**Review 1**

**Comments from the reviewers are reproduced below in black, while our responses are in blue.**

**Review of egusphere-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.**

First of all, we would like to thank the reviewer for his/her appraisal of our work and the many positive and useful suggestions.

Please note that we have also modified the title of the article, changing “stratocumuli” to “stratocumulus” since it represents more as an adjective rather than a noun in this context. Additionally, the Abstract has been revised to highlight the temporal evolution of the AR rather than its absolute values, ensuring alignment with the reviewers' feedback and the nuanced discussion in our results.

We answer the remarks, point by point in the following.

**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.

Thank you for your remark. This is indeed an important issue, given the increasing use of LLMs in scientific writing. However, no LLM was used to generate the content of this manuscript. Its use was limited to minor rephrasing aimed at improving lecture. We do not believe that such limited assistance needs a specific acknowledgement.

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

The choice of August and September was motivated by the maximum occurrence of Stratocumulus (StCu) and their cellular patterns over the region (Muhlbauer et al 2014), since these structures are our focus of interest. As a consequence, other seasons can indeed exhibit distinct structural and diurnal characteristics. To illustrate this, we computed the daytime evolution of λLWP and ARLWP and the full diurnal cycle of λΔTb and ARΔTb for January and February (2021–2026), which corresponds to the austral summer (Figures R1 and R2).

During this summer period, it has been shown the South East Pacific experiences a minimum in StCu occurrence (Muhlbauer et al 2014), leading to a reduction in LWP. As a result, Figure R1 shows a similar daytime trend but characterized by wider cells and a reduced amplitude. Similarly, the diurnal evolution in Figure R2 is less pronounced than during the winter months (Figure 8 of the manuscript) and appears more sensitive to radiative biases, with visible artefacts at sunrise and sunset.

These summer observations are highly analogous to the results obtained for the Q1 Cloud Fraction regime presented in Figures 12 and 13 of the manuscript. During summer, the cloud cover is reduced, stratocumulus decks tend to break up, and disorganized and/or Cumulus-like regimes would become dominant. These strong similarities (radiative biases, wider cells, weaker diurnal amplitude we discussed in the manuscript) confirm that the mean summer diurnal cycle is strongly influenced by the prevalence of this Q1 regime.

Furthermore, the wider confidence envelopes in both Figures R1 and R2 reflect the lower number of detected cells compared to the more cloudy winter period (August–September).

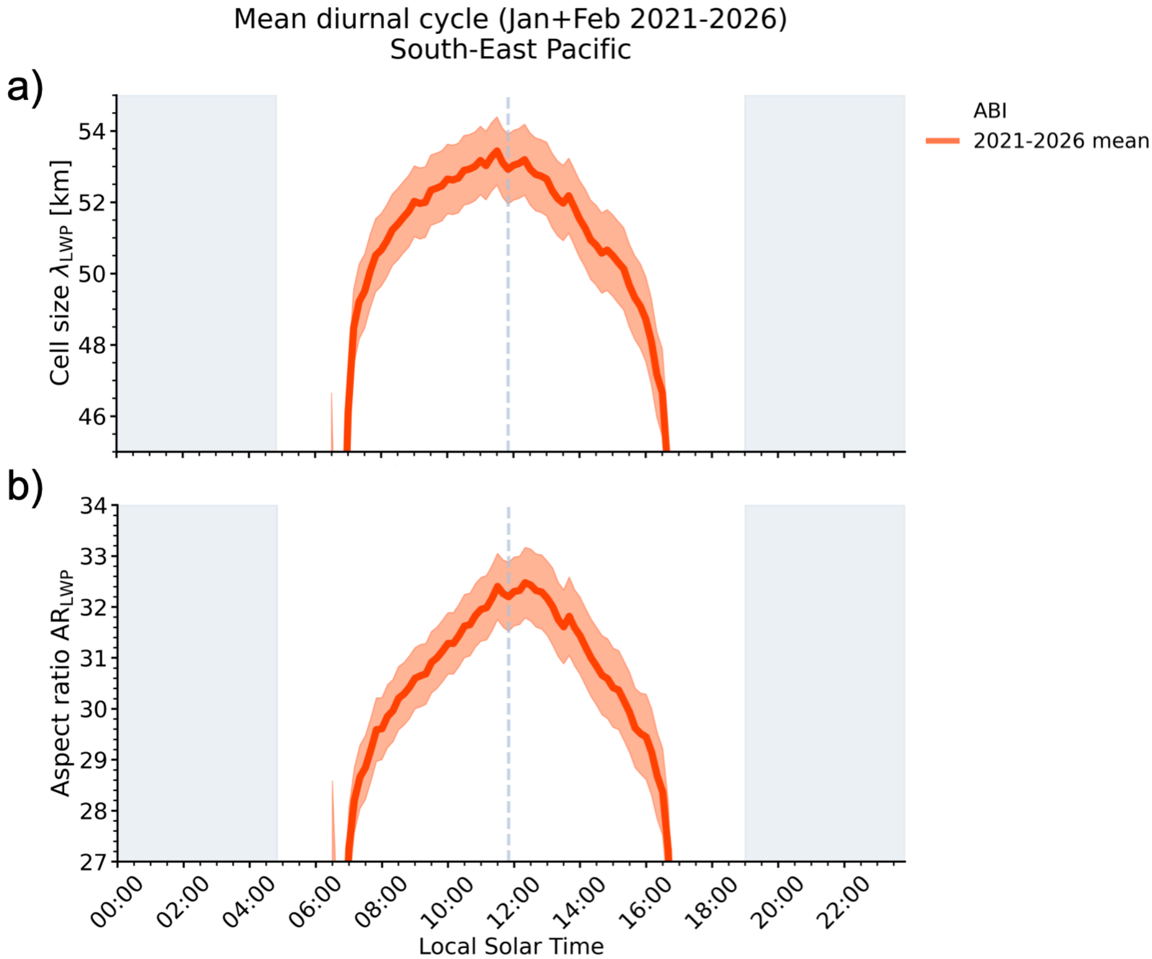



**Figure R1:** Same as Figure 6 of the manuscript but for the two months of January and February for the 2021-2026 period.

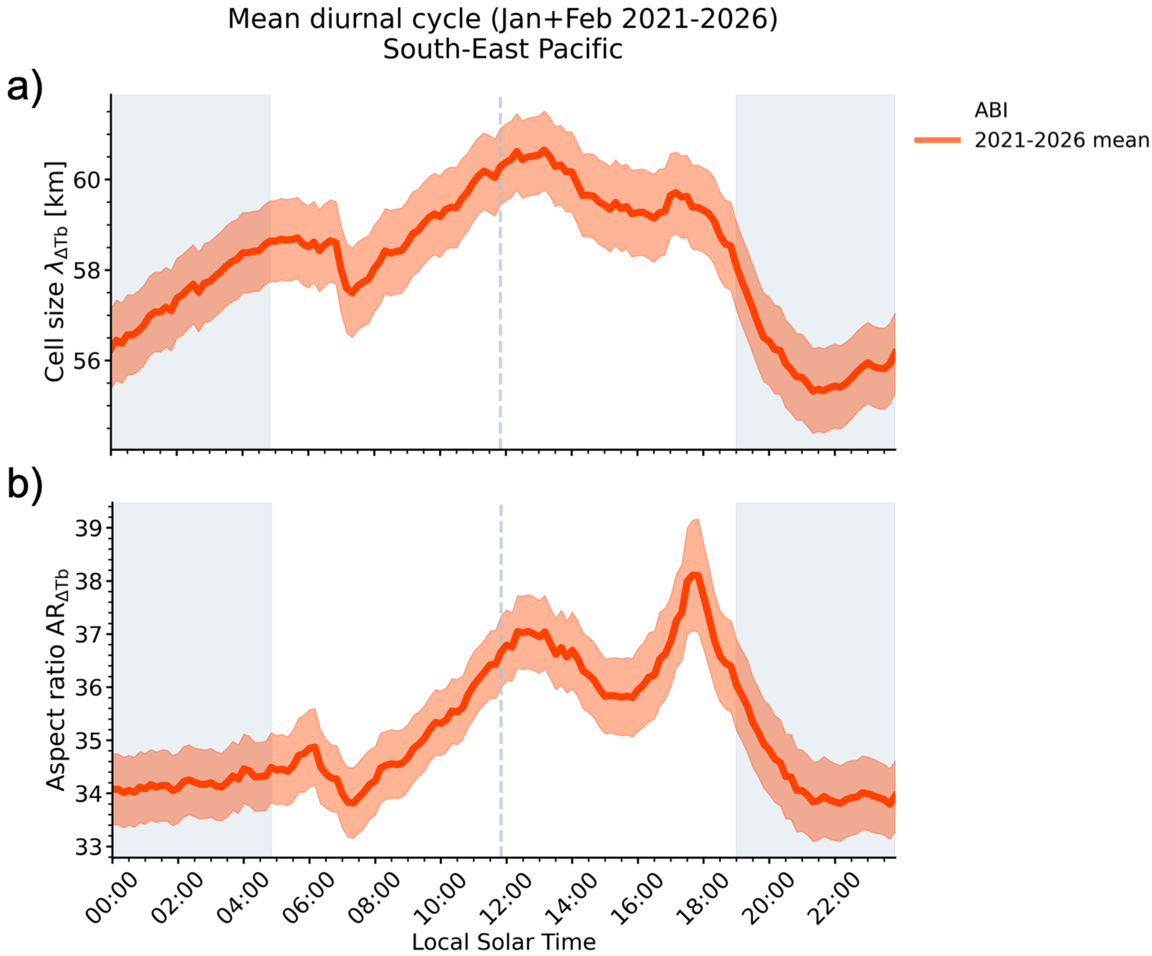



**Figure R2:** Same as Figure 8 of the manuscript but for the two months of January and February for the 2021-2026 period.

