Responses to reviewer 1 of "A cellular automaton model of tropical oceanic rain clusters with criticality" by Cheung, Teo and Koh (egusphere-2026-2469)

I found the methodology, scaling analysis, and discussion to be thoughtful and transparent. In particular, I appreciated the authors' careful distinction between the CA analyzed in the manuscript and more classical SOC formulations discussed later in Section 4. The scaling analyses are carefully conducted, and the demonstration that the non-interacting case maps closely to two-dimensional site percolation is convincing. Overall, I found the manuscript interesting and well written and would propose only minor revisions.

Responses: We appreciate the appraisal from the reviewer, who found our manuscript interesting and well written. Please see our detailed responses to your individual comments.

1. Historical context of cellular automata in atmospheric modeling

The introduction provides a useful overview of CA approaches for cloud and rain organization. However, I think the discussion of CA applications in convection parameterization could be expanded and historically reframed somewhat.

The manuscript may benefit from distinguishing between CA used primarily as idealized/statistical models of criticality and CA used as stochastic organization operators embedded within physical parameterization frameworks.

The manuscript currently cites Palmer (1997, 2001) and Bengtsson et al. (2022), but this compresses a substantial body of work on CA-based representation of convective organization and memory in numerical weather prediction models. Following the original conceptual proposals of Palmer (1997, 2001), a series of studies developed and implemented CA-based convective organization frameworks within mass-flux convection parameterizations (e.g., Bengtsson et al., 2011, 2013, 2019, 2021, 2022). These studies demonstrated applications to sub-grid convective organization, convective memory, and cross-grid communication in atmospheric models, including implementation in operational forecast systems (e.g., GFSv17; Bengtsson and Han, 2024).

Responses: Thank you for pointing to these additional works on CA-based representation of convective organization and memory in numerical weather prediction models, which we have overlooked in the original manuscript. In the revised version, we have added the following literature review, together with a few other studies suggested by reviewer 2 and 3, in the Introduction section:

"In addition to these physically motivated CA models, a parallel line of research has focused on using CA as a stochastic parameterization tool within numerical weather prediction (NWP) models to represent subgrid convective organization and its uncertainty (Bengtsson et al., 2011, 2013, 2019, 2021, 2022; Bengtsson and Han, 2024; see also Nober and Graf, 2005; Graf and Yang, 2007). These studies have demonstrated that CA-based stochastic schemes can improve the representation of convectively coupled equatorial waves, Madden–Julian oscillation propagation, and ensemble spread in operational forecast systems. However, their emphasis is on forecast skill rather than on understanding the fundamental scaling properties of rain clusters. Other reduced-complexity models have approached convective organization from different physical perspectives. Craig and Mack (2013) developed a coarsening model based on free-tropospheric moisture budget, drawing analogy with phase separation in materials science. Ahmed and Neelin (2019) adopted a stochastic modelling approach that was based on the noise-driven moisture–weak temperature gradient (WTG) equation. Their model was on a 2-dimensional plane and represented processes from large-scale circulation to storm internal dynamics as well as precipitation parameterization. Thus, their approach was more like a NWP model, with the benefits of a large variety of sensitivity tests. Indeed, cluster area and rain scaling exponents very close to those found in T17 were reproduced. Ahmed and Neelin (2019) also discussed that why they intended to exclude site percolation as a framework for precipitation clustering, mainly based on the fact that the cluster scaling exponents are sensitive to grid size and that there was no clear physical scale corresponding to the cutoff (deviation from power law) in percolation model. Nevertheless, as will be seen in the following, our approach is to develop a convective physics-based SOC model with near-field dynamics and relationships to the cluster scaling exponents, rather than from a pure percolation approach. Biagioli and Tompkins (2023) proposed a stochastic reaction–diffusion lattice model that predicts the onset of self-aggregation through a dimensionless 'aggregation number'. More recently, Neggers and Griewank (2021, 2022) formulated the BiOMi (Binomial Objects on Microgrids) framework, which models populations of interacting convective thermals on a two-dimensional grid and couples them to a spectral convection scheme to capture spatial organization and its impact on vertical transport. While these models offer valuable insights into different aspects of convective organization, from onset prediction to population dynamics, they have not systematically examined the critical scaling exponents of rain cluster size and rain rate distributions, which are the focus of the present study."

2. Framing of criticality and self-organized criticality

Some of the discussion surrounding criticality could perhaps be framed somewhat more cautiously.

I very much liked Sections 2 and 3, which I found clear, transparent, and thoughtfully presented. However, after reading these sections, it appears that the discussion of criticality and SOC could perhaps be framed more carefully already in the Introduction.

In particular, the emergence of scaling behavior near a parameter-dependent critical value appears conceptually closer to tuned critical phenomena or absorbing-state transitions than to classical self-organized criticality in the strict sense, where the system dynamically evolves toward criticality without external parameter tuning. Since the manuscript already acknowledges some of these distinctions in Sections 2-4, it may be beneficial to introduce this nuance earlier in the paper as well.

