I would like to first thank both reviewers for carefully reviewing my manuscript and for providing helpful comments to improve the manuscript. I believe the quality of the manuscript will be improved after addressing the reviewers' comments and suggestions.

Both reviewers expressed some common concerns on a few aspects of my manuscript, including the presentation of methodology, discussion of some results and the need for clarification on the model set-up. I addressed these suggestions and comments by significantly revising my manuscript. The major changes will include a clear definition of the notion of 'self-organization', a diagram illustrating the working mechanism of the graph, clarification of rules for the graph model, restructuring of the flow of the manuscript and more discussions on topics suggested by the reviewers. Please refer to the revised manuscript for details.

Detailed responses to the reviewers' comments are provided below in a point-by-point fashion.

Response to reviewer 1:

The manuscript by T. Yuan covers an immensely important topic, namely the formation of cloud size spectra of low-level warm clouds. Basically organization of cloud sizes follows a power law – a result that per se is not new. The method used is based on graph theory, a well established mathematical methodology allowing to handle random organization of individual elements by well defined rules. The study also benefits from a very large sample of cloud size information over different geographical locations and for several years. Results show dependence of the cloud size spectra on geophysical conditions, but much less so on interannual variability in these settings. While these facts are much appreciated, it seems that nevertheless the manuscript needs truly major revision (or better resubmission) before it could be possibly published. This concerns the description of the methodology as well of the results and discussion. Especially, there is lacking discussion on the physical reasons of geographically varying power law exponents. What are the main determinants for these differences? A complete rewrite of the main parts of the manuscript seems to be necessary in order to present a convincing strain of arguments for the application of the method used to determine cloud size spectra from rules based on Graph theory to naturally occurring cloud fields.

Response:
I thank the reviewer for good suggestions. The manuscript is revised with substantial changes made to the sections as suggested by the reviewer. In addition, I would like to note that the intent of this manuscript is to briefly outline a new perspective to understand the observed macro-organization of clouds and to demonstrate the validity of this perspective with a network model. The question raised by the reviewer on the physical reasons of varying power law exponents is an excellent one. I could toss some speculation on this subject. However, this knowledge is a prerequisite for the statistical model construct in my manuscript to correctly simulate the spectra change. This knowledge would require investigations from observations and convectional physical models. This knowledge also will likely to improve the statistical model to account for missing mechanisms. That is the point I tried to make in the discussion section on the

interplay between physical models/measurements and the statistical model. It is also my hope that the observations and discussions in my manuscript could arouse future investigations to address the reviewer's excellent question.

In the following I will comment on the manuscript in more detail and I will follow the flow of text rather than separating between methodology and results. I am not commenting in detail on the numerous language deficiencies!

Data and Method: P1107: L 16: Sk is not defined L 19: why are diagonal neighbors ignored? L 21: The sentence star ting "For every level-2 ...." is hard to understand L25: Discuss the effects of cutting off scales less than 3km! That causes omission of a large number of cloud elements. How much of spectrum information is going to be missed by this approach? P 1108: L 4+: What is the exact algorithm of the "computer program"? How are non-liquid cloudy pixels defined? This is not adequately defined in the SOM (and actually is too important to be hidden there!)

Response:
Definition of S_k is now provided. It is the width of a specific size bin. The choice of neighbor definition does not affect the result presented here. This is also shown in previous studies referred in the manuscript. I changed the sentence to "For every MODIS granule we find the locations of each cloud patch and keep only those that stay within the area bounded by 5N and 30N and 170W and 155W. We include only pixels that are confidently cloudy by the MODIS quality assurance flag (Platnick et al., 2003).". We made the choice of omitting clouds smaller than 3km because of two reasons. The critical reason is to avoid dealing with effects of partially cloudy pixels and we are only interested in larger clouds anyway. The statistics on smaller clouds have been reported in previous studies. The second reason is computational. Including all cloud sizes dramatically slows down the calculation. However, the tests done to several granules indicate the power law holds if we remove the cut off. In other words, besides omitting the smaller clouds the cut-off does not affect our conclusions.

The algorithm is a standard two-pass algorithm used to label connected component in an image. For details, please refer to, for example, Shapiro, L., and Stockman, G. (2002). *Computer Vision*. Prentice Hall. If it is necessary a description of the algorithm can be provided.