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 $Tb$! products that makes their difference suitable for detecting low-level clouds?

To clarify this choice, we add a sentence in our methodology "The 10.3 micron channel is where the water vapor is minimum and is useful to detect clouds but of all types, the 12.3 micron has more absorption of water vapor at the lower levels. The difference between the two channels emphasizes low-level clouds and their spatial heterogeneities. Thus making it effective for stratocumulus detection." around Line 110 of the revised manuscript.

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?

The choice of 300km has been based on previous literature to compare with state of the art,  and has been shown to be suitable to detect organization around cell size. We also have tested the sensitivity of cell size detection regarding the size of scene. For a set of scenes (66 cases), we have derived at the cell scale from 600kmx600km scenes, and compared it to the cell scales derived from the 4 corresponding 300kmx300km scenes (Figures R3 and R4).The methodology is able to detect the same small scales we detect in the smaller scenes and increasing the size doesn't impact small scales detection.

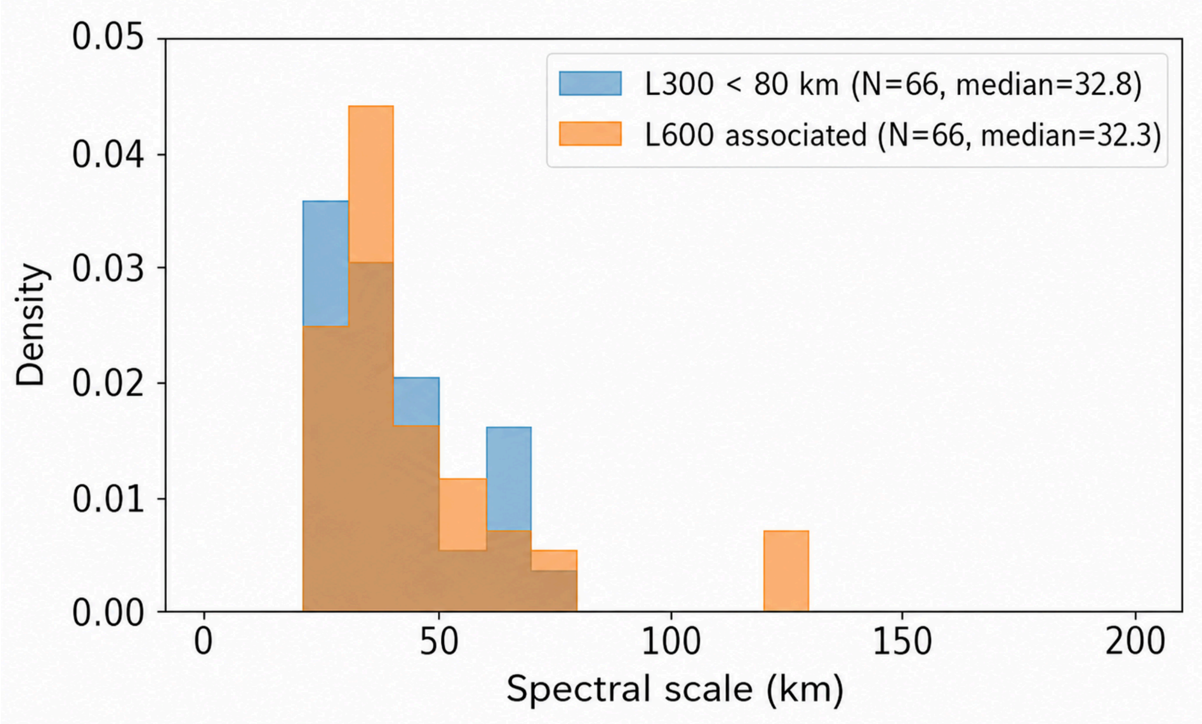



**Figure R3:** Cell scales computed for a set of 66 300km side scenes and the corresponding 600km side scenes. 95.5% of the associated 600km side scene still detect a cell size inferior to 80km we observed at 300km side.

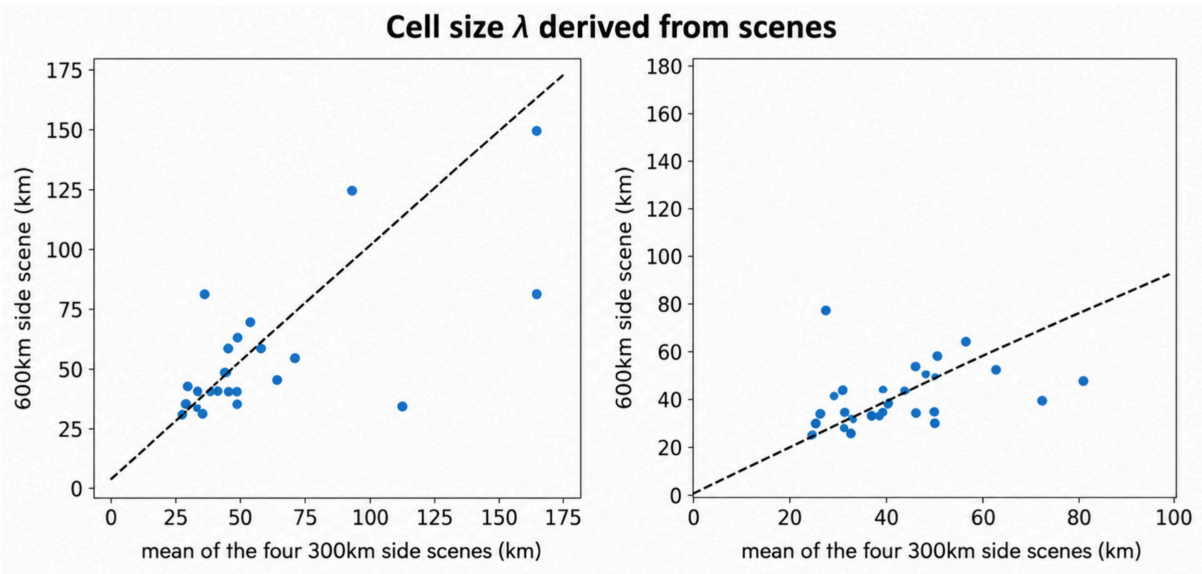


**Figure R4:** Correlation between the cell size derived from the 600kmx600km scene and the mean of the cell sizes obtained from the 4 corresponding 300kmx300km scenes. Correlation is even better when restricting the analysis to $\lambda \leq 80$km: $R^2$ are 0.45 for the full set (left) and 0.68 after restriction under 80km (right).

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

We use the variable 'Cloud Top Altitude' from the L2 Product CTTH: Cloud Top Temperature and Height, computed by the Nowcasting Satellite Application Facility (NWC SAF) from EUMETSAT.
These data are available free of charge, after creation of an account:
https://www.nwcsaf.org/ctth_description

We thank you for your watchfulness, we add the information in the Data Availability section.

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?

First, note that missing values mostly affect the LWP field and not the $\Delta$Tb one, which are the most widely used data in our article. For the LWP, it is due to issues we mentioned in Section 2.2.3 of the manuscript such as thin clouds or clear sky zones. We didn't want a higher percentage ($\geq 50\%$) since it would mean we "created" more information than we have from the retrieval.

Actually, when looking at the full 6 years dataset, the preprocessing step (with the cloud mask and setting LWP to zero in clear sky zone) is filling most part of the cloud scenes. We end with a very small amount of scenes with missing values, and if so there are in a very small proportion (see Figure R5). Therefore the 45% threshold is maybe too cautious but using a smaller threshold (10% for instance) won't change our results since the holed scenes mostly contain less than 5% of missing values. We add a sentence (Line 171 of the revised manuscript) to clarify this.

