

## Review of Oh and Austin 2024:

The goal of this paper is to analyze the periodicity of vigorous versus quiescent periods in a trade cumulus region (CGILS S6). The slope of the distribution of large-cloud sizes, the convective mass flux, the cloud fraction, and the surface precipitation rate are all analyzed and the lead/lag relationship between these variables is used to hypothesize about the underlying physics.

I thought the paper was generally well written and the topic is interesting, but I have some concerns about the methodology.

### Major concerns:

1. I'm skeptical of your Gaussian process modeling (GPM).
  - a. I see spectral analysis as the gold standard for identifying periodic signals in a dataset - if the dataset has a signal at a certain wavelength, it *must* show up in a spectral analysis. I also disagree that spectral analysis is particularly sensitive to noise - noise usually shows up in a power spectrum as a lower limit to spectral power across a range of frequencies. It is true that this lower limit can be so large that it swallows your signal, but there are 2 peaks in Fig. 5 that rise above this floor. In this context it is hard to square the results of your GPM against what I see in your power spectrum or even via visual inspection of your time series.
  - b. I'm pretty sure all you are doing is effectively band-pass filtering the data by looking only for periods between 5 and 150 minutes which throws out the large signal at ~230 minutes and causes the secondary peak at ~100 minutes in Fig 5 to rise to the level of statistical significance because so much of the rest of the power spectrum has been thrown away. I could be wrong about this, but I would need to see my hypothesis proven wrong before I'd be comfortable accepting this paper.
  - c. I think my simple story in 3b is getting lost in a maelstrom of math. Einstein once said that "a model should be as simple as possible, but no simpler". In that context, why do you perform your analysis on the derivative of the signal rather than the signal itself (when there is no trend in the data to get rid of and subtracting the linear regression would be simpler if there were)? Why do you need GPM instead of applying a digital filter? I'd like to see clear justification for each step towards complexity you take.
2. I'm concerned that oscillations in cloud populations might be fairly local in nature such that averaging over a 45 km fetch averages out most of the variability you seek. Thankfully, you can easily address this question (which would be a nice addition to your paper)! In particular, can you repeat your analysis by only analyzing output over subsets of your domain? If so, does the peak frequency or spectral power change as a function of region size? Also, discussion of averaging over a large region occurs around

line 330, but I'd like to see more discussion and I'd like to see it show up in the experimental design section.

3. When I look at Fig 9, I see ~4 very clear waves with 95 min period while the rest of the timeseries doesn't fit this frequency very well at all. This leads me to suggest that your active/quiet hypothesis works well for some chunks of your simulation but something is interrupting the signal for other chunks. Can you watch a movie of your simulation or perform other analysis to figure out what is interrupting your behavior of interest? Finding conditions which disrupt these oscillations would be a very noteworthy contribution!
4. I'm also worried that the narrow dimension of your bowling-alley domain might be corrupting your results. It seems to me that the narrow horizontal direction would constrain the size of cloud radii. And since your whole paper centers around the cloud size distribution, locking the biggest clouds to the domain size seems like it could affect your conclusions. Can you reassure me that this isn't happening? Just providing citations of previous work showing this isn't a concern would be fine.
5. around line 105: I don't understand whether you're just taking cloud cross sections at a single level (e.g. 1 km) or whether you consider each vertical level to be an independent cloud. The latter approach could be problematic because clouds at different levels wouldn't be independent samples which would mess up statistical analysis tests. This would also distort interpretation because what we think of as a distribution of cloud sizes could in fact just represent the vertical structure of a single cloud. Can you clarify what you're doing and address potential concerns if you are using all levels?
6. There's a lot of focus on matching the periodicity found in previous studies, but do we know that such periodicity should be spatially and temporally invariant? Recharge/discharge seems like it would be proportional to boundary layer depth and the vigor of turbulent/convective mixing. Timescales might also be different in the different sorts of cloud analyzed by these previous papers.

Minor points:

1. It would be nice to see the timeseries of cloud size distribution, mass flux, and cloud fraction all on a single graphic. The degree of correlation between variables is difficult to see by flipping between graphics. A lead/lag correlation analysis would be a nice way to show the relationships between these fields.
2. A cartoon explaining the relationship between cloud size, mass flux, cloud fraction, and precipitation in terms of hypothesized regional life cycle might be useful for driving home what you're seeing.
3. I really like Figure 1! It does a nice job of grounding the paper in reality. It might be even more powerful if you showed snapshots like this for both a very active and very quiescent period.
4. The KDE gives you a non-parametric PDF which you can use to get the slope you need... so why do you introduce a power-law distribution? I suspect all you want is to compute the linear regression of the nonparametric KDE PDF in log-log space (after chopping off the nonlinear bit on the left-hand side of the curve in e.g. Fig 3). I suspect I'm complaining about your wording rather than what you've actually done.

5. I found Fig 3 to be a bit confusing. I would expect the derivative of the original plot to come below/after the plot it is the derivative of. It also took me a while to see the delta in front of the ylabel in panel a, which added to my confusion. Further, it would be reassuring to see several snapshots in time to make sure your log-log linearity assumption is justified in general instead of just in this test case. Alternatively, you could plot RMSE of fit (or something like that) for your linear regression as a function of time.

Typos:

1. L86 - I think "cell by 48" should be "cell INTO 48"
2. L221-222: you already said that noise is included above.
3. L262L you call timescale  $l$  here when it should be  $\lambda$ .
4. L503: "base" should be "based"