More generally, power-law cluster statistics are certainly suggestive of critical behavior, but they do not uniquely imply SOC. Similar scale-invariant behavior can emerge from a variety of multiscale dynamical processes. I therefore encourage slightly more cautious wording in the Introduction and Abstract regarding the interpretation of tropical rainfall as a critical phenomenon.

Responses: Thank you for this comment. Indeed, in the original manuscript we only discussed the distinction between tuned critical phenomena, as in our model, and classical SOC when we presented the model results and further discussion in sections 2–4. Also, scale-invariant behaviour does not imply SOC directly. In response, we have revised the last part of the abstract as:

"…, further tuning of it such as through the convective cells interaction strength or how they interact may allow the CA to produce the observed exponent. Whether such behaviour can arise from a genuinely self-organized mechanism, rather than through parameter tuning, remains a question for future investigation."

There are also new discussions added to the Introduction section:

"In the specific framework of self-organized criticality (SOC) (Bak et al., 1987), such behaviour arises spontaneously without external parameter tuning, and the critical exponents such as ζ_S for a certain observable would be universal, should be seen in the distributions across observations and numerical models, and depending only on a few fundamental physical constraints rather than on the detailed interactions and microphysical processes. However, it is important to note that power-law statistics alone do not uniquely imply SOC; similar scale-invariant behaviour can emerge from a variety of multiscale dynamical processes, including tuned critical phenomena (where a control parameter must be finely adjusted) or absorbing-state phase transitions (Dickman et al., 1998; Vespignani and Zapperi, 1998). In the present study, we do not claim that the CA exhibits SOC in the strict sense; rather, we use it as a controlled testbed to investigate which physical mechanisms may drive the system toward criticality and to examine the resulting scaling exponents."

"Our model exhibits critical behaviour when the large-scale drive is tuned to a specific value, and its cluster statistics resemble those of systems near a critical point, reminiscent of the behaviour hypothesized for tropical rainfall (Peter and Neelin, 2006; Peters et al. 2009; Stechmann and Neelin, 2016; T17). However, as we will discuss in sections 2–4, the CA requires fine-tuning of the external drive to reach criticality, placing it in the category of tuned critical phenomena rather than SOC in the strict sense. This distinction is important: the value of the CA lies not in demonstrating SOC per se, but in enabling a controlled investigation of how specific physical processes (e.g., gravity wave interactions in our model) influence the emergent scaling exponents of rain clusters."

3. Physical interpretation of the percolating regime

The discussion of percolating clusters and the critical transition is mathematically clear, and I learned a lot from this section. However, I would welcome additional discussion of the physical interpretation of the percolating regime in the context of tropical convection.

In percolation theory, a percolating cluster corresponds to a connected structure spanning the computational domain. Since one does not typically observe a continuous precipitating cluster spanning an entire tropical basin (or the tropical belt) in nature, some discussion of the atmospheric analogue of the percolating state would be useful.

Relatedly, the discussion section linking basin-wide RCM rain clusters to incipient infinite percolation clusters is intriguing. However, it may be useful to distinguish more clearly between a mathematical analogy and a demonstrated physical correspondence. Additional discussion of what atmospheric structures would physically correspond to the percolating regime would help place this interpretation in context.

Responses: We thank the reviewer for raising this important point. We agree that a literal interpretation of a "percolating cluster" as a continuous precipitating entity spanning an entire basin is unphysical. In the revised manuscript, we have clarified that the atmospheric analogue of a percolating cluster should be understood as a large-scale, connected region of active convection, e.g., the ITCZ or a monsoon trough, within which individual convective cells are dynamically coupled, rather than as a contiguous precipitation shield. We have also strengthened the distinction between a mathematical analogy and a demonstrated physical correspondence when we compared RCM-simulated clusters and those in the CA.

The new discussion in section 4.2 of the revised manuscript is as follow:
"The distributions of the modelled rain cluster size, for example by regional climate model (RCM) in T21, for all tropical oceanic regions that include the spanning clusters in the data samples show a "bump" at the large cluster sizes (see Fig. 2 of T21). This is reminiscent of the CA in the supercritical region, where a domain-spanning cluster emerges. In the atmospheric context, such a spanning cluster should not be interpreted literally as a continuous precipitating entity, but rather as a large-scale, connected region of active convection (e.g., the ITCZ or a monsoon trough), within which individual convective cells are dynamically linked. The "bump" in the RCM distributions may therefore reflect the model's tendency to produce overly persistent and spatially extended convective organization compared to observations."

4. Interpretation of the scaling exponents

I found one of the most interesting results of the manuscript is that the interacting CA departs from standard site percolation in several respects (e.g., Ds, ηs , and dc vary with γ), while the cluster-size exponent ζA remains approximately invariant and close to the percolation value. This raises interesting questions regarding universality and the physical interpretation of the critical transition. Additional discussion of why ζA appears robust to the introduction of local convective interactions, despite substantial changes in the other critical exponents, would further strengthen the manuscript.
Similarly, because the inferred scaling exponents vary with the coupling parameter γ, some discussion of the physical interpretation of γ and the implications of the resulting changes in scaling behavior would be valuable.

