Dear Editors,

We would like to thank the associate editor and the two anonymous referees for their insightful comments and for taking the time to read our paper carefully. Their feedback helped us improve the paper immensely and we are very gratefully for that. Below, we provide point-by-point response to these comments and state what we did accordingly to improve the paper. Reviewer's comments are presented in *italic* characters, while our responses are indicated in **bold**. Text modifications, as reflected in the revised manuscript, are highlighted in **blue**.

The authors.

Response to Referee # 1

In the article "A statistical mechanics model for cloud cover: Case of low clouds over the Gulf of Guinea" the authors propose that clouds can be described using the 2D Ising model. As evidence, they fit two parameters of 2D Ising model simulations, to replicate the distribution of cloud cover from satellite-observations.

Although I find the idea compelling, I cannot recommend the article for publication. I find that the connection of clouds to the Ising model requires more justification. The motivation offered is that clouds are self-similar, and the Ising Model also gives rise to self-similar structures. There is no discussion of what physical mechanism in the clouds could play the role of the Ising model's neighbor interaction J.

We agree with the reviewer that a proper description of the internal potential was lacking in the original submission. We added the following to the text:
The internal interaction potential represents the effects of sub-mesoscale processes that favor ($J > 0$) or disfavor ($J < 0$) could cluster self-organization or self-aggregation due to dynamical and thermodynamical processes such as turbulent mixing, gravity waves and radiation feedback ??????? while the external potential accounts for direct effects of the synoptic state variables on the bulk cloud field, represented by $\sum \sigma_i$.
Moreover, we conducted further analysis which shows that the model reproduces cord-length distributions, fractal dimensions, and cluster-size distributions that are in good agreement with the MODIS data, which further justify the use of the Ising model for cloud cover representation.

The only quantity compared between satellite observations and the Ising model is the cloud area fraction. After all the discussion of self-similar structures and distributions of chord lengths, I would like to see some deeper analysis of the clouds and the model, something that measures the spatial structures.

As mentioned above added we now include other metrics, namely, cord-length distributions, fractal dimensions, and cluster-size distributions and all show good agreement between the model simulations and MODIS data.

In general, I find the picture too simplistic. The cloud scenes considered are small pieces (31x31 pixels) of MODIS images. This misses cloud processes and patterns at both finer and larger scales. And then only the cloud area fraction of these small pieces are analyzed, ignoring all other spatial structure. The cloud scenes are analyzed by binning only according to a mean atmosphere temperature, which misses a lot of rich cloud behavior controlled by other large-scale quantities. This limited framing would be acceptable if the link to the Ising model could be made very strong and clear, which is not the case here in my opinion.

While we agree with the reviewer that our earlier analysis has focused only on CAF distributions and its variability, we disagree that these are small pieces of MODIS data implying that they are statistically insignificant. We note that each mesoscopic box consists of roughly 1000 pixels of MODIS data and we have 17×17=289 of them, over a long period of time, over

25 years (8 months per year, as monsoon season was excluded) of daily data. In fact, we mostly wanted to emphasis the fact the Ising model's capacity, via the multi-equilibria regimes, to capture the varying multi-modal distributions of CAF. Nonetheless, as already mentioned we now include new metrics, including cluster-size and cord-length statistics that clear go beyond the single CAF analysis, in term of representing micro-scale cloud organization.

Smaller remarks:

I'm hesitant on the remapping of the traditional sigma = -1 or 1, to sigma = 0 or 1 here. The model behavior remains the same, but the meanings of the parameters J and h change. With the traditional values, J gives an interaction strength, an alignment energy. With the 0 or 1 values, J also favors one value above the other, which traditionally is the role of h. This becomes important when trying to interpret the fitted values of J and h.

We agree with the reviewer that the role and thus interpretation of h and J changes radically from the $sigma = \pm 1$ setting. The $\sigma = 0, 1$ (known as the QUBO model in quantum computing/machine learning world) is more appropriate for cloud modeling in the sense that h now acts directly on the filling fraction observable, i.e, the bulk cloud field, while $J > 0$ directly favors self-organization. See our answer to first comment by this reviewer.

line 68. The term "order parameter" doesn't make sense here in the model definition. For a specific phase transition one could then identify an order parameter, but that seems not to be discussed in this paper.

The order parameter jargon is used for historical reasons, but the referee is right we don't need to. The whole half paragraph is edited and now, reads a follows.
"A binary variable σ is then defined on the microscopic lattice to indicate which lattice sites are cloudy and not cloudy. More precisely, σ takes the values 0 or 1 on each lattice site according to whether the site is clear sky or occupied by a cloud, respectively. This is illustrated in Figure 1 of KB19. For magnetization, the binary values are set to ± 1, representing atomic spin-ups and spin-downs, respectively, and σ is known as the order parameter in the context of phase-transition."

line 200. I don't see the point of using (4) for h, if h_0 is anyway fitted independently for each temperature bin.

