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Interactive comment on "Cloud macroscopic organization: order emerging from randomness" *by* T. Yuan

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This paper presents empirical distributions of cloud sizes and proposes a simple stochastic cloud model to describe the scale-invariant properties of the size distributions. The empirical cloud size distributions based on satellite data are not new, as noted in the manuscript. Many other papers have covered this ground. The more novel aspects of the empirical part of the paper include the notion that clear areas are similarly distributed and that the cloud scale distribution is robust to large inter-annual variability in mean cloud cover. Admirably, the author attempts to explain how the stochastic cloud model might be useful for deterministic models of clouds and climate. This is a missing element in the literature. For all the papers that explore these scaling properties of clouds and propose models to explain them, this information has not had

much of any impact on the development of global models or the interpretation of detailed mechanistic cloud models. The model proposed in the paper may have promise, but the paper requires substantially greater description and justification to prove useful. Aspects of the paper that I feel need greater development are discussed further below.

The term "self-organization" is invoked throughout the paper. Given the lack of penetration of these concepts into the cloud-climate modeling community, I think this concept should be defined early in the paper, and the paper should explain why the cloud statistics are an example of self-organization and not organized by some external property of the atmosphere, or disorganized, for that matter. P.1109, line 4 is an example of an area of this discussion that should be clarified. There should be literature (on clouds specifically, or the concept of self-organization generally) available to shore up this part of the paper.

The paper suggests that inter-annual variability of gamma have been reported for other regions (p.1109, lines 9-10). This seems to contradict earlier discussion that suggests the prior studies were limited by small sampling statistics (p.1108, lines 18-20). For the empirical part of this paper to be considered publishable, it should clearly establish how the observed statistics improve upon the previous literature.

Similarly, the discussion of the land/ocean and diurnal variations needs clarification (P.1109, lines 10-17). This section implies that the land/ocean and diurnal differences are important, but the caption for figure 1 simply says that they are small. Please clarify and expand not only on whether these differences are significant, and also how they are apparent in figure 1. Are the diurnal differences you mention merely morning/afternoon differences between daytime Terra/Aqua retrievals?

A model is proposed based on two rules for organization of cloud fields. Such models can be useful if the rules can successfully be argued to be reasonable abstractions of the true dynamics of the system and the model exhibits properties that are similar to observed properties of the true system.

A physical argument with references is provided for “clumping” of clouds, but not the merging of clouds. Do the two references associated with merging provide physical arguments for this dynamics? Do clouds only merge when one or more clouds expands to reach the boundary of an adjacent cloud? Or are there other mechanisms?

I might be able to better understand how this dynamics is represented in the model if some of the terminology were better defined. Are edges merely connections between vertices? Is the degree of a vertex the number of edges intersecting a vertex? This is my interpretation, but these basics of graph theory need to be clarified. Can a diagram be developed to visualize the vertices and edges? That would be helpful.

Please revise: “Redundant edges with common neighbors of the merged vertices are also removed” (p.1110, line 9-10). I cannot understand what is meant here.

The discussion of the model should describe the parameter space. How were the values for the parameters chosen? How could one constrain them with available observations? How sensitive is gamma to the choice within reasonable bounds? Does the power-law behavior break down for certain parameter choices?

I think the case for the utility of a simple stochastic model to the more common reductionist modeling approaches needs a bit more development. I think the author is heading in an interesting direction with “our stochastic model can effectively produce the regional variation of gamma” (p.1112, line 26). But this is not demonstrated, nor is it clearly described what controls variations in the free parameters of the model. Are these entirely controlled by microphysical properties - a mechanism suggested a few sentences later? Or are there aspects of the large-scale flow or thermodynamics of the cloud environment that adjust these parameters? My sense is that the answers to questions such as these will more clearly illustrate the utility of the model.

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