Responses: We thank the reviewer for this insightful comment. We have added a paragraph in section 4.3 of the revised manuscript discussing the physical interpretation of γ and the contrasting behaviour of the exponents, which is as follows.

"Such robustness warrants further physical interpretation. The parameter γ controls the strength of gravity-wave-mediated coupling between convective sites. This leads to stronger spatial correlations in the instability field *X*, which manifests in the critical exponents that describe the spatial structure and correlation length of the clusters: the fractal dimension D_A and η_A possessing sensitivity of the cluster sizes to the proximity of the critical drive. In contrast, ζ_A appears to be governed by a more fundamental topological constraint. The rain clusters defined by nearest-neighbour connectivity of toppled sites form a set of connected components on the lattice. Regardless of the details of the interaction rules that determine which sites topple, as long as the system is driven to a critical point characterized by a divergent correlation length, the cluster size distribution of such connected components falls into the universality class of two-dimensional percolation. In the CA, γ modifies the "microscopic" rules (the interaction strength) but does not change the fundamental fact that the clusters are nearest-neighbour-connected sets on a 2D lattice at a critical point, thus leaving ζ_A unchanged. The other exponents, η_S and D_S, are "non-universal" in the sense that they describe the approach to criticality and the geometric details of the clusters, which are indeed sensitive to the specific interaction rules."

Finally, it would be useful to distinguish more clearly which scaling relationships are emergent properties of the CA dynamics and which are externally constrained. For example, the cluster geometry and associated scaling exponents appear to emerge naturally from the model, whereas the area-conditioned rain-rate scaling is imposed through Eq. (6). Explicitly separating these aspects would help readers understand which observed characteristics are explained by the model and which are prescribed.

Responses: We thank the reviewer for prompting us to clarify this important distinction. In the Summary section 5 we have added the following discussion:
"It is important to distinguish between two distinct classes of scaling behaviour in the CA: those that are emergent from the dynamics (the cluster area distribution, its fractal dimension and the associated critical exponents) and those that are externally imposed (relationship between cluster rain rate and cluster area). This distinction highlights both the strength and the limitation of the CA approach. The model successfully demonstrates that percolation-like cluster geometries can emerge from relatively simple convective interaction rules, a finding that helps explain the ubiquity of power-law cluster size distributions in observations and models. However, the intensification of rainfall with cluster size, and thus the precise values of ζ_R, requires additional physical processes (e.g., mesoscale updrafts, cold-pool convergence, or organized inflow) that are not yet represented in the current CA. Future extensions of the model should aim to make the β exponent an emergent property rather than an imposed constraint."

We believe that this clarification would help frame the CA's contribution and strengthens the manuscript by clearly delineating what the model explains and what it assumes.

References:

Bengtsson, L., Körnich, H., Källén, E., & Svensson, G. (2011): Large-Scale Dynamical Response to Subgrid-Scale Organization Provided by Cellular Automata. Journal of the Atmospheric Sciences, 68(12), 3132–3144.

Bengtsson, L., Steinheimer, M., Bechtold, P., & Geleyn, J.-F. (2013): A stochastic parametrization for deep convection using cellular automata. Quarterly Journal of the Royal Meteorological Society, 139(675), 1533–1543.

Bengtsson, L., Bao, J., Pegion, P., Penland, C., Michelson, S., & Whitaker, J. (2019): A Model Framework for Stochastic Representation of Uncertainties Associated with Physical Processes in NOAA's Next Generation Global Prediction System (NGGPS). Monthly Weather Review, 147, 893–911.

Bengtsson, L., Dias, J., Tulich, S., Gehne, M., & Bao, J.-W. (2021): A stochastic parameterization of organized tropical convection using cellular automata for global forecasts in

NOAA's Unified Forecast System. Journal of Advances in Modeling Earth Systems, 13, e2020MS002260.

Bengtsson, L., Gerard, L., Han, J., Gehne, M., Li, W., & Dias, J. (2022): A Prognostic-Stochastic and Scale-Adaptive Cumulus Convection Closure for Improved Tropical Variability and Convective Gray-Zone Representation in NOAA’s Unified Forecast System (UFS). Monthly Weather Review, 150(12), 3211–3227.

Bengtsson, L., & Han, J. (2024): Updates to NOAA’s Unified Forecast System’s Cumulus Convection Parameterization Scheme between GFSv16 and GFSv17. Weather and Forecasting

Responses to reviewer 2 (Claudia Stephan) of "A cellular automaton model of tropical oceanic rain clusters with criticality" by Cheung, Teo and Koh (egusphere-2026-2469)

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.

Responses: Thanks, Claudia for your constructive and specific comments. We are glad that you considered our manuscript insightful and well composed. Please see our point-by-point responses in the following.

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.

