Review: Quantifying the Oscillatory Evolution of Simulated Boundary-Layer Cloud Fields Using Gaussian Process Regression

By Oh and Austin

This manuscript addresses the question of oscillatory behavior in the cloud systems. It emanates from earlier work that identified oscillations in cloud size distributions in open cell stratocumulus and shallow non-precipitating cumulus (BOMEX). It develops very nice, refined tools to differentiate the periodicity of the oscillations in noisy time-series data. Key results are that the authors find a similar periodicity in the slope of the cloud size distribution, the mass flux, and the cloud fraction (~ 93 min). The cloud size distribution oscillation is anticorrelated with the mass flux oscillation. The mass flux oscillation precedes the cloud fraction oscillation by half a period. The authors attribute the oscillations to charge-discharge cycles driven by cloud development and rain, respectively.

Main comments

The methodology developed by the authors is very rigorous and I appreciated the effort that went into finding methods to discern oscillatory signals in noisy cloud model data. I do feel however that the authors are missing some important context, and that they could have put more effort into developing a deeper understanding of what the oscillations are telling us. In my mind the paper could place more focus on interpretation. I recognize that this is a GMD manuscript and therefore that some of my suggestions may not seem pertinent. Perhaps the authors are planning a deeper analysis of the oscillations in a different paper. Nevertheless, I provide these comments as food for thought.

1) Quite a few studies that have discussed oscillations in cloud systems are referred to. These range from shallow cumulus (BOMEX) to the deeper CGILS case discussed here to open cell stratocumulus. I don't think enough distinction is made between these cases. For example, an open-cell stratocumulus system is characterized by significant internal coupling through colliding outflows associated with surface precipitation such that clear oscillatory behavior is expected. The 90 min periodicity in those systems is likely a time required for spatial rearrangement of the up- and down-drafts (or charging vs discharging areas). Shallow BOMEX clouds barely precipitate at the surface and are in a different class so that arguments about cloud-rain charge-discharge don't seem relevant, and certainly the fact that the signals are weak is to be expected. The CGILS S6 case precipitates more significantly and is quite different from BOMEX. Another study of precipitating Cu (10.1029/2019JD031073) shows that aerosols can change the charge-discharge time depending on the degree of clustering (e.g., Fig. 10).
   The paper would really benefit from a more nuanced discussion that discriminates between cloud types, cloud organization, precip amounts, and coupling in the cloud system. Note, I thinks this is important even for a GMD publication.

2) The imbalance between the more technical parts of the paper and the interesting discussion that starts on pg 16 could be corrected a bit. I felt that there were missed opportunities on the discussion to dig into the boundary layer physics. Examples: lines

412-414, lines 415-417, but I think there is much more that can be said in Sections 3.2, 3.3, and 3,4.
The figures require some work since one has to mentally superimpose plots of b and M, or pick off peaks and troughs in different plots to see that they are out of phase.
Also, because b is normalized, it should be made clearer that a smaller b is a more negative b, i.e. a larger fraction of small clouds. The plots contain important information but reference to them is too cryptic in my opinion.

3) What about the possibility of a charge (production of instability) – discharge (consumption of instability) as a driver of oscillations. https://doi.org/10.5194/acp-5-2749-2005. Is this how non-precipitating systems differ from precipitating systems?

4) Oscillations in M lead the oscillations in CF by 45-50 minutes. This made me wonder which size clouds contribute most to CF, which of course depends on b. I also wondered how detrainment at cloud top contributes to this (see Fig. 1). Could you strengthen this analysis and discussion?

5) Dagan et al. (2018) change their domain size and show that oscillations get smoothed out for larger domain sizes – at least for BOMEX. Have you tested the sensitivity of the periodicity to domain size. One could imagine that small domains introduce higher frequency oscillations because of the periodic boundary conditions. If the oscillations become harder and harder to discern at large domain size, are they really important? The fact that they can be discerned by your GP regression is very nice but they may not have much physical significance in a natural cloud system unless the system experiences strong internal coupling (e.g., open-cell Sc).

Other:

6) It would be nice to know how the normalized b values translate to actual b values that e.g., a satellite imager would see.

7) Line 132: could you help with physical meaning of 'bandwidth h'?

8) Line 102, $q_l > 0$ is a very low threshold, unless your cloud edges are very sharp. Does $q_l > 0.01$ g/kg change the picture?