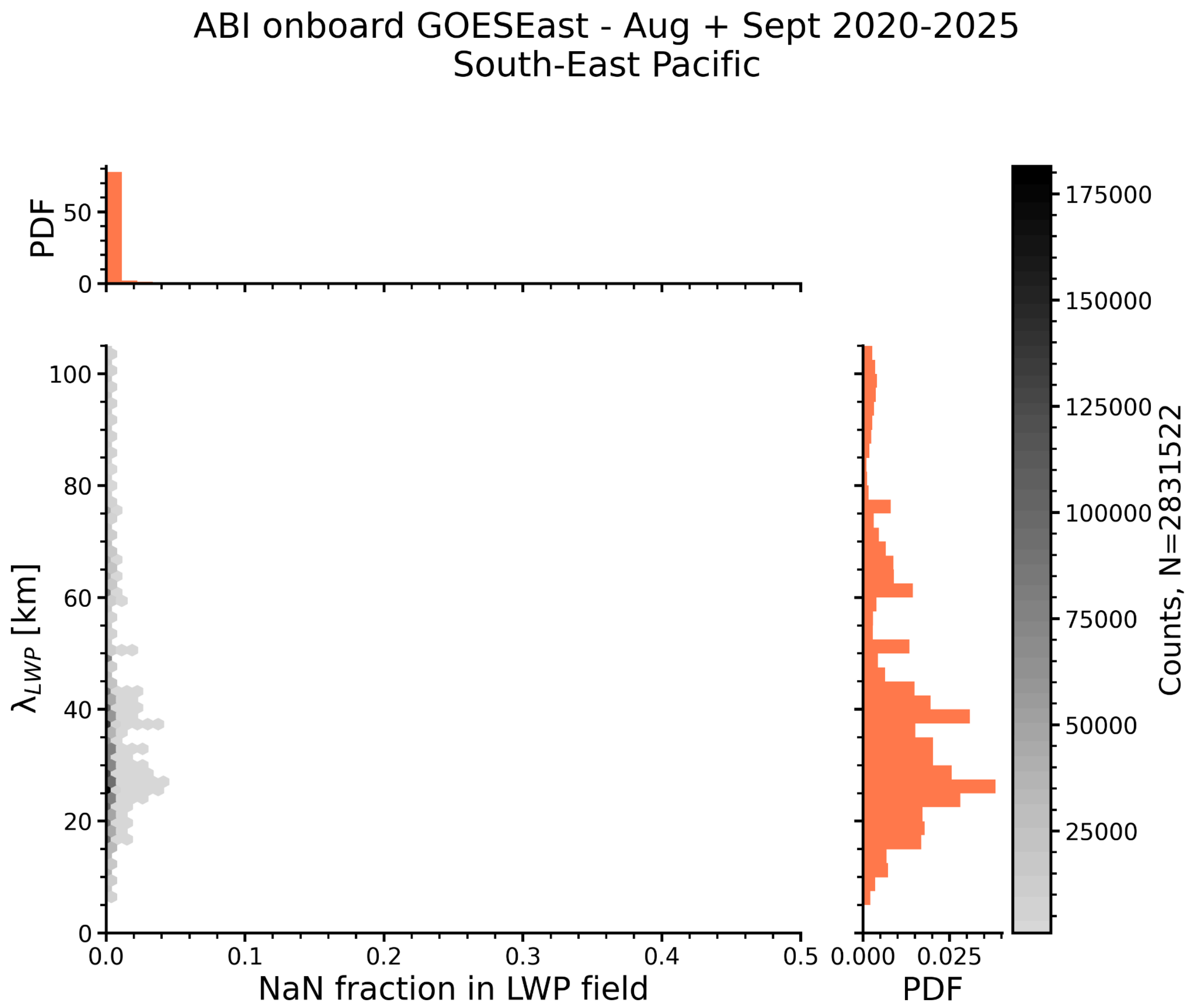



**Figure R5:** Values λLWP computed in view of proportion of missing values of the cloud scenes. Computations were performed for each scene individually, for the full 6 years dataset.

Line 183: Px(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 Px(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?

Thanks for your comment. We understand the ambiguity of the vocabulary used here.

For our computations, we utilize Px(k). Following standard practice in previous studies, we maintain the terminology of 'power spectral density' throughout the remainder of the article. Furthermore, we slightly adapted the explanation provided around Line 190 of the revised manuscript (Line 183 of the old one), hoping it clarifies well enough the relationship between variance and power spectral density.

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.
We have carried out several tests, which we summarize below.

They aim to show that the main characteristics of the diurnal cycle we present in the manuscript are not affected by either binning or the intrinsic discretisation associated with the Fourier transform: only the mean values of the metrics are affected, but the evolution pattern and dynamics remain the same.

Here, we firstly have investigated the sensitivity of lambda retrieval to the number of bins in the averaged spectrum. And secondly its sensitivity to the polynomial fit method.

The uncertainty associated with the characteristic spatial scales was estimated from both the spectral resolution of the Fourier analysis and the spectral binning. For a 300-km side domain, the wavenumber spacing is fixed at $\Delta k = 0.0209$ rad km$^{-1}$. For spatial scales ($L = 2\pi/k$), this gives an uncertainty ranging from approximately ±0.2 km at L = 10 km to ±10.7 km at L = 80 km. Because the analysis is restricted to scales smaller than 80 km, the uncertainty associated with the discrete Fourier spectral resolution remains below ~13% and is typically between 4% and 10% for the 20–60 km scales that dominate the results. We further develop on this discretisation related to Fourier and illustrate it, in your next comment on Figure 4 of the manuscript.

The spectra are then averaged with logarithmically spaced bins, in order to reduce the variance or noisy pattern inherent to Fourier spectral computation.

To give a further argument on the binning method,  we show here an application of our methodology of cell size detection for 6, 8 or 10 bins in the averaged spectrum. The distributions of λ remain comparable (Figures R6 and R7) and the binning doesn’t impact much the cell size detection.

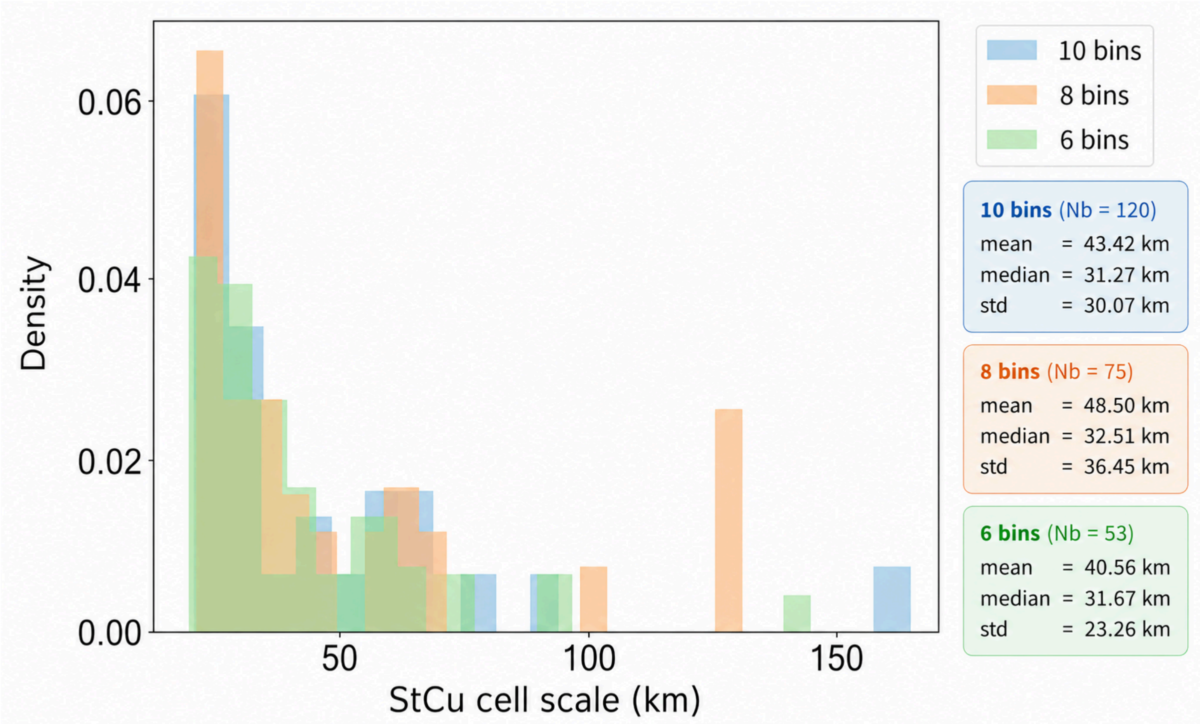



**Figure R6:** Distribution of the cell size derived from our binning + interpolating method, with 6, 8 and 10 bins to average the spectrum. To be noted, we restrict our analysis to cell scale inferior to 80km.

