Review of „**A Cellular Automaton Model of Tropical Oceanic Rain Clusters with Criticality**“ by Kevin K. W. Cheung et al.

Reviewed by Claudia Stephan

I found the manuscript insightful and well composed. The schematics (phase space) are particularly nice. It is certainly worth publishing, although I still see a need for some clarifications and some potential to improve the presentation of this work. I will list the overarching points first, followed by a short list of minor comments. My overall recommendation is minor revision.

Point 1: Scaling exponents

Scaling exponents for area probability, which seems to be the most central exponent, are mentioned in all sections. Different values come up: 187/91, 5/3 and 1.35. It only became clear to me later on how they are related and what the desired result based on the CA ought to be. My suggestion is to introduce these three values at the beginning (maybe introduction, or a section that follows) and to foreshadow their significance for this manuscript. It would also be useful to summarize what type of models are able to produce what slope. It could even become a table.

For example, at L 19-20: “the CA cannot account for the observed $\zeta A \sim 5/3$” Above it says it is near 187/91, and that this is robust. Here it is unclear what the expectation is on what ζA should be. Later, in the discussion, this topic is picked up. As I understand it, there is an SOC model with a slope of 1.35 and their CA with a slope close to 187/91 and they hypothesize that anything in between can be achieved, to get at the 5/3.

My favorite takeaway of the paper is hidden at L 317-320, which acknowledges that even the “observed” value of the scaling exponent is not well known. Then at lines 320-330 they mention that there may be a plausible physical explanation for it. Surely this is directly related to their hypothesized variability of gamma. This to me is new and a key aspect of the work.

Point 2: Reductionist models

Related to the above, I suggest that the authors give a little more background information on what reductionist models there are, like percolation, SOC, and the more complex ones. For example, the study *Ahmed, F. and Neelin, J. D.: Explaining Scales and Statistics of Tropical Precipitation Clusters with a Stochastic Model, J. Atmos. Sci., 76, 3063–3087, https://doi.org/10.1175/JAS-D-18-0368.1, 2019*. is not mentioned, but seems relevant. I would like to know why the authors hope to be more successful with their approach compared to what others have tried before. What is the main difference? It is likely obvious to the authors, but I think it should also be understandable to readers who are not familiar with all of those SOC models and percolation.

A related point: L 34-38: Critical scaling does not provide causality. The fact that so far no simple model has been able to reproduce all observed scaling exponents is in contrast to the hypothesis stated here that the scaling may rely on only “few fundamental physical constraints”. It would be nice to see a statement on what the authors think about the level of explainability that can be derived from these mathematical models.

Point 3: Gravity wave bores

Clearly, they are central. But I find the length of the derivation not well balanced with how they are treated in the CA and sCA. I suggest to simplify it as much as the authors feel comfortable. Specifically:

B.3: I am not convinced that anything before Eq. B8 is needed. Maybe B.3. can be shortened. Also: L 476: Q is the profile, should it be the amplitude L? Why is omega proportional to Q? Given the units of energy, omega^2 should be proportional to Q.

Treatment of GW source: The math is for a single cell, but Fig. B3 looks like the authors have the waves move out from the convective area, which could be a cluster. But if it were, then the math would be wrong due to the count of neighbors. So please clarify. Is it applied to each single cell? And if applied to each cell, then it is not physically correct either, as the whole convective tower generates the waves. The size of the system has an influence on the wave properties. If this is neglected, okay, but it should be stated. This is why I think the discussion of gamma and the wave mechanisms could be simplified. There are so many assumptions in it anyway. And for sCA GW1 is even turned off.

Point 4: Model code

Why is Z so small? I would have expected Z of order 10.000. The model must be computationally cheap. So why is Z such a limitation? Can you provide a more detailed description of the code and how it is run? Nothing is open source. Why can you not publish the code? L 115: "how the model is initialized is irrelevant." That may be true, but writing it like this sounds like it is some secret. In my opinion, for completeness, it should be stated how the model is initialized.

Point 5: Rain rate

At L 121 it is mentioned that rain intensity is basically a diagnostic to enforce observed behavior. Why is this done? Basically, this CA cannot be used to study rain intensity scaling as it gets it wrong. This is taken up in the discussion, which is again interesting (where they talk about modelling water vapor). But I think the plots showing rain can perhaps be omitted, as it is confusing and not insightful (or I am missing something).

Other comments:

L 296-300: I think the authors should not expect the readers to know what "rules" of OFC are, or what the "non-conservation" or "bulk conservation" refers to. Please also explain how the bulk driving scheme of OFC works. When d=0, I would expect no driving. So why is there still driving?

L 11: ß is mentioned, but not defined

L 18: At this point it was unclear to me what is meant by "scaling exponent of characteristic scale with driving force"

L 21: "or manner": what is meant by this?

L 27: "S=s" seems nonsensical

After L 38 values for ζA are discussed, but ζR is not discussed in the introduction. This seems strange given that it is mentioned in the abstract.

L 66: "as what tropical rainfall could be" I do not understand the formulation, please rephrase

L 117: please define "avalanche intensity" also in the text.

L 119: MCS acronym is not defined

In Fig. 2: I assume the percolation values are determined analytically. Could you please clarify how this is done. And then I wonder why the red curve in panel (a) has a kink at $x=10^4$.

L 320: Why is the bump discussed here? I must be missing some link to the text above.

In the summary, RCM is undefined.

Formula (A1): What is s_c?

L 421: That should read B3 instead of A3

Eq. B4: they use tau, but earlier t was used for the driving timescale. Consider using t?

Also, both “a” and “A” are used for area. Best to stick to one of them.

Obviously, the text contains grammatical errors all over the place, which, I suppose, the editorial team will have to sort out.

You misspelled my last name in the reference list: Stephen -> Stephan.