Results: L 10: "The scale-free..." The power law relationship is not defined before. Probably the relationship from Fig. 1 is meant, but that needs to be defined mathematically. Re-arrange sentences here. A correlation of log/log relationships is not very meaningful as it covers magnitudes rather than actual values. L 19: "... because they..." Who is meant here? Quotations missing! Since the "break" in scale is an important issue, more discussion is needed here. L 26: "...strong yearly variation" What is meant here? Annual (i.e. within the annual cycle) or interannual?

Response:
I agree with the reviewer and a definition is provided. I also agree with the reviewer that

the correlation is not very meaningful in any sense other than indicating the goodness of fit to the power law. The numbers are just put there for reference. The text is revised to reflect this point.

The sentence on L19 is now changed to "The break reported in previous studies is probably due to insufficient sampling of larger clouds (recall that $P_k \sim K^{-\gamma}$) because only a few cloudy scenes with each covering an area ~10,000km^2 or less are used in these studies (see Zhao and Di Girolamo, 2007)."

Here I am referring to interannual variability. The wording is changed accordingly.

P1109: L 1+: If I understood right that means whenever clouds develop they create a certain size spectrum independent of how many individual clouds are there and what the cloud coverage is. But should that not be the case when normalized cloud numbers are being used anyway? Self-organization does not necessarily mean constant organization, i.e. following a constant power law. Are there other mechanisms than just randomized merging and separation in play? L 5: "from other regions" Here some literature should be quoted! L 14: I cannot see the mentioned differences from Fig 1. I suggest discussion of the exponents for different conditions, including a statistical analysis of potential differences. L 16+: What is the meaning of "(it is transition season ... over Amazon)"?

Response:
The reviewer's understanding is correct. However, normalization does not guarantee the shape of the distribution to be the same as the reviewer pointed out. This similar or constant organization patterns is not a necessary condition of self-organization. I have changed this part of discussion to better reflect this point.
L5: reference is added.
L14: the sentence is changed to "Finally, the diurnal variation in γ_{eff} has a land-ocean contrast: the variation is larger over land [Figure 2a] than over ocean [Figure 1]" to remove the confusion.
L16: It is changed to "...it is opposite over land (e.g. over the Amazon during September).

Model: P 1109/1110: What is meant as a Model, seems to be rules following GRAPH Theory. It will be necessary to introduce these rules in a more understandable and systematic way since it is not a usual tool in atmospheric sciences. The whole paragraph needs to be rewritten showing how Graph theory rules would match the processes observed. In principle most of the information is there, but the sequence of arguments is not adequately substantiated and organized. It remains unclear on which information the first guess ("few vertices") is based upon.

Response:
The reviewer's suggestion is very good and I have changed the text and added a diagram (Figure 1 in this document) to explain the rules of the network (or, graph). The few vertices at the beginning are put there without any edges and the initial configuration

does not affect the final result, which is a property of the self-organization. This choice is arbitrary and it is only to spin up the evolution of the graph.

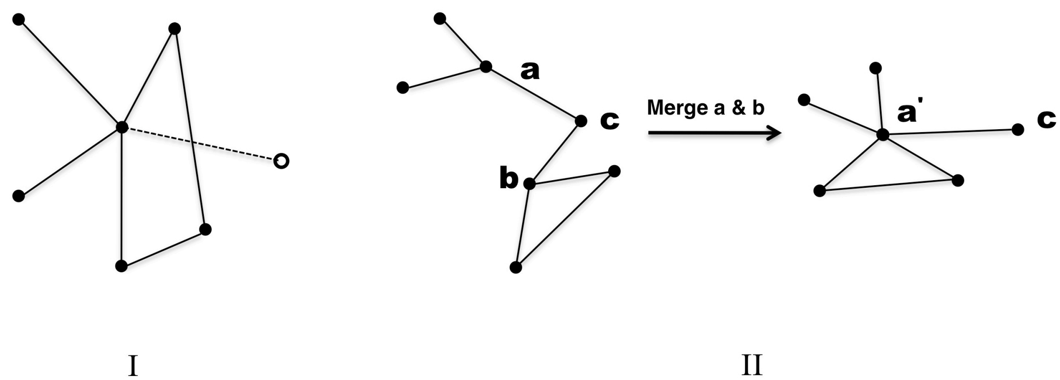

Figure 1: Illustration of attachment in I), where a new vertex is added to the graph and it is connected to an old vertex by an edge and merging in II), where vertices a and b are merged into vertex a'; any edge between a and b would been removed; a and b have a comment neighbor, c and after the merging only one edge remains between a' and c.