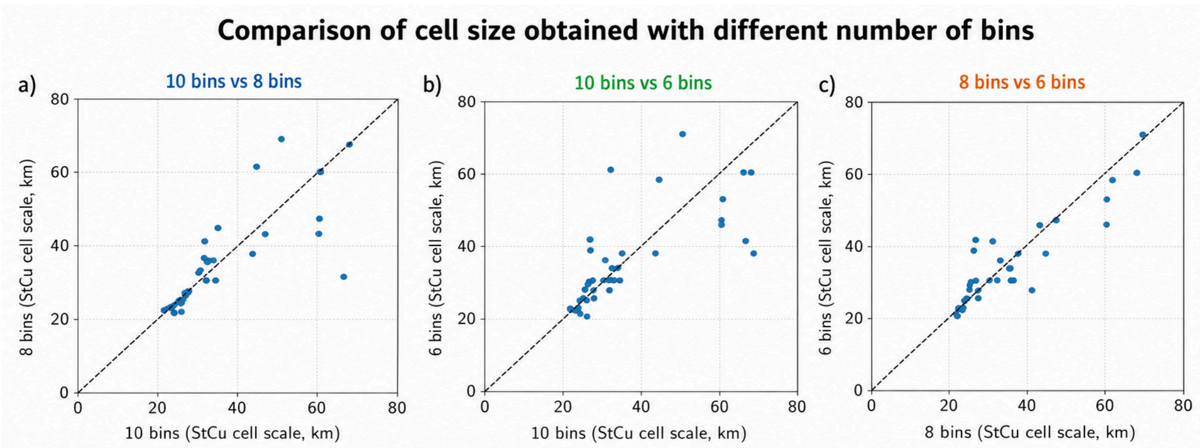



**Figure R7:** Same cell sizes as the previous Figure, but looking at the correlation between the different binning approaches. Satisfying correlation is seen.

Nevertheless, we ended up using the maximum number of logarithmically spaced bins allowed by the available wavenumber sampling in order to preserve as much scale information as permitted by the spectral resolution, so 10 bins. This choice minimizes additional discretization introduced by the averaging.

We can quantify as well the uncertainties related to the binning. The corresponding wavelength range is approximately ±24% around the bin center (the 66-km bin encompasses roughly 53–82 km). However, if the bin width constitutes a large source of uncertainty, its impact on our results is monitored thanks to the polynomial fit we perform over the 3 bins interval at the local maximum. Figure R8 shows the mean diurnal evolution of cell size obtained with and without the polynomial fit step (only taking the raw discrete values of wavelength in the local maximum bins interval for b) may affect the mean values of λ or AR but not our conclusions on the diurnal evolution.

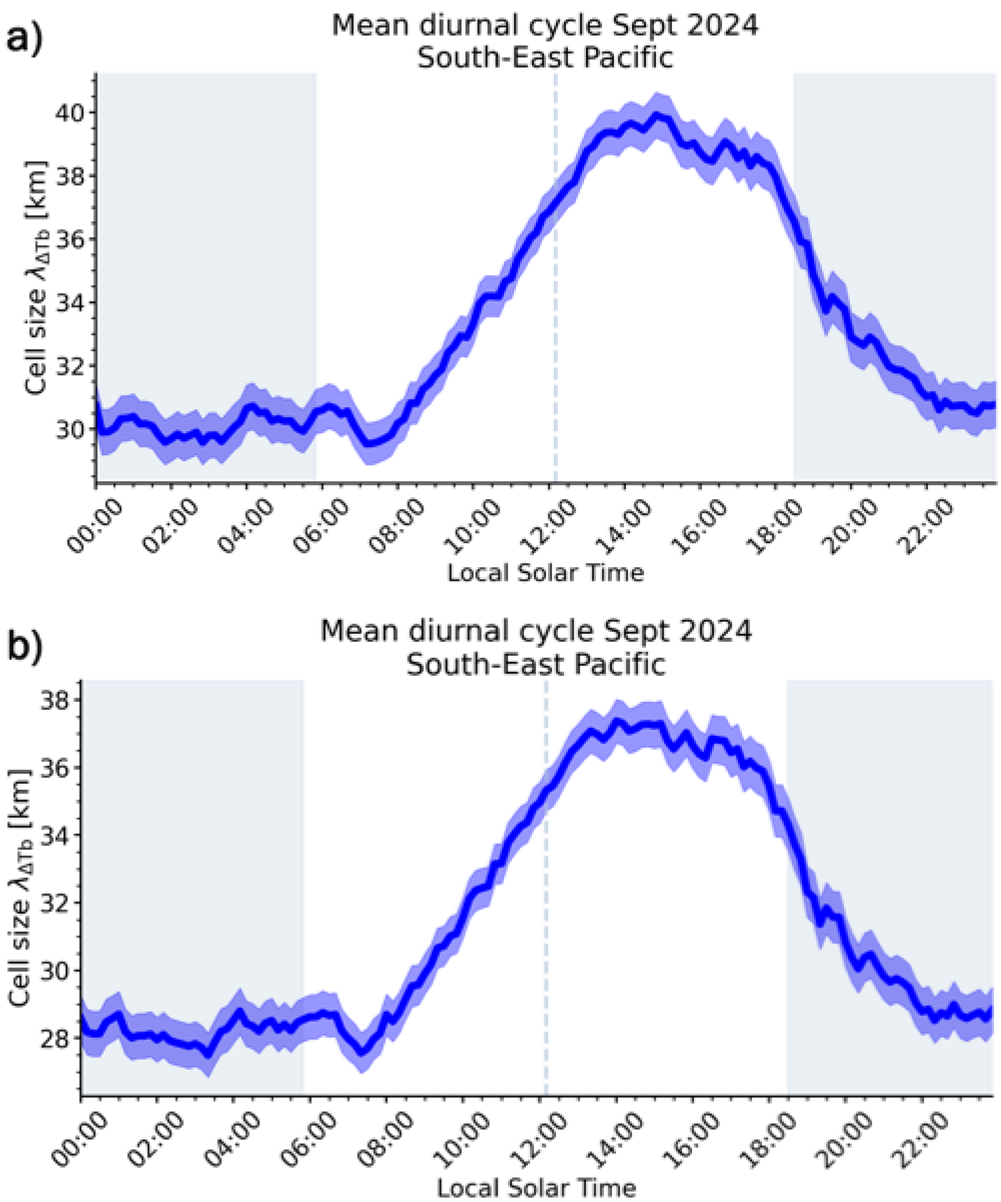



**Figure R8:** Same as Figure 8a of the preprint but considering cell scale derived from a) the polynomial fit based of the raw values inside the bins interval b) without the fit, taking only the raw wavelength corresponding to the maximum of variance. Computations done for the month of September 2024. Similar results are seen for interannual and inter CF-regime diurnal evolution.