Responses: We are sorry about such confusion in the abstract. We meant that the exponent for rain area from our model is close to the theoretical value (187/91) of 2-dimensional site percolation, and this value is also close to modelling results (especially over Indian and Atlantic Ocean) we found previously (T21). However, the CA's exponent still deviates from the observation-based value identified in T17, which is about 5/3. We have modified the related sentences in the abstract to reflect our meaning better:

"… and the ζ_A is estimated to be near the theoretical 2-dimensional percolation value of 187/91. This agrees well with the ζ_A estimates over the Indian Ocean warm pool and the tropical Atlantic reported in previous modelling study, implying numerical models behave like percolation. … Although the CA cannot account for the observation-based $\zeta_A \sim 5/3$ reported elsewhere that is quite universal over all oceans, …"

For the value 1.35, we think you referred to the estimated beta value using the exponents for *A* and *R* (equation A4) and summarized in Table 2? Since Table 2 has summarized all the exponents identified from our model and the reference values from 2-D percolation, we consider that an additional table is not quite necessary.

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.

Responses: Thank you for pointing out the reference Ahmed and Neelin (2019). We did not mention this study because it was not based on a CA or percolation framework. However, as a stochastic model of precipitation clusters the study is relevant to ours. Thus, we have added the following discussion to the Introduction: "Ahmed and Neelin (2019) adopted a stochastic modelling approach that was based on the noise-driven moisture–weak temperature gradient (WTG) equation. Their model was on a 2-dimensional plane and represented processes from large-scale circulation to storm internal dynamics as well as precipitation parameterization. Thus, their approach was more like a NWP model, with the benefits of a large variety of sensitivity tests. Indeed, cluster area and rain scaling exponents very close to those found in T17 were reproduced. Ahmed and Neelin (2019) also discussed that why they intended to exclude site percolation as a framework for precipitation clustering, mainly based on the fact that the cluster scaling exponents are sensitive to grid size and that there was no clear physical scale corresponding to the cutoff (deviation from power law) in percolation model. Nevertheless, as will be seen in the following, our approach is to develop a convective physics-based SOC model with near-field dynamics and relationships to the cluster scaling exponents, rather than from a pure percolation approach." In addition, reviewer 1 and 3 also suggested new references for us and we have reviewed them in the Introduction. Please see the newly added literature review in that section.

On your last point, we do not think that the fact of no simple model has been able to reproduce all observed scaling exponents is in contrast to the hypothesis of scaling may rely on only a few physical constraints, because we focus only on gravity wave dynamics in the current model but there are other mechanisms that we can test. Thus, we have added the following paragraphs in section 4:

"The fact that our simple CA, incorporating only gravity-wave-mediated interactions, yields a ζ_A near the 2-dimensional percolation value (187/91) rather than the observed ~5/3 does not contradict the hypothesis that the observed scaling may depend on only a few fundamental physical constraints. On the contrary, it offers a valuable diagnostic attribution: it demonstrates that gravity wave dynamics alone drive the system towards a percolation universality class, and that reproducing the observed 5/3 exponent likely requires additional fundamental physical ingredients, such as cold-pool dynamics (Haerter, 2019), large-scale vertical overturning (Stephan and Stevens, 2025), or moisture–radiation feedback, that are not yet represented in the current model.
This highlights the distinctive level of explainability offered by reductionist CA models. Because each rule in the CA maps directly to a specific, well-defined physical process, any emergent change in the statistical exponents can be causally traced back to its underlying mechanism. In contrast, high-resolution dynamical models, while more realistic, often convolve numerous interacting processes, making it difficult to isolate which physical ingredient drives a particular statistical behaviour. The CA approach thus serves as a controlled testbed or sandbox for systematically testing which physical mechanisms are necessary and sufficient to reproduce observed rain cluster statistics."

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.

Responses: Indeed, we have included a lot of details of derivation leading to equation B8. In the revised manuscript we have simplified section B.3 by retaining how the two gravity wave bores change the model variable *X*.

Of course, square of vertical velocity is proportional to heating energy. In the sentence before equation 8 we just meant that the magnitude of vertical velocity induced by the gravity wave would be proportional to the heating magnitude. To be more concise we have revised it to "Since $|\omega_{c\ell}|$ is proportional to $|Q_\ell|$ (more rigorously square of vertical velocity is proportional to heating energy) ……"

On the treatment of GW source, the comment is insightful and we have considered this during model development. Our logic of thought was like this: In T21 we demonstrated that even a full-physics model (WRF in our case) could not reproduce the scaling exponents in cluster area and rain adequately, and not the universality over all oceans, compared with observations (T17). Thus, our strategy was to take the “reductionist” approach and tested whether a set of simplest “rules” can reproduce the scaling exponents, and thus the CA model as presented in this manuscript. The treatment of the GW source on a cell-by-cell basis in the current model we based on our motivation of using SOC theory to explain rain cluster behaviour, which we have argued in T17 that matched with observations quite well. In SOC dynamics, there are two critical properties. On of them is the separation of slow driving (radiative) time scale and fast toppling (convective) time scale, which we have done in the model. The other critical property is that there is only near-field interaction in a SOC model (as in the sandpile model), based on which large area of toppling can be generated and thus existence of large spatial correlation in the model. This is why we have implemented only the nearest-neighbour destabilization and next-nearest-neighbour stabilization in the CA model, representing the two well-known processes in GW effects, and at the same time resembling SOC dynamics. Certainly, a convective tower can collectively generate gravity waves that may propagate beyond the next-nearest neighbour (given the consideration of the grid resolution in our model). We have avoided such far-field interaction in this version of model, for which we intended to be keep it as simple as possible. The estimation of gamma did rely on some empirical evidence and thus we have put that in the Appendix for completeness. As in our response to your point 2, additional convective dynamics can be added in a later stage to test whether the observed scaling exponents can be reproduced even better.

