discuss., 11, C5517–C5524, 2011
www.atmos-chem-phys-discuss.net/11/C5517/2011/
© Author(s) 2011. This work is distributed under
the Creative Commons Attribute 3.0 License.

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 #1

Received and published: 27 June 2011

documentclass[11pt]article fullpage,times [pdftex]graphicx

General comments

This manuscript presents cloud fraction estimates from ARM surface observations and uses these to evaluate the monthly and annual cloud fraction, as well as the shortwave radiation at the surface, predicted by the AR4 global models. Doing this evaluation, for three point locations that have very different patterns of cloudiness, is worthy of publication and an important step towards making use of the wealth of observations available to learn about errors made in global models. There is a rich amount of results

in the manuscript that could be very valuable for both the modeling and observation community. However, the manuscript misses to clearly outline important details of the methods used that may significantly impact the evaluation and conclusions reached. It also suffers from not being organized in a concise manner, and therefore fails to bring a clear message across. I recommend major revision based on the below comments.

1. Except for one paragraph in the last section, the paper does not describe how the cloud fraction profiles from the observations are constructed. The comparison of cloud fraction profiles from the models and the observations however is not at all trivial, and if the authors have thought carefully about the impact of vertical grid and temporal grid, they should discuss this explicitly earlier in the paper. In doing such a comparison, the observations should be mapped onto the same vertical grid as used by the models, at least. Otherwise it appears one cannot use Figures 9 - 14 to support statements in the text regarding whether models producing more, or less clouds, than observed at a given altitude. It is highly recommended to re-do these profiles as to allow a fairer comparison (which would greatly increase the value of the manuscript), or, the authors should very clearly highlight which pieces of information can in fact be derived from their comparison, given all the uncertainties discussed in the last section, and which are unique and new compared to previous model-satellite comparisons of cloudiness.

2. The paper presents detailed background information on the prevalent cloudiness patterns (and the dominating dynamics) at each of the three sites (in section 5.1, 5.2, 5.3). It is recommended that this information is presented earlier and connected to figures that present the observations. Knowing the different cloud patterns, why can one expect one measurement of cloudiness being better than another, at a given site? If three different observations provide very different (or very similar) estimates of cloudiness, does that relate to the specific clouds being present or their variability, and based on that, what would one like to evaluate from the model? Presenting and discussing the differences in cloudiness at these

three locations, and their impact on the accuracy of the measurements, could be used better to motivate the evaluation of say, a seasonal cycle, or a vertical profile of cloud fraction. For this it would be very good to add the results for SGP and NSA to Figures 2, 4 and 5 and bring out the (dis)similarities.

3. Different pieces of text, being either motivating comments, description of the methodology, background information or important results, are scattered throughout the document. However, relevant information on how cloud fraction profiles are constructed, or on why only a certain measurement is used to evaluate the models, should not be mentioned only once the reader gets to the conclusions. Also, motivating comments can be mentioned once in the introduction, but are distracting if they appear at the beginning or end of each paragraph in the remainder of the text. Including these pieces of text in their respective sections would make the manuscripts much more clear and concise.

Specific comments

1. P. 14936 line 16-20: What new light does this study shed on the findings in these cited papers (i.e., that total cloud fraction is better predicted in global models than the vertical profiles of cloud fraction)? Can you come back to this in the discussion?

2. P.14938 line 7: Which definition of cloud fraction would hence lend itself more naturally to compare with the modeled cloud fraction? (hence later in the paper: why is the hemispheric cloud fraction TSK used in the comparison with the monthly total cloud fraction?)

3. P.14939 line 10: To be consistent you removed some models from the pool that have missing radiation fluxes of vertical profiles, so why do the 11 models listed in Table 1 still have these missing variables? which ones are removed?

4. P.14939 lines 20-25: Here would be a good point to mention how CF and total cloud cover in the global models are calculated, this aspect is critical to your study and should not just be referred to by giving the link to the documentation. What are the cloud overlap assumptions? What is the time period over which cloud fraction is averaged?

5. Section 2 and 3: The limitations of each measurement, and what may or may not be usefully compared to the models, should be clearly discussed here (that is, before the conclusions). If not, one gives the impression that after all, the comparison of cloud fraction from the models and the observations is not useful. It would be very good to provide estimates of the contribution of rain particles to cloud fraction, as well as being clearer about the different sensitivities for detecting upper level cloudiness (for the ARSCL product) in a more quantitative matter if possible.

By giving the different products or instruments a subsection (with a separate header) this section as a whole would read much better. Some more detail on how cloud fraction is derived from the Total Sky Imager and how vertical profiles of cloud fraction are derived from the ARSCL product would be good.