P 1111: The first and second paragraph on this page reads more like an introduction and might be moved to the beginning of the model section. Then be followed by introducing Graph theory as a means to simulate the random interactions of individual elements. As it is now, the logic of the method remains quite unclear.

Response:
Some rearrangements are made along the lines of what is suggested by the reviewer.

Discussion: The confusion of the reader continues in this part. The logic would require discussion to what degree the proposed method based on Graph theory can mimic observed (and such simulated in explicit modeling) processes, not introducing more complications. The last part of the last sentence of the manuscript is certainly true:" ... this study barely scratches the surface."

Response:
Here I am trying to convey the idea that due to the abstract construct of the model it can be used to study other phenomena in atmospheric science. In the revised manuscript I tried to clarify the connection among these different phenomena.

SOM: What is the meaning of negative frequencies shown in the one figure included in SOM?

Response: It is the logarithm of the frequency.

Response to reviewer 2

This paper presents empirical distributions of cloud sizes and proposes a simple stochastic cloud model to describe the scale-invariant proper ties 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 par t 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 proper ties 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.

Response:
The reviewer's (Dr. Wilcox) comments give a rather good summary of what I want to convey in the manuscript. I also want to emphasize the point that the manuscript is only attempting to give a new, potentially viable perspective on the cloud organization. The full potential and evaluation of the perspective and method proposed here would require much more investigations and are beyond the scope of current work. Investigation is underway to address some of the unanswered questions.

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.

Response:
Giving a definition of self-organization is an excellent suggestion. It will facilitate many discussions in the manuscript and clarify confusing parts of the manuscript. I do not know if there is one precise definition of 'self-organization' that fits everything that is labeled so. The general idea of self-organization is, however, as the following: a system shows emerging order or structure on its own without any external involvement. In other words, in such systems the system-wide, emerging order, pattern or structure result automatically from micro-scale interactions among internal components, and usually the interactions are independent of the physical nature of these components.

The small sampling issue is not contradictory to reported seasonal variation. The small sampling is referring only to larger clouds and ultimately related to the ratio between sampling area (e.g. swath width of satellite data) and the spatial resolution. With the ratio not large enough large clouds may never be amply sampled to give a correct spectra due to cut-off at the edge and other reasons, which typically result in 'scale break'. I will clarify this point and state more clearly what is new in this work.

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?

Response:
Yes. The diurnal difference is only referring to Terra/Aqua retrievals. I also took Dr. Wilcox's suggestion and clarify and expand some discussions on the diurnal difference and the land/ocean contrast in this difference in the revised manuscript.

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 proper ties that are similar to observed proper ties 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?

Response:
The references provide mechanisms how clouds might merge. The clouds might also merger when downdrafts of trade cumulus create cool pool and generate new clouds. Mesoscale convergence can also lead to merging of clouds as demonstrated in Tao and Simpson (1984). I feel the rules of proposed model in this work can effectively mimic the important natural processes of clumping and merging. More importantly, this work wants to show that these interactions, once implemented, do lead to self-organization behavior.

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.

Response:
I take the reviewer's suggestion and provide a diagram (see Figure 1 above) in the

revised manuscript to clarify the mechanics and terminology used.

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.

Response:
I changed it to "if a pair of merged vertices has common neighbors the redundant edges are removed to make sure only one edge is connecting the new merged vertex and those common neighbors." Along with this, the illustrating diagram (Figure 1 in this response) should also be helpful to understand the rules.

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 proper ties - 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.

Response:
I take the reviewer's suggestion and clarify the parameter space in the revised manuscript. The reviewer raised excellent questions regarding the model proposed here. I would like to answer some of them here. The parameters chosen are results of trial and error. The observational constrain for these parameters will have to come from continuous measurements of clouds using radar and high-resolution time-lapse movies. These measurements can come from ground-based radar and imager or long-duration airborne imager onboard of unmanned aerial vehicles. Fine scale cloud modeling would also shed some light on these parameters. Some of the other questions require a thorough and systematic numerical investigation of the model behavior, which will be a subject of follow-up papers dedicated to this task.

The physical mechanisms controlling the parameter space of the model can definitely be due to both microphysics and large-scale dynamics and thermodynamics. These physical mechanisms and how they change under different environments are very important and interesting subjects and shall be investigated, which I think is beyond the scope this short manuscript besides a few speculative discussions. In the revised manuscript, I added some open-ended discussions on these aspects. For example, Wilcox (2003) provided interesting observational evidence of cloud merging and cloud statistics. Such observations and modeling equivalents will be useful to bridge the statistical model here and observed distribution pattern.