We have added brief discussion on this in section 2.2 where we described the GW treatment: “Note that in this scheme of gravity wave effect, treatment is site-by-site and thus only near-field effect has been simulated. In reality, convective cells form a cluster and collectively a strong gravity wave may propagate from the convective tower as a whole and influence the far field. We only simulate the near-field effect because that is more in accord SOC models, which generates long-range spatial correlation from short-range interaction.”

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.

Responses: We did not explain the choice of Z enough in the original manuscript. The lattice size was set actually following our two previous publications (T17 and T21) that analysed satellite rain data and model simulated rain, respectively. In section 2.1 of the revised manuscript, we have added the following explanation: “For lattice size Z, we consider the data resolution that was analysed in T17 (25-km satellite rain) and T21 (36-km numerical model simulation). While Z=120 and Z=96 was applied in T17 and T21 respectively, which were large enough to cover each of the tropical oceanic regions with deep convection, we extend the lattice size to Z=512 for the CA here such that scaling up to very large rain clusters can be realized."

Our model code is open to anyone of interest (will be stated in the data availability statement), just that we have not uploaded the codes to an open repository.

L115: The model was initialized by driving sites randomly to unstable state. We have modified the statement to "During initialization of the model, randomly selected sites are driven by the value d defined above. The model is then analysed when it attains a stationary state based on the number of clusters in the model."

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).

Responses: Yes, in our model the rain rate scaling with rain area has been constrained according to observations because that scaling did not naturally evolve in the model, due to reasons we have discussed in section 2 and 4. See also reviewer 1's final point and our responses. However, we think that the rain rate associated results (Fig. 4, now Fig. 6 in the revised manuscript) are still informative for future work when new or additional mechanisms are introduced to the model such that the constraint on rain rate can be relaxed.

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?

Responses: As an earthquake model, the OFC model uses stress as the model parameter and when it is over a threshold a site topples and spread the stress over other sites, similar to our model but with atmospheric instability as the model parameter and site interaction according to gamma. "Non-conservation" and "bulk conservation" refer to whether all the toppling energy spread to the neighbours, and in our case controlled by the dissipation coefficient ε.

We have revised the relevant discussion as "…is similar to the OFC earthquake model (in which stress in the lattice sites is analogous to atmospheric instability in our CA and when stress is over a threshold a site topples; Olami et al., 1992), in terms of its rules for nearest-neighbour interactions, γ as a control of the level of non-conservation (i.e., not all toppling energy passes to neighbours), as well as how the model is driven. The OFC model is widely accepted to be an SOC when bulk conservation is enforced (i.e., dissipation $\varepsilon = 0$; …)

The model $sCA_{\gamma,0^+}$ has d approaching zero and thus there is still a small driving.

L 11: ß is mentioned, but not defined

Responses: The phrase "β is that for rain rate conditioned by rain area" has been added to the abstract.

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

Responses: "with driving force" is quite confusing. We have simplified the phrase to "scaling exponent of the characteristic scale".

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

Responses: We have revised to "how they interact".

L 27: "S=s" seems nonsensical

Responses: We use capital S to denote either area A or rain rate R as a random variable, however, since this is not quite necessary in this statement, we have removed it.

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.

Responses: It was because there is direct correspondence from 2-dimensional site percolation to rain cluster area, but not for rain rate. We have to wait until section 2 to discuss how we relate cluster rain rate and avalanche intensity.

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

Responses: We have revised that statement to "Our model exhibits critical behaviour when the large-scale drive is tuned to a specific value, and its cluster statistics resemble those of systems near a critical point, reminiscent of the behaviour hypothesized for tropical rainfall (Peter and Neelin, 2006; Peters et al. 2009; Stechmann and Neelin, 2016; T17)."

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

Responses: It is the I in equation (3) and summation over a cluster. We have revised to "…cluster rain rate (taken as proportional to the avalanche intensity: I in equation (3) and summation over a cluster) and…"

L 119: MCS acronym is not defined

Responses: Full name of "mesoscale convective system" has been added.

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 $x=10^4$.

Responses: This figure becomes Fig. 4 in the revised manuscript. As described in the figure caption, the percolation occupancy probability (p) was set to the averaged fractional rain cluster coverage for the CA, and this choice of p value led to the good match in the probability distribution between the CA and percolation.

The slope of the scaling regime in the figure is very close to the analytical value of 187/91, and the kink is due to deviation from scaling because of finite-size effect.