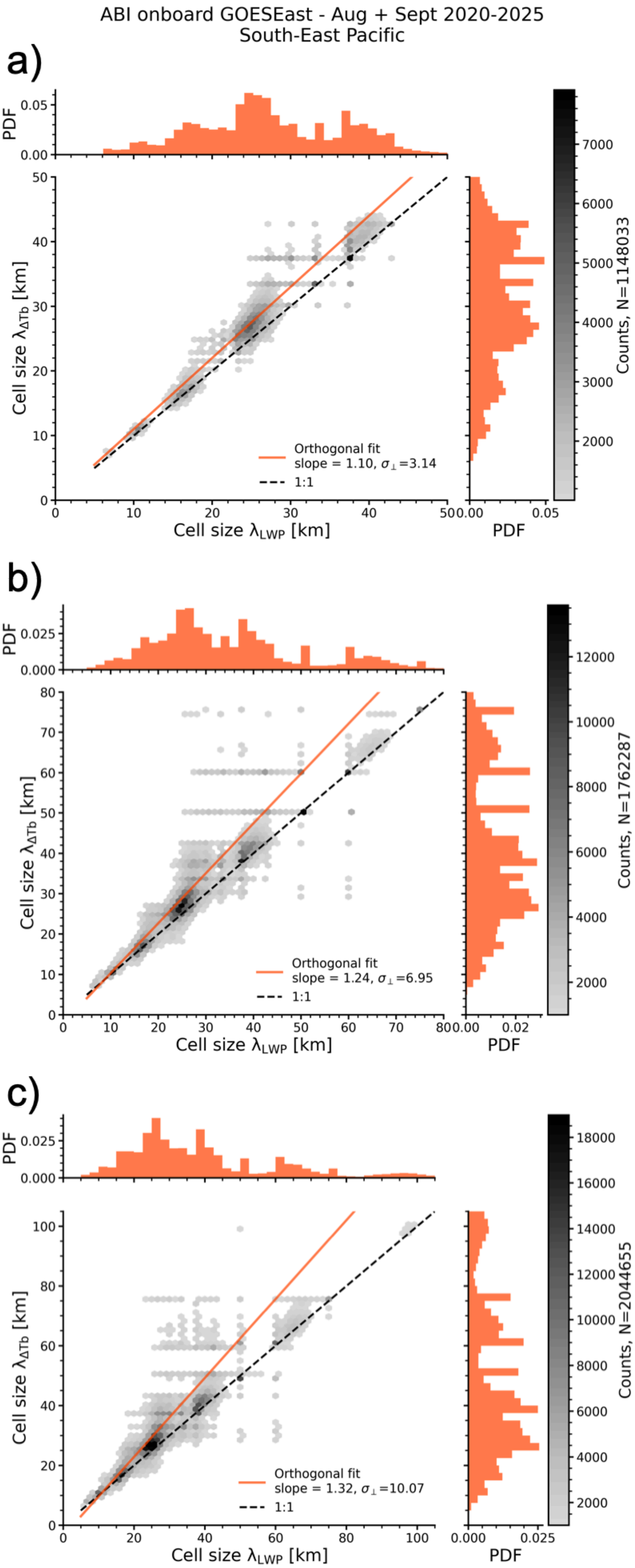
Figure 3: In interpreting the scale identification routine's skill, it would be helpful if the two leftmost columns had some indication of the length of the extracted dominant length scale, e.g. a line with the length of the dominant wavelength.
Thank you for the advice, it is true it would help the understanding with these examples. We add this information on Figure 3 of the manuscript.

Figure 4: The overestimation of λ ΔTb 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.
Because of the Fourier transform, the spectral decomposition gives a spatial range equally spaced in k (approximately 0.02 rad/km space) and so equally spaced in 1/λ. The discretization we see here with the cell size more clustered around typical values of 50, 60 km is a direct consequence of the spectral decomposition, and the uncertainties become larger for larger wavelengths as well, we have quantified these incertitude and have given arguments in the review above. These discrete values can be spotted in Figure R9, where the typical values from the Fourier transform arise as clustered values when computing λ in LWP and ΔTb.

However, this discretization doesn't impact the main conclusions of the manuscript as it detects quite well the behaviour of the StCu cells. Figures R10 and R11 show that for various thresholds in cell size (λ≤50, λ≤80 and λ≤105km), the mean values of the metrics change a bit (less for AR then for λ), as expected, but the diurnal tendency remains the same.

Alternative approaches based on object identification, such as watershed segmentation (Gufan et al., 2016 https://doi.org/10.1080/2150704X.2016.1142681

), can provide more continuous estimates of cloud sizes. However, such methods rely on user-defined parameters (thresholds limit for object contours, smoothing parameter, …) that largely influence the retrieved size distributions and introduce methodological biases that are difficult to quantify objectively. In contrast, the spectral approach we present provides a reproducible estimate of characteristic spatial scales ,with fewer parameters, and with uncertainties that can be directly quantified from the spectral discretization and binning procedure.



ABI onboard GOESEast - Aug + Sept 2020-2025
South-East Pacific
a)
PDF
Cell size λΔTb [km]
Cell size λLWP [km]
Orthogonal fit
slope = 1.10, σ⊥=3.14
1:1
Counts, N=1148033
b)
PDF
Cell size λΔTb [km]
Cell size λLWP [km]
Orthogonal fit
slope = 1.24, σ⊥=6.95
1:1
Counts, N=1762287
c)
PDF
Cell size λΔTb [km]
Cell size λLWP [km]
Orthogonal fit
slope = 1.32, σ⊥=10.07
1:1
Counts, N=2044655

**Figure R9:** Same as Figure 4 (main text) but with different thresholds of maximum cell scale a) λ ≤ 50 km, b) λ ≤ 80 km and c) λ ≤ 105km. λ ≤ 50km represents approximately 65% of λ ≤ 80km and 56% of λ ≤ 105km. λ ≤ 80km represent approximately 86% of λ ≤ 105km.

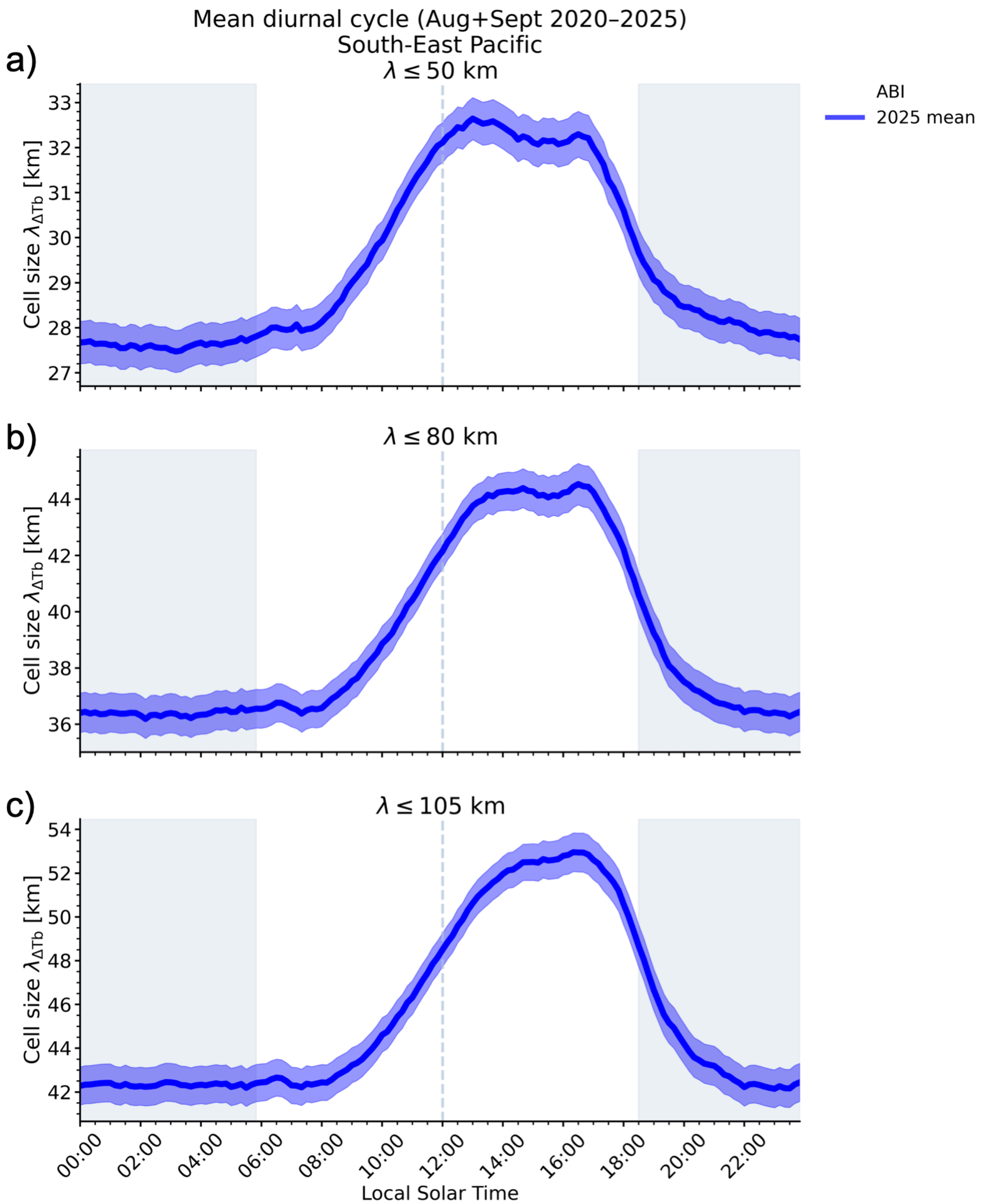


**Figure R10:** Same as Figure 8a of the preprint but considering cell scale a) λ ≤ 50 km, b) λ ≤ 80 km and c) λ ≤ 105km. Similar results are seen for interannual and inter CF-regime diurnal evolution.