Equation (4) sets the physical intuition that h is effectively proportional to the level of moisture saturation, which under saturation disfavor cloud formation while super-saturation favors it. The coefficient h_0 is the proportionality constant that needs to be fitted from data. We edited the text below that equation to make this point clearer.

line 208. The 20x20 lattice seems arbitrary and fairly small. The justification "to roughly match the MODIS images of 31x31" is not convincing. If one were to study something else than the cloud area fraction, for example chord lengths, this small size would be limiting.

The 20×20 lattice size was a typo. In reality, we used a 14$\times$24 lattice to exactly match the number of MODIS pixels within each ERA5 grid-cell. We respectfully disagree with the referee on the point that such lattice size is small in this context. One can argue that a large lattice is needed if we were to study phase transition near equilibrium, which requires a large lattice to come close to the meanfield dynamics. In fact, the cord-length and other finer statistical analysis, done for the 14$\times$24 lattice, which we now have added, provided a good match between the model and MODIS data, which confirms that the adequacy of the lattice size used.

line 209. How is it determined that the simulation has run long enough?

The simulation length was determined through convergence tests based on the temporal evolution of the mean CAF and its standard deviation. Figure 1 shows a representative exam-

ple in which these quantities reach a statistically stationary regime after an initial transient period. Similar behavior was verified for the simulations analyzed in this study, and the integration time was selected to be substantially longer than the corresponding equilibration time, with all simulations run for 3000 time steps. To ensure that the reported diagnostics are representative of equilibrium conditions, CAF statistics were computed using only the final 500 time steps of each simulation. Therefore, the reported statistics are not affected by the initial spin-up phase. The text was edited to clarify these details.

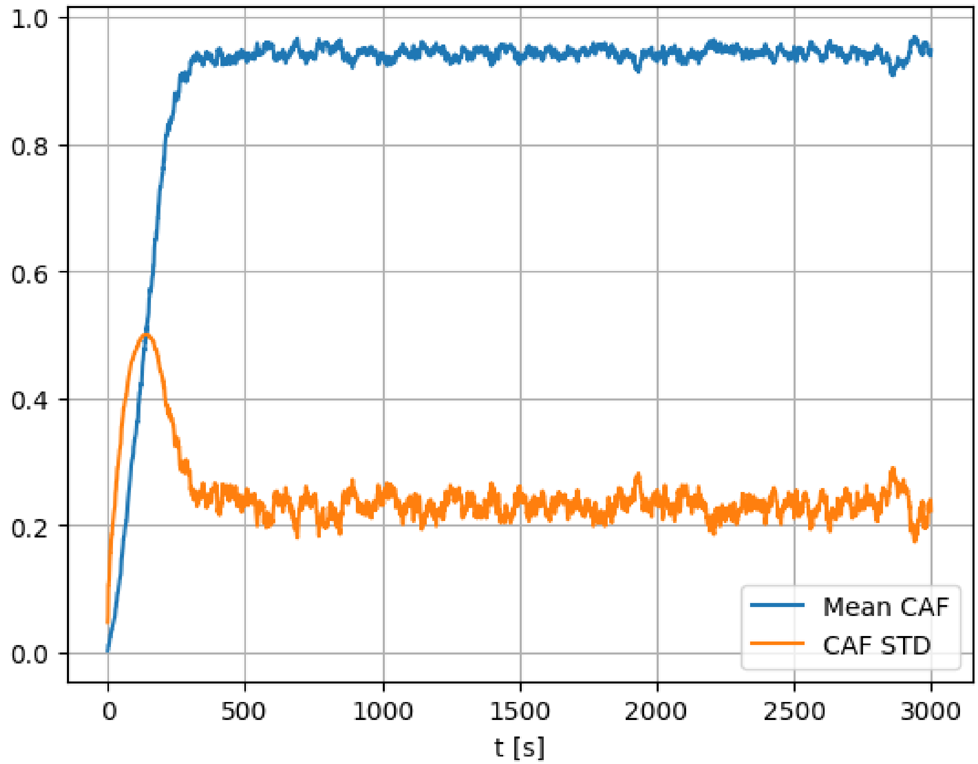

Figure 1: .

Code and data availability. In general, making the code and model data publicly available, permanently archived and citable by a DOI, is preferable over "available on request".

The code and data availability has been changed accordingly and now appear in the revised manuscript as : The code supporting the findings of this study is publicly available on a Zenodo archive (https://doi.org/10.5281/ zenodo.20546150). . . .