

Review of egosphere-2026-2160

General remarks

This paper presents a detailed observational analysis of “cell size”, a metric derived by spectral analysis from satellite proxies of low-level cloud-liquid water path in the South-East Pacific. The analysis expands on previous, similar analyses, by studying a longer time period and geostationary satellite data. This allows characterizing the diel cycle of cell size and its normalization with boundary layer depth (aspect ratio), and leads to the discovery of a distinct diel evolution of aspect ratio, which is independent of year: Growth in during the morning, an afternoon plateau, an early evening decay and a stable night. This daily evolution is further broken down by cloud fraction, revealing the pattern to be present across cloudiness regimes, with further interesting differences.

To me, this is a well-structured, well-documented characterization of stratocumulus cells, which raises many new and interesting questions: Why do the cells follow this particular daily evolution? Why is their aspect ratio so robust? Why do they behave differently across regimes of cloudiness? The manuscript is thus likely to spark much further research. I therefore think it is highly suited for publication in ACP, pending a list of mostly minor comments.

My most pressing concern regards the methodology: I am not fully convinced that the discretization error introduced in the estimate of cell size does not project into the main results section. I would thus encourage the authors to demonstrate that their choice of method to extract a cell size from the fields does not meaningfully alter the conclusions they most wish to communicate. Please see my comments below for further detail.

Specific remarks

I get the feeling at some points in the manuscript that a large-language model has assisted the writing. If this is so, please acknowledge it transparently, and please be specific about how it has been used.

Line 88: Do the authors expect their analysis to have a seasonal bias if they focus on two months only? Please explain.

Line 108: Could the authors please expend a sentence to explain what it is about the differences in the 12.3 and 10.3 micron T_b products that makes their difference suitable for detecting low-level clouds?

Line 126: Could the authors briefly justify the choice of 300 km? It makes sense as a scale sufficiently far removed from the expected scale of the cells (10 times smaller), but why not choose larger cells? It appears around line 205 that larger domains might have allowed identifying larger structures. So: Do the authors expect the results to be sensitive to their choice of scale?

Line 146: Could the authors clarify which cloud-top height product is being used here?

Line 161: Can the authors justify retaining fields with 44% missing values? They say “interpolation does not affect large-scale organization”, how was this verified?

Line 183: $P_x(k)$ as presented in equation 1 is not a variance spectrum, but the power-spectral density, i.e. its integral is the variance. Do the authors now actually use $P_x(k)$ or simply the variance in each wavenumber? Both may serve the purpose the authors seem to intend, but the interpretation for why the method is chosen would differ slightly. Could the authors please clarify, and then update the terminology used throughout the section (Power, PSD, variance) so it is consistent?

Lines 190-195: Binning and then interpolating in the spectrum introduces discretization uncertainty in the determined cell scale. How large is this discretization uncertainty? If it is on the order of a few km, it may not matter, but uncertainties of order 10 km would presumably affect the reliability of the results quite profoundly. I therefore also do not believe in the statement on lines 199-200.

Figure 3: In interpreting the scale identification routine’s skill, it would be helpful if the two leftmost columns had some indication of the length of the extracted dominant length scale, e.g. a line with the length of the dominant wavelength.

Figure 4: The overestimation of $\lambda \Delta T_b$ at larger cell sizes occurs at discrete intervals. That usually indicates artefacts due to discretization (why can the method not identify any cell sizes between 50 and 60 km)? I agree that the overall fit is satisfactory, but the joint pdf indicates such clustering around several other sizes that would make me additionally suspicious about the impact of the aforementioned discretization error of the scale extraction routine.

Lines 243-248: Are these CTH products also those used to filter the scenes for high clouds? And how is the method of Eastman et al. (2016) implemented? Please elaborate.

Line 250: Can the authors explain why the GOES-East LWP spectra have such a low detection rate, with respect to all the other detection methods, which seem similar?

Line 276: Can the authors also reflect on the large bias in AR between the platforms? That is, can the authors clarify whether they believe this study actually can quantitatively nail down what the aspect ratio of stratocumulus cells is? Or is their conclusion rather that we do not quantitatively know the actual values, just the variability.

Line 330 (and later in line 391): This discussion would be more powerful if the authors would actually demonstrate that there was measurably lower stability, warmer SST and deeper BLs in the El Niño years (a reference would probably do). The authors almost seem to suggest that boundary layer height does not respond to these factors as expected, as they do not find the year-to-year variability in BL depth that they expected. Highlighting if this is so could be of value.

Line 340: The authors seem to already speculate on the dynamics that control the daily evolution of λ_{LWP} here. Yet it is not clear to me that this is already justified. I would have stuck with a statement like “their imprint of the diurnal evolution of cell size must remain secondary to the dynamics that cause this evolution”.

Line 375: It might be interesting for the authors to reflect on why near-coastal cells should behave differently, because it may offer clues as to what controls the diurnal evolution in cell size.

Line 423: The plateau could still be a general feature of cells however?

Lines 433-438: I could use a more precise discussion here of how you interpret these results. I am left with many questions:

- Why do high-CF scenes have smaller cells?
- Why do they grow more rapidly during the day?
- How can the cells in Q1 still grow, even as the cloud fraction reduces heavily during the day?
- What does the normalization with boundary layer height imply for the relationships between cell size and cloud fraction?
- What aspect of the cloud-cover dependent evolution should I interpret to hold robustly independent of dataset?

I am not saying this paper needs to answer these questions, but it does raise so many interesting questions that I think it would help listing them more explicitly at the end of this section than is currently done.

Line 456: I would emphasise that this is true also for the diel cycle

Line 460-462: Do you mean interdependence with MBL *height*?

Section 7: Also in this section, I would highlight the range of secondary questions raised by your research, such as my list above, but most fundamentally: What processes control the diel evolution of AR?