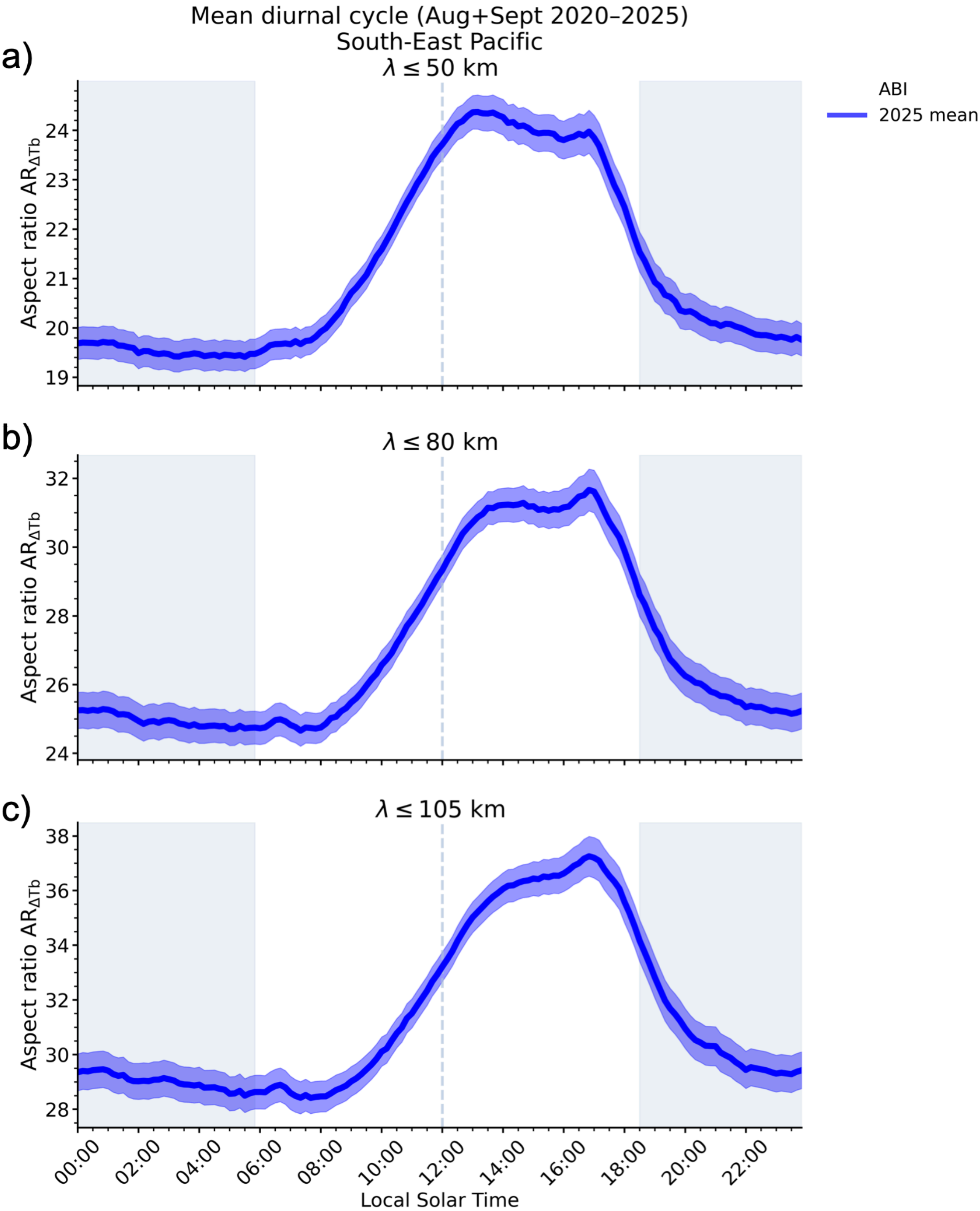



**Figure R11:** Same as the previous Figure but for ARΔTb. Similar results are seen for interannual and inter CF-regime diurnal evolution.

We hope our answer to the two reviews regarding the cell size detection convince you the methodology is robust explaining the temporal evolution of StCu patterns.

We consider the discretisation, the binning and the restriction to λ ≤ 80km don't change the main conclusions of the article and still show the robustness of the AR invariance. We have taken this into account and nuanced our words in the abstract and the conclusion of the revised manuscript , insisting more on the temporal evolution pattern rather than the values of the metrics.

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.
Yes, we used the same variable 'Cloud Top Altitude" from the NWC SAF CTTH Product, mentioned in Data Availability.

We have provided more details for the Eastman et al. (2016) method in the Appendix.

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?
The detection rate is particularly low because data only available at daytime, and percentage computed for the full day for GOES-East. We added a sentence explaining this in the caption of Table 3 of the manuscript.

Line 276: Can the authors also reflect on the large bias in AR between the platforms?
We believe the difference in retrieval may be due to the higher sensitivity of MODIS near clear sky area and at cloud edges, which lower the cloud top height. Thus, the MBL value is complicated to estimate with MODIS but we have confidence in the one computed by ABI. We tackle these differences in the Appendix, especially mentioning the method of Eastman et al. (2016) that should compensate for these discrepancies. This method largely improves the MBL depth estimation, giving more coherent results regarding the link between the cloud cover and the MBL depth (higher cloud fraction cases in shallower MBL). Some offsets with ABI/GOES estimations are still seen, majorly due to the parameters and hypothesis used in Eastman et al. (2016) method.

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.
Yes, it is true that our goal here is to insist on the robustness of the diurnal evolution and the AR invariance rather than the mean values themselves. We have seen than the cell size values can be slightly sensitive to the discretisation, the instrument retrieval or even the chosen variable for derivation (LWP or ΔTb), but in any cases

the main conclusions remain valid: the diurnal cycle look alike and the unifying behaviour of AR is found.

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 Nino 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.
Figure R12 shows the 2020–2025 period reveals a distinct thinning of the MBL, which reached a notably low altitude in 2023 and decreased even further during the peak of the event in 2024. This observed trend supports the atypical large-scale thermodynamic forcing documented during the special 2023–2024 El Niño. While El Niño events traditionally deepen and decouple the MBL through a warmer SST and a weaker lower-tropospheric stability (Bretherton et al., 2010; Wood et al., 2014), it has been shown the 2023–2024 anomaly was majorly dominated by remote atmospheric feedbacks rather than traditional local trade wind-SST coupling (Zhang et al., 2024).

This study has shown 2023–2024 El Niño event was generated by internal ocean dynamics. These dynamics led to an enhanced convective activity in the equatorial Pacific and intensified large-scale subsidence over the SEP subtropical gyre. This mechanical and thermal compression from above significantly strengthened the Estimated Inversion Strength (EIS), acting as a strong atmospheric 'cap' that prevailed over the buoyant forcing from the warm local SST anomalies (Myers et al. (2021) and Qu et al. (2014)). Consequently, as the El Niño event matured from late 2023 into mid-2024, this persistent upper-level warming forced the MBL to remain exceptionally shallow and compressed.

This provides a physical explanation for the decreased values of 2023 and 2024 outliers, we captured in our 2020–2025 dataset.

We have integrated this physical explanation and the Figure in the Section 4.2. We also recall it when talking about interannual variability with ΔTb in the end of Section 5.3.

**References:**

**Bretherton, C. S., et al. (2010).** Marine boundary layer over the subtropical southeast Pacific. *Atmospheric Chemistry and Physics*.

**Wood, R., et al. (2014).** Ocean–Cloud–Atmosphere–Land Interactions in the Southeastern Pacific: The VOCALS Program. *BAMS*.
**Zhang, L., et al. (2024).** Strong 2023–2024 El Niño generated by ocean dynamics. *Nature Geoscience*.

**Myers, T. A., Scott, R. C., Zelinka, M. D., et al. (2021).** Observational constraints on low-cloud feedback reduce uncertainty of climate sensitivity. ***Nature Climate Change***, 11(6), 501-507.
**Qu, X., Hall, A., Klein, S. A., & Caldwell, P. M. (2014).** On the spread of Amundson-plus-baseline low-cloud feedbacks in climate models: the role of the properties of the remote tropical ocean. ***Climate Dynamics***, 42, 2985–3003.