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

Responses: Indeed, this mentioning of the bump due to the spanning clusters modelled in T21 was quite sudden. However, reviewer 1's point 3 also commented on a related issue of the physical interpretation of the percolating (domain-spanning) clusters. Thus, we have provided more context and dedicated the related discussion as a separate paragraph:

"The distributions of the modelled rain cluster size, for example by regional climate model (RCM) in T21, for all tropical oceanic regions that include the spanning clusters in the data samples show a "bump" at the large cluster sizes (see Fig. 2 of T21). This is reminiscent of the CA in the supercritical region, where a domain-spanning cluster emerges. In the atmospheric context, such a spanning cluster should not be interpreted literally as a continuous precipitating entity, but rather as a large-scale, connected region of active convection (e.g., the ITCZ or a monsoon trough), within which individual convective cells are dynamically linked. The "bump" in the RCM distributions may therefore reflect the model's tendency to produce overly persistent and spatially extended convective organization compared to observations."

In the summary, RCM is undefined.

Responses: As per our response above, RCM has been defined in the newly added paragraph.

Formula (A1): What is s_c?

Responses: It was a typo. s_c is the upper cutoff in the scaling regime, which was wrongly put as ξ_S. This has been corrected.

L 421: That should read B3 instead of A3

Responses: This has been corrected.

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

Responses: Indeed, we use t for the driving timescale and tau for the micro timescale. Thus, we have changed the tau_{LS} here to t_{LS}.

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

Responses: We have followed the notations used in T17 and T21 here. Capital A (R) represents the random variable of area (rain rate), while a (r) represents sample value of the variable (as in $E(R \mid a)$).

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

Responses: The manuscript has been thoroughly checked for grammatical errors.

You misspelled my last name in the reference list: Stephen -> Stephan.
Responses: Sorry about the misspelling, this has been corrected.

Responses to reviewer 3 of "A cellular automaton model of tropical oceanic rain clusters with criticality" by Cheung, Teo and Koh (egusphere-2026-2469)

Understanding the distributions of the cluster area and total rain rate for tropical oceanic rain clusters is a challenging problem. This study developed a cellular automation (CA) model for simulating the evolution of cloud clusters by only imposing a few simple rules for convective organization. Among them, the key rules are the large-scale destabilization of the atmosphere and effects of propagating gravity waves. The results reproduces some observed scaling exponent as reported in other studies. Considering its scientific merits, I believe this study matches the broad scope of the journal. That said, I have several major and minor comments regarding its presentation and scientific discussion. Overall, I would suggest a major revision before it is considered for publication.

Responses: We are glad that the reviewer commanded the scientific merits of our study and it met the broad scope of the journal. Please see our detailed responses to your major and minor comments in the following.

Major Comment:

1. Section 2: CA for tropical rain clusters and Table 1. This part is very important but poorly presented. I would suggest the authors to rewrite the whole section and clearly present the CA model. The details in Appendix B is also very important, I would suggest to move Appendix B back to the main context.

Responses: In the original manuscript we put a lot of model details in Appendix B due to manuscript length consideration but has sacrificed clarity in the main text. In the revised version we have moved Figures B1 and B3 and associated discussion back to the main text (as Figures 1 and 2, respectively). We still retain the mathematical details in Appendix B to keep the length of the main text reasonable.

2. Two time scales. I can understand that the two time scales are introduced in the model to mimic radiative time scale and convective time scale. But it is unclear to me how it works in the model perspective. Please clarify how the variable X changes in terms of two time scales (can tau be arbitrary large when t is fixed?) and why you choose to do so. It seems to me that using a single time scale should be a more straightforward way.

Responses: Separation of time scales, as emulation of the slower synoptic (radiative) driving and faster convective energy release in realistic atmospheric convection, are critical to the rain cluster size distributions simulated in the CA. In implementation, driving of all the model sites (according to timestep t) is only resumed after all toppling (avalanche) activities complete, after which all sites become stable again and the system as a whole is in equilibrium (then the cluster statistics are analysed). Accordingly, there could be many short tau timesteps before the next t timestep.

3. Cellular automation model. It seems to me that the so-called cellular automaton model is just grid models that have been extensively studied before in this field. The authors seem to ignore such previous literatures such as Craig and Mack (2013) and Biagioli and Tompkins (2023).

Please go search the literature and add more discussions about the grid models for convective organization.

Responses: Thank you for pointing to these relevant literatures on grid models and convective organization. Together with suggestions from reviewer 1 and 2, we have added the following discussion in the Introduction section:

"In addition to these physically motivated CA models, a parallel line of research has focused on using CA as a stochastic parameterization tool within numerical weather prediction (NWP) models to represent subgrid convective organization and its uncertainty (Bengtsson et al., 2011, 2013, 2019, 2021, 2022; Bengtsson and Han, 2024; see also Nober and Graf, 2005; Graf and Yang, 2007). These studies have demonstrated that CA-based stochastic schemes can improve the representation of convectively coupled equatorial waves, Madden–Julian oscillation propagation, and ensemble spread in operational forecast systems. However, their emphasis is on forecast skill rather than on understanding the fundamental scaling properties of rain clusters. Other reduced-complexity models have approached convective organization from different physical perspectives. Craig and Mack (2013) developed a coarsening model based on free-tropospheric moisture budget, drawing analogy with phase separation in materials science. Ahmed and Neelin (2019) adopted a stochastic modelling approach that was based on the noise-driven moisture–weak temperature gradient (WTG) equation. Their model was on a 2-dimensional plane and represented processes from large-scale circulation to storm internal dynamics as well as precipitation parameterization. Thus, their approach was more like a NWP model, with the benefits of a large variety of sensitivity tests. Indeed, cluster area and rain scaling exponents very close to those found in T17 were reproduced. Ahmed and Neelin (2019) also discussed that why they intended to exclude site percolation as a framework for precipitation clustering, mainly based on the fact that the cluster scaling exponents are sensitive to grid size and that there was no clear physical scale corresponding to the cutoff (deviation from power law) in percolation model. Nevertheless, as will be seen in the following, our approach is to develop a convective physics-based SOC model with near-field dynamics and relationships to the cluster scaling exponents, rather than from a pure percolation approach. Biagioli and Tompkins (2023) proposed a stochastic reaction–diffusion lattice model that predicts the onset of self-aggregation through a dimensionless 'aggregation number'. More recently, Neggers and Griewank (2021, 2022) formulated the BiOMi (Binomial Objects on Microgrids) framework, which models populations of interacting convective thermals on a two-dimensional grid and couples them to a spectral convection scheme to capture spatial organization and its impact on vertical transport. While these models offer valuable insights into different aspects of convective organization, from onset prediction to population dynamics, they have not systematically examined the critical scaling exponents of rain cluster size and rain rate distributions, which are the focus of the present study."

4. Effects of propagating gravity waves. This model only consider destabilization effects for the nearest-neighboring grids and stabilization effects for the next-nearest-neighboring grids. Why is that? Gravity waves generally can propagate further than that. In fact, a recent study by Wilson and Hearter (2026) (https://meetingorganizer.copernicus.org/EGU26/EGU26-21637.html, presented in EGU this year but probably not published yet) discussed this from a conceptual

model perspective. I would suggest the authors to discuss the similarity and differences between this model and other models that incorporates gravity waves.

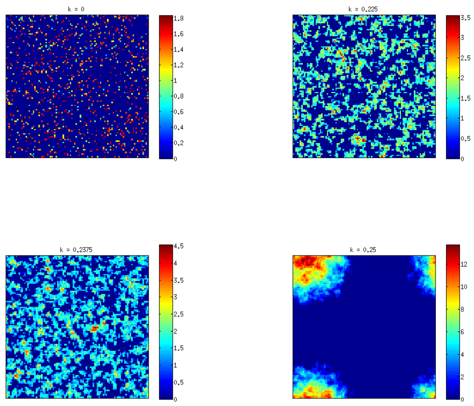
Responses: In fact, the grid size (largest Z=512) in our model follows the observational analysis in T17 and modelling analysis in T21, with lattice resolution 20–30 km. See also our response to reviewer 2's point 4 and additional description in section 2.1. Thus, the nearest-neighbour destabilization and next-nearest-neighbour stability has considered the propagation speed of gravity waves already.

Thank you for pointing to the recent EGU presentation by Wilson and Hearter (2026). Their study is very relevant to ours. Unfortunately, a formal publication from Wilson and Haerter seems not available yet and without the details in their study we are not able to discuss more about the similarity and differences between our models.

Please see also our responses to reviewer 2's point 3 on how we treat gravity wave propagation based on sites but not the convective cluster as a source, and for the later there is likely far-field effect as you mentioned. Basically, our argument is that we intend to test the SOC dynamics that would generate spatial structures (i.e., from the small to large avalanches or clusters) based only on near-field interaction, although we fully realize that gravity wave effect can propagate long distance. On the other hand, our experiments with a dynamical model (WRF) in T21 already demonstrated that when long-distance gravity wave propagation is simulated in such model, the scaling exponents of cluster area and rain rate still deviate from those observed. Thus, in this CA model we intend to test the simplest dynamics in idealized ways.

5. Figure 1. It is better to show some data analysis based on your simulation, beside the schematic diagram. Otherwise, readers have no idea how the actual model simulations matches the schematic diagram in Figure 1. Same comment to Figure 9.

Responses: Thank you for this comment. We think the schematic diagram and the rules in the CA model (Table 1) have depicted clearly how we performed the simulations. During the model run, clusters form in the model based on the variable X, one snapshot of the clusters in is shown below as an example. Then we computed the cluster area and rain (based on the constraint we imposed) statistics, based on which all the scaling results in our figures were generated.



6. It is a bit surprising to me that the cases without interacting lattice still exhibit the power law. Why is that? I would suggest to add the scaling exponent in the panels of Figure 2. Same comments to the remaining figures with the distribution curves such as Figure 3-5.

Responses: Without interaction, our model resembles 2-dimensional site percolation with known analytical scaling exponents. We have added a reference line showing the scaling exponent (i.e., the slope) 187/91 in Figure 2 (now Figure 4 in the revised manuscript).

For Figures 3–5, the several scaling exponents have been shown in the data collapse (c and d) panels. For clarity, we have stated them in the figure captions again.

