

Interactive comment on “Total cloud cover from satellite observations and climate models” by P. Probst et al.

Anonymous Referee #1

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The paper compares CMIP3 model produced monthly mean climatologies of total cloud cover against similar satellite derived measurements. Global annual means, averages for large latitude bands, specific seasons, and the strength of the seasonal cycle are investigated. It is found that most models underestimate total cloud cover by about 10-15% for the global annual mean, with a large intermodel spread. This bias is most pronounced in subtropical and higher latitudes, and to a lesser extent in the inner tropics. However, the models reproduce quite well the zonally varying structure of cloud cover and its typical seasonal cycle. It is also found that the interannual variability of total cloud cover is underestimated by the models.

I do not find that the analysis and results presented in the paper are particularly new or interesting. It is well known that the simulation of clouds is a major task for any type

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of model, and I am actually quite surprised that the mean model bias amounts only to 10-15%. In addition, the main results of the paper, that the CMIP3 (and other) climate models tend to underestimate both total cloud cover and its interannual variability, have already been shown before (Pincus et al. 2007, Figure 2). The amount of new information presented is small and the shown analysis is quite simple, which taken together does not justify publication in a “stand-alone” paper. I also find that some of the interpretations and conclusions of the paper are overly strong, for example that “the models feature a remarkable negative bias” or that “models seriously underestimate interannual variability”. Also, some of the explanations and interpretations given in the paper are common text book knowledge (e.g., that baroclinicity and cyclogenesis is stronger in winter) and should only be mentioned briefly in a scientific paper. For these reasons I do not recommend the publication of the paper in ACP.

I also have a number of more technical comments. For example, it is mentioned in section two that the ISCCP data have difficulties with overlapping clouds, but does this really matter for the present paper? Also, only one observational data set is used in the study, making it impossible to judge how large the observational error is. However, this study is on the very specific and isolated topic of total cloud cover, and one can expect that more attention is devoted to this issue. In addition, the paper does not discuss the problem of defining of what actually defines a cloud. It is very likely that models and observations use different criteria and thresholds for their total cloud cover definition, making it likely that some of the shown discrepancies are related to this lack of a common definition. The paper also mentions several times that there is little consistency amongst errors from different models, and this behavior is interpreted as a negative aspect of model performance. I actually would interpret this discrepancy in the opposite way, since this shows that models are more or less statistically independent and that model errors cancel each other to some extent in the multi-model mean. I also find that the introduction and methodology section of the paper are unnecessarily long and detailed (e.g., Table 1 has already been shown in a countless number of previous papers), in particular compared to the little amount of information given in the result

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section. Similar comments hold for the conclusion section, which mainly constitutes a long summary of the results. The paper also contains a number of (minor) errors in the English language.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 21023, 2010.

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