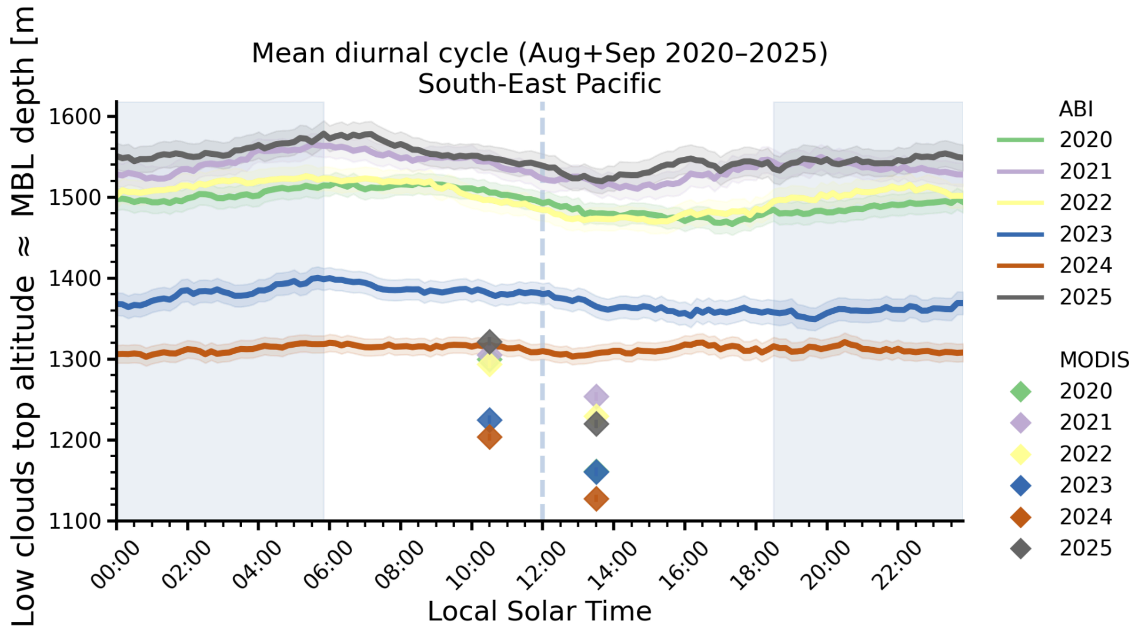



**Figure R12:** Diurnal evolution of MBL depth in SEP region with the ABI/GOES-East across the years from 2020 to 2025.
Each color represents data for a year. Line is the annual mean from ABI with its standard error envelope. The color matching diamond markers represent the mean and standard error obtained with MODIS/Terra and MODIS/Aqua for the same year. Uncertainties on diamond markers may not be visible due to marker size.

Line 340: The authors seem to already speculate on the dynamics that control the daily evolution of λLWP 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".
We nuanced our words in the text.

Line 375: It might be interesting for the authors to reflect on why near-coastal cells should behave differently, because it may oler clues as to what controls the diurnal evolution in cell size.
We believe this could be one limitation of the use of ΔTb for StCu cells detection. Near the coast, we mostly find large StCu decks with really tightly coupled closed

cells, so tightened that the contrast is barely visible in ΔTb. Thus the variance spectrum does not detect the small cell sizes.

Line 423: The plateau could still be a general feature of cells however?
Yes a feature but regarding the whole StCu situation, the dynamics differ according to the CF regime. It is a feature highlighting the relative contribution of the various cloud organization regimes.

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.
Thank you very much for these interesting questions. We agree that this work raises many stimulating questions for future research. We detailed them at the end of Section 6 as suggested.

Line 456: I would emphasise that this is true also for the diel cycle
OK, we added this to the sentence in Line 456 of the revised manuscript.

Line 460-462: Do you mean interdependence with MBL height?
Rather the MBL's state and/or internal processes. We have modified Line 462 of the revised manuscript (L460-462 of the original one).

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?
As for the previous suggestion, it is true we need to enhance the fundamental questions brought by this work. We highlighted it in Lines 506-512 of the revised manuscript.

**<u>Review 2</u>**

**Comments from the reviewers are reproduced below in black, while our responses are in blue.**

The authors have constructed an aspect ratio product using cloud cell size and estimated cloud top height for MODIS and GOES retrievals, the latter also offering a day-night product. A diurnal analysis shows a robust pattern of increasing aspect ratio from sunrise until a few hours after noon, then an afternoon decrease, and a stable signal overnight. This pattern is regionally consistent in the SE Pacific study region, and temporally consistent for the five years of GOES data, though ENSO does appear to modify the mean aspect ratio. Cloud amount modulates the amplitude of the diurnal cycle of aspect ratio to some extent, but the study suggests that the aspect ratio diurnal cycle may be an intrinsic feature of Sc clouds, mostly invariant to other controlling factors.

This study produces unique and valuable results concerning the process-scale properties of marine stratocumulus. Successful modeling of such clouds is essential for accurate climate and weather forecasting, and these emergent features offer a great baseline for comparing model results with reality. The study is well presented, with excellent figures. The language could benefit from additional copy-editing, but is not problematic. The paper is well-constructed overall, the results are well supported, and the work is in no need of major methodological changes, however a few minor revisions would benefit the work.

First of all, we would like to thank the reviewer for his/her appraisal of our work and the many positive and useful suggestions.

Please note that we have also modified the title of the article, changing “stratocumuli” to “stratocumulus” since it represents more as an adjective rather than a noun in this context. Additionally, the Abstract has been revised to highlight the temporal evolution of the AR rather than its absolute values, ensuring alignment with the reviewers' feedback and the nuanced discussion in our results.

We answer the remarks, point by point in the following.

**Main points:**

Scene selection: Can you add a few words on why you chose the domain sizes, and whether results are sensitive to this decision?

The choice of 300km has been based on previous literature to compare with state of the art,  and has been shown to be suitable to detect organization around cell size. We also have tested the sensitivity of cell size detection regarding the size of scene. For a set of scenes (66 cases), we have derived at the cell scale from 600kmx600km scenes, and compared it to the cell scales derived from the 4 corresponding 300kmx300km scenes (Figures R1 and R2).The methodology is able to detect the same small scales we detect in the smaller scenes and increasing the size doesn’t impact small scales detection.

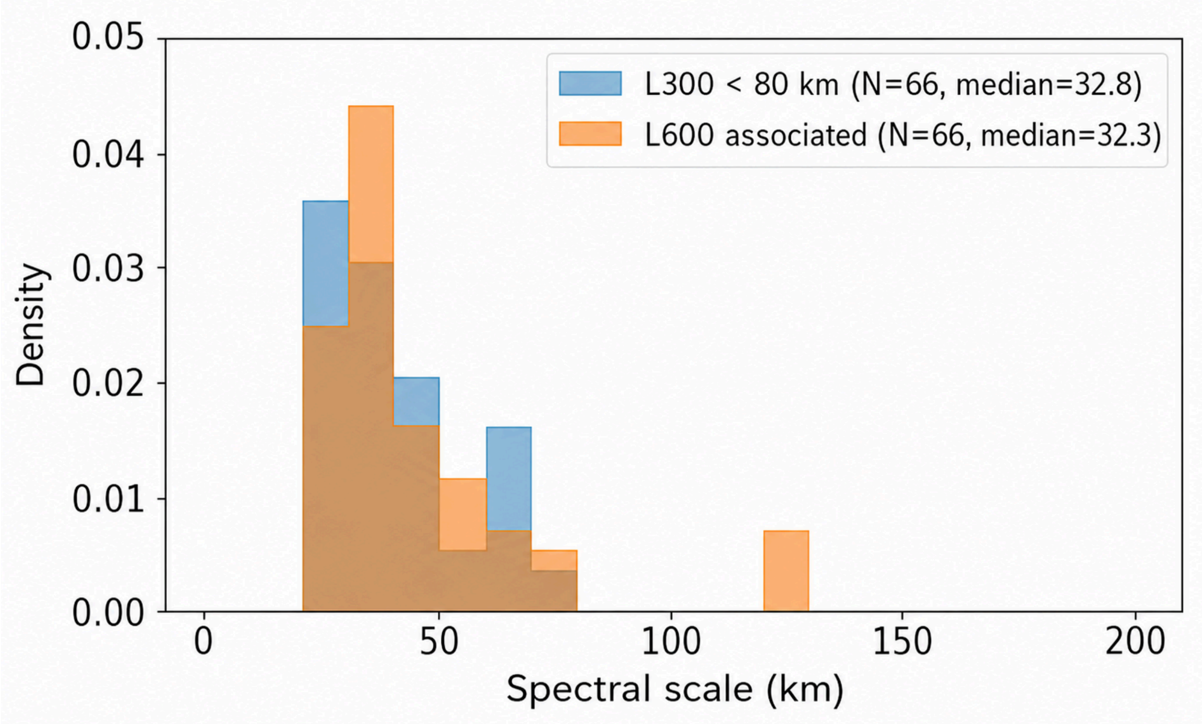



**Figure R1:** Regarding 300kmx300km scenes with a cell size inferior to 80km, 95.5% of the associated 600kmx600km scene still detect a cell size inferior to 80km.