7. The discussions in Section 3.2 and 3.3 are way too technical. I would suggest the authors to rewrite these discussions, avoid too many numbers, and try to focus more on physical interpretation.

Responses: Indeed, the technical details are essence of our model results. However, for readers to understand the physical interpretation better, even without going through all our technical details, we have prepared short qualitative summary at the end section 3.2 and 3.3. They are as follow.

Section 3.2:
"In qualitative terms, the non-interacting case ($\gamma = 0$) serves as a baseline for understanding the CA's behaviour. Here, each lattice site topples independently of its neighbours, and the only source of spatial organization is the geometric connectivity of toppled sites at quiescence. The fact that the cluster size distribution for $CA_{0,d}$ is essentially indistinguishable from that of standard 2D site percolation demonstrates that this CA's toppling dynamics naturally generates a percolation-like critical state when the external drive d is tuned to a critical value. In this state, clusters of all sizes emerge, with the largest clusters becoming increasingly space-filling as d approaches d_c. The power-law exponent ζ_A (for cluster area) matches the theoretical percolation value, confirming that the CA's connectivity rule (nearest-neighbour links between toppled sites) is sufficient to reproduce the universal cluster geometry of 2D percolation. This baseline case also establishes that the separation between slow driving and fast relaxation timescales is already

sufficient to produce scale-invariant cluster statistics, even before introducing convective interactions."

Section 3.3:
"Physically, the interacting case ($\gamma > 0$) introduces convective coupling between neighbouring sites through the stabilizing and destabilizing effects of gravity waves (GW1 and GW2). As γ increases, the spatial correlations in the instability field X become stronger: the destabilizing GW2 encourages convective triggering in the immediate vicinity of an existing event, while the stabilizing GW1 suppresses convection further away. This leads to two notable qualitative changes in the cluster statistics. First, the fractal dimension D_A decreases with γ; clusters become more ramified and hole-ridden because the stabilizing far-field influence inhibits the filling of gaps within clusters. Second, the exponent η_A, which governs how rapidly cluster sizes grow as d approaches d_c, increases with γ; the clusters become more sensitive to the precise tuning of the external drive.
Remarkably, despite these changes in the geometric details of the clusters, the exponent ζ_A remains robust to variations in γ, staying close to the 2D percolation value. This robustness suggests that ζ_A is governed by a topological constraint that is independent of the microscopic interaction rules: as long as clusters are defined by nearest-neighbour connectivity on a 2D lattice at criticality, the asymptotic power-law decay of the cluster size distribution falls into the percolation universality class. In contrast, the exponents describing the approach to criticality (η_S) and the geometric structure of clusters (D_S) are sensitive to the details of the interactions, in other words they encode the "fingerprint" of how convection couples neighbouring sites. Thus, this CA provides a clear causal attribution: gravity-wave-mediated interactions shape the spatial structure and correlation length of rain clusters, but they do not alter the fundamental 2D percolation geometry that governs the asymptotic cluster size distribution."

8. Figure 6-10. I had a hard time to follow all the details. I guess at least one thing you need to focus on is the effects of propagating gravity waves when comparing the interacting cases with non-interacting cases.

Responses: With our additional qualitative summary to section 3.2 and 3.3 we mentioned in point 7, we believe that readers can understand better the differences between the two sets of experiments with and without interaction based on our treatment of propagating gravity waves. These figures become Figures 8–12 in the revised manuscript.

Minor Comment:
1. Table 1, Equation 3: The symbol "I_j" first occurs in this equation without definition before or after it. You need to first introduce what I_j is. (May be the avalanche intensity ?)

Responses: Yes, I_j is the avalanche intensity. This has been added before equation 3.

2. Line 104: "coordinates"

Responses: This has been corrected.

3. Line 108: "lattice indices that defines ..."

Responses: This has been corrected.

4. Equation (7): The symbol "Z" first occurs in this equation without explanation before or after it. It is better to first introduce the lattice length Z.

Responses: "*Z* is the lattice size" has been added after equation (7). Also, according to point 4 from reviewer 2, we have first introduced the lattice size used in section 2.1 of the revised manuscript.

5. Figure 1: There are too many words in this figure and the fontsize is too small. I would suggest first simplifying the descriptions into several words in the figure and then explaining them in detail in the figure caption.

Responses: We have simplified this figure with only large headings in it and moved the description to the figure caption. This is now Figure 3 in the revised manuscript.

6. Line 145: "Ds is the a ...", "the" is needless.

Responses: This has been corrected.

7. Line 159: "be mapped to"

Responses: This has been corrected.

8. Line 332: The description of the question is too hard to understand and a question mark is missing. How about "Is feedback from convection (i.e., generation of waves and vertical motions by condensation) relevant for the relationship between ... ?"

Responses: It was an original quote from the reference Stephan and Stevens (2025) and thus we keep the question as is. Question mark has been added.

9. Line 348: "a limited set of ..."

Responses: This has been corrected.

10. Line 355: "exhibits criticality which is similar to ..."

Responses: This has been corrected.

11. Line 378: "A and R", a space is missing.

Responses: This has been corrected.