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Geoscientific Model Development Discussions

Interactive comment on "GTS v1.0: A Macrophysics Scheme for Climate Models Based on a Probability Density Function" *by* Chein-Jung Shiu et al.

Anonymous Referee #2

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This paper describes a new macrophysics scheme based on PDF and then implements it in CAM5. Two PDF distributions, uniform, and triangular PDF, are tested and compared with the default Park scheme, which uses the triangular distribution. They found the new scheme could simulate clouds and other atmospheric variables better than the default Park scheme. In that, the uniform PDF overall outperforms triangular PDF in many variables. I would recommend publishing this draft after the major revisions listed below.

Major comments:

1. Overall paper organization

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Discussion paper

1.1 The paper is easy to be followed, but the whole organization could be further improved. I would strongly recommend highlighting the most significant effect of the new scheme on simulations, rather than presenting every perspective of the simulation by comparing with observation with an improved RMSE or correlation. For example, although clouds and radiation both are improved, it is deserved to have more discussion about the reason behind. Another concern is large amounts of tables (Table 1-9) in the main context. Although it is nice to provide overly quantitative results, it might be better to put only the most related tables in the main context and move others in the supplementary.

2. I feel the method part might be not effective for others who want to understand and reproduce the method in-depth. Some other details need to be clarified. Therefore, I suggest the authors to include more details in their method part. Some specific questions are as follows:

2.1 Equation (2), (5) and (6) give the relationship between CF/cloud water and grid mean water condensate and width of the PDF. I cannot exactly follow how the width of PDF is determined based on qs, qt and ql. For the triangular PDF part, two equations (5) and (6) with two unknown variables could be solved. This is what I can derive from your method. However, equation (2) for CF includes two unknown variables: CF and the width of PDF. If RHc is eliminated, how the weight of PDF is obtained? Maybe you also need to give the equation of cloud water under the uniform assumption. Please clarify your derivation more clearly.

2.2 Moist parameterizations in CAM5 include both convective and stratiform clouds, and they are handled by convection and cloud macrophysics, separately. If my understanding is correct, your new scheme only takes effect on the stratiform cloud fraction. So, the cloud condensate is still handled by the default prognostic cloud condensate scheme, right? My question is: if the new scheme can obtain the good consistency between cloud fraction and condensate, does it still need the consistency check between cloud fraction and condensate to avoid “empty” or “dense” clouds? Please give more discussion about this in the method part.

2.3 L176: should add one bar for qt because it is a grid-mean value.

2.4 L212-214: "RHc is still required when clouds start to form from a clear region" – What kind of RHc distribution is used for starting the cloud formation? Does it use the same RHc varying with height (low, middle, and high) in the default Park scheme? Please clarify it. A more general question is: will the results be sensitive to the initial given RHc?

3. Some specific questions for results

3.1 I think one of the most important arguments in this paper is the "stronger" relationship between CF and RH while using the GTS scheme than the default Park scheme. However, from the observation signal (Figure 4a, Xie et al), it looks like there is no quite a strong relationship between cloud fraction and RH. My question is: if obs does not show such a feature, why do we need to get this strong relationship in the model? Like Figure 4.

3.2 Radiation balance after using GTS scheme: the radiation balance is quite important for a climate model, and the TOA radiation is very sensitive to modified cloud-related processes. Will the introduced new scheme strongly affect the TOA radiation imbalance? It is necessary to discuss the TOA radiation balance after using the new scheme and overall model climate. Can the imbalance be tuned by other parameters? Will the improvement be "reduced" while reaching a radiation balance?

3.3 Single-column tests:

3.3.1 L327-330: how about the results from U_pdf if it gives a better performance in the offline test?

3.3.2 how do you calculate the correlation between CF and RH in Figure 3? first vertical-integrated CF and RH to get two time series and then correlate them? Please clarify it.

3.3.3 I think there are observation data of cloud fraction and relative humidity for this site. Except for comparing with the default cloud scheme, it will be more informative to compare with observation data. Meanwhile, checking the correlation between CF and RH in observation will give a good reference about which scheme performs best. Furthermore, the observation data could also give a correlation reference between CF and RH.

3.3.4 GTS-Triangular tends to overestimate RH when the cloud fraction is small (Figure 4c). many points are below 40%, and this feature is not shown in observation. why?

3.4 L396-397: why U_pdf results in larger RMSE in JJA and does not perform well? it might be related to a large amount of cloud ice at the upper level in the Arctic in Figure 6b. do you know why there is a second peak of cloud fraction at the upper level of the Arctic? It looks like some "false" clouds are formed at that high level in the GTS scheme. I think more discussions about this bias are necessary since it is highly related to one of the key points in this paper – ice cloud fraction is also calculated by PDF scheme. If it brings such bias, to what extent we should adopt this scheme, especially in the high latitudes?

3.5 Figure 8: I think the scatter plot does not show a great linear correlation between CF and RH, more like exponentials. I am not sure whether I should agree with the author's "higher" linear correlation efficient to evaluate good or bad relationships.

3.6 this paper gives out many statistical results for evaluation, but the reasons behind and the connection between modified clouds and change of other related variables are discussed less. I recommend the authors to add more discussion about this.

3.7 How sensitive the final model's performance to the tuning parameter – sup? From Figure S4, this parameter could reduce the cloud fraction up to 20%, and this will bring a substantial effect on radiation. How the overall model performance will be? Maybe a Taylor plot with different sensitivity simulations could give a good evaluation.

Minor comments:

1. Figure 2: U_pdf performs better at a lower level and has a similar performance with T_pdf at the upper level. what might be the potential reason?

2. L263: in section 3.1, please include the references for each observation data you used and the time period used.

3. L277-279: do you use another CF and CWC data here? It is slightly confusing. I suggest reorganizing this paragraph.

4. L 279: "The methodology from Li et al. (2012) is used to generate gridded data". Are there any specifications for this method to generate gridded data? It might be better to briefly describe the method used here and the reader would not take more time to read the referred paper to figure out what the method is.

5. L388-390: "on the other hand, ..." This sentence is confusing. Please consider rephrasing it.

6. Figure 17: what contributes to the stronger DTCOND in U_pdf compared to T_pdf below 700 hPa? The authors might want to add more discussions about it.

Technical:

1. Figures: labels in some figures are too small. Please increase the font size.

2. Figure 9, the lat-lon plot is also too small. Please consider another type of layout for this figure.

3. Figure 17: add the unit of each variable

4. Please consider add legends for line plots.

5. Figure S4: if my understanding is correct, the red solid lines in the upper panel and the lower panel should be from the default Park. However, it looks like they are not the same. Please check it.

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