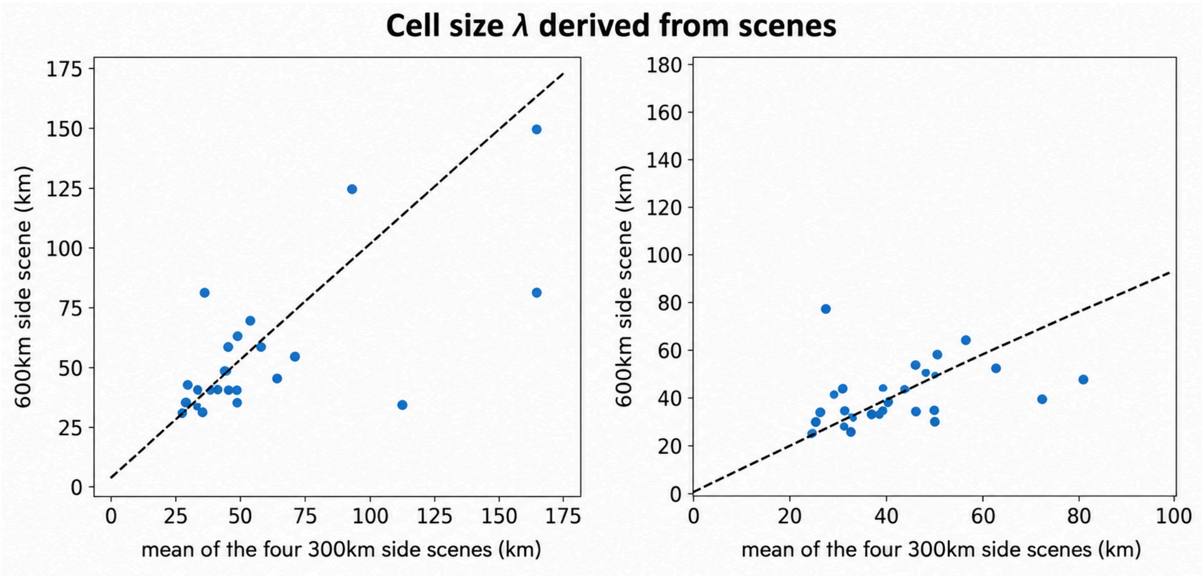


**Figure R2:** Correlation between the cell size derived from the 600kmx600km scene and the mean of the cell sizes obtained from the 4 corresponding 300kmx300km scenes. Correlation is even better when restricting the analysis to $\lambda \leq 80$km.

Paragraph on line 161: How is the 45% threshold determined? Also, how are you interpolating the gaps. Linear 2-D? Maybe be a little more explicit describing this so it could be more accurately reproduced.

Thank you for your comment and suggestion.
First, note that missing values mostly affect the LWP field and not the $\Delta$Tb one, which are the most widely used data in our article. For the LWP, it is due to issues we mentioned in the manuscript such as thin clouds or clear sky zones. We didn't want a higher percentage (≥ 50%) since it would mean we "created" more information than we have from the retrieval.

Actually, when looking at the full 6 years dataset, the preprocessing step (with the cloud mask and setting LWP to zero in clear sky zone) is filling most part of the cloud scenes. We end with a very small amount of scenes with missing values, and if so there are in a very small proportion (see Figure R3). Therefore the 45% threshold is particularly cautious and we can limit it to 10% threshold.

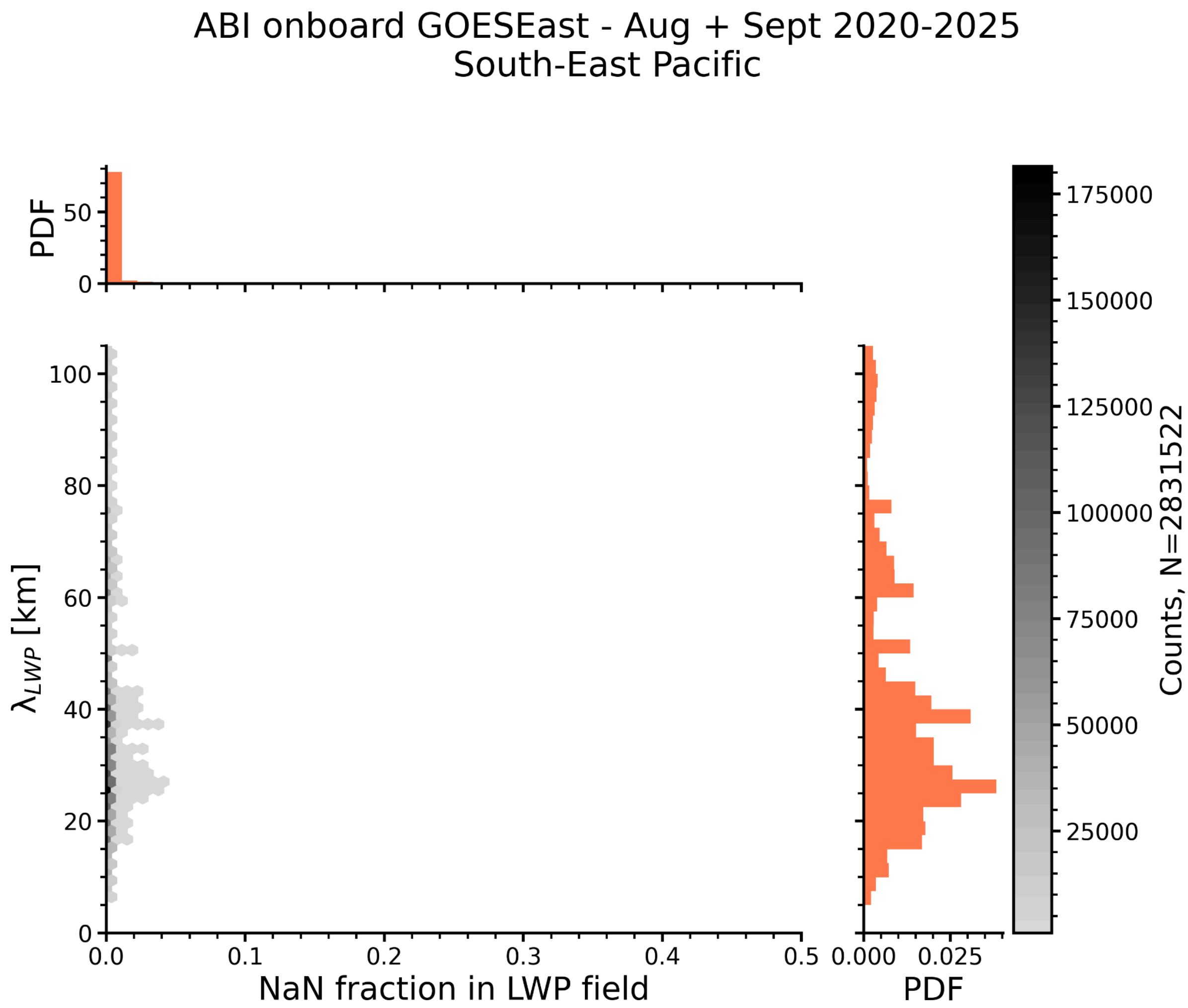



**Figure R3:** Values λLWP computed in view of proportion of missing values of the cloud scenes. Computations were performed for each scene individually, for the full 6 years dataset.

We add this explanation in Lines 165-170 of the revised manuscript "The filling procedure corresponds to a nearest-neighborhood local averaging interpolation constrained to small spatial scales. Each NaN pixel is replaced by the mean of the valid neighboring pixels within a moving square window of size 3×3 pixels. The local averaging is computed using a generic filter with mirrored boundary conditions to avoid edge artefacts. If no valid neighboring pixels were available inside the 3x3 window, the pixel is set to 0. Scenes without missing values are kept unchanged".

The Aqua-Terra CTH disparity in Figure 5a looks like a sensitivity/calibration issue perhaps? Any insights into this?

The CTH Product is used only for MBL depth estimation and corresponds to Figure 5b rather than 5a.

We believe MODIS hyperspectrality leads to lower values near clear sky area and at cloud edges, lowering the mean values of CTH (see Figure R4). As Figure 11 of the manuscript shows, CF is larger at 10:30 (Terra) than 13:30 (at Aqua overpass time). This explains why the discrepancy is even more seen with Aqua because of a larger proportion of clear sky area.

Maybe I missed it somewhere in the text, but did you match the CTT spatial scales between MODIS and ABI before calculating CTH? Differing resolutions will likely cause differing CTT distributions.

We do not use CTT for ABI. CTT is only used for MODIS MBL estimation using the Eastman et al 2016 method. Regarding CTH values, we have looked at the two MODIS Product 'Cloud_Top_Height' at 5km and 'cloud_top_height_1km' , but the difference in resolution didn't solve the issue of drastic low values (∼500m and below) near clear sky zone and at cloud edges (Figure R4).