6. P.14941 lines 16 and 22-23: It is not clear how much precipitation has affected the results - can you give an estimate of how much of the rain influence is reduced with and without the ceilometer/lidar screening? For light rain events, such a screening might work well, because the ceilometer/lidar is still able to see cloud base and not rain, and this can be used to judge whether a cloud is overhead or not. For heavier rain, this becomes a more difficult task.

7. P.14942: The first paragraph (lines 1-17) might be better written along with paragraph 2.2 and can be more concise. What is the temporal resolution of the other instruments?

8. P. 14943 and 14944: The authors use a 1 hour over which to derive cloud fraction (is that true for the profiles as well?) from the different measurements. As discussed in the text, one hour is short and leads to more frequent occurrences of cloud fractions being either zero (not shown) or one, for the narrow FOV ARSCL product (see also Boers et al. (2010) - JGR, Vol 115, D24116.). Does this period represent a spatial scale equivalent to the model grid box? As mentioned, as long as the one hour cloud fractions are averaged over long enough periods (a month, a year) they converge to other estimates of cloudiness, but the 1 hour cloud fractions reflect different spatial scales (as a result of different wind speeds), so it is not trivial that averaging 1 hour cloud fractions to a daily estimate, or a monthly estimate, would give the same results as when cloud fraction is determined over six hours, one day (a month). It would be good to think this through.

9. P.14943 lines 5 - 10: Here the influence of wind speed on biases between the hemispheric and FOV CF estimates is discussed. First, can you plot the wind

speed versus the biases (on a daily, 6 hourly, hourly basis)? It is very hard to see from Figure 1 that periods of low wind speeds correspond to larger biases, and it is not clear how much these biases reduce when moving from hourly to 6-hourly values, or 12 hourly (daytime) values. Second, periods with different wind speeds might correspond to periods with different cloudiness. May part of what you are seeing relate to the difficulty of measuring certain types of cloudiness that are predominantly present during periods of certain wind speeds?

10. P. 14943 lines 11-29: Why does the TSK have much larger day to day variability (see 1 - 11 May 2006 in Fig.1) than the TSI? Does a certain instrument perform better at one site, because of the dominant cloudiness pattern? Is ARSCL TCF higher than TSI TCF over Manus because of frequent occurrence of high - level cloud?

11. P.14945: Would the discussion of daily value histograms fit better before the discussion of the monthly mean values (annual cycle)? Why does Figure 4 not show TSI versus ARSCL? Is TSK overestimating in partly cloudy conditions or times with low sun elevation? It would be valuable to show ARSCL, TSI and TSK in the same PDF and make the PDF for Manus, SGP and NSA.

12. P14947, line 21 - 22: Are there some missing references here, i.e., are you the first to show this?

13. P14948, line 1: Here you choose to do the comparison of the models' TCF with the TCF derived from the TSK observations: why? Is it just because you have longer records of TSK and because they are compatible with the surface radiation flux data? Why not ARSCL?

14. P14949: Errors in TCF and SW are due to errors in cloud overlap assumptions too, this should be discussed much earlier, when introducing how cloud fractions are determined for the global models.

15. P14951 line 3-4: This is an example of a sentence that has been frequently repeated and could be omitted.

16. P14951 line 6: If you take out the cnf hires GCM then the Arctic site looks just as diverse (or even less diverse) as SGP and Manus.

17. P14952 line 4: In terms of the distribution, but not in their monthly mean (Fig 6b).

18. P14956 lines 16-17: This should have been mentioned much earlier when comparing the different estimates of TCF derived from the observations.

19. P14956: line 26: If you would map cloud fractions onto the same vertical grid, what differences remain?

20. P14957 lines 20-21: This cannot be argued from the model-measurement difference (that is larger at higher altitudes) because the ARSCL may be less sensitive to seeing high cloud and you are using a different vertical grid.

21. P 14963 lines 18-29: Evaluating the models according to their cloud scheme, and cloud overlap assumptions, has not been systematically done in this paper. If this was the original goal, what part of it has been achieved? What can you conclude that was not known before?

Typo's/errors/graphics

1. P.14938 line 1: between themselves or among themselves?

2. P.14945 line 15: seasonable should be seasonal

3. P14950 line 12: is 'potentially' at the right place here?

4. P14957 lines 18: 'which is the height that the' should read 'which is the height at which' or 'which is the height where'.

5. P14959 line 11: reveals should be reveal

6. Graphics: Many figures show cloud fractions up to 1. 5. We know it cannot exceed 1, so why not make the y-axis go to 1. and include the labels outside of the main figure? For Figure 2, the x- and y-axis should have the same length (scale). For Figure 6: it is hard to distinguish the TSK (SW) line against the background grid, the same is true for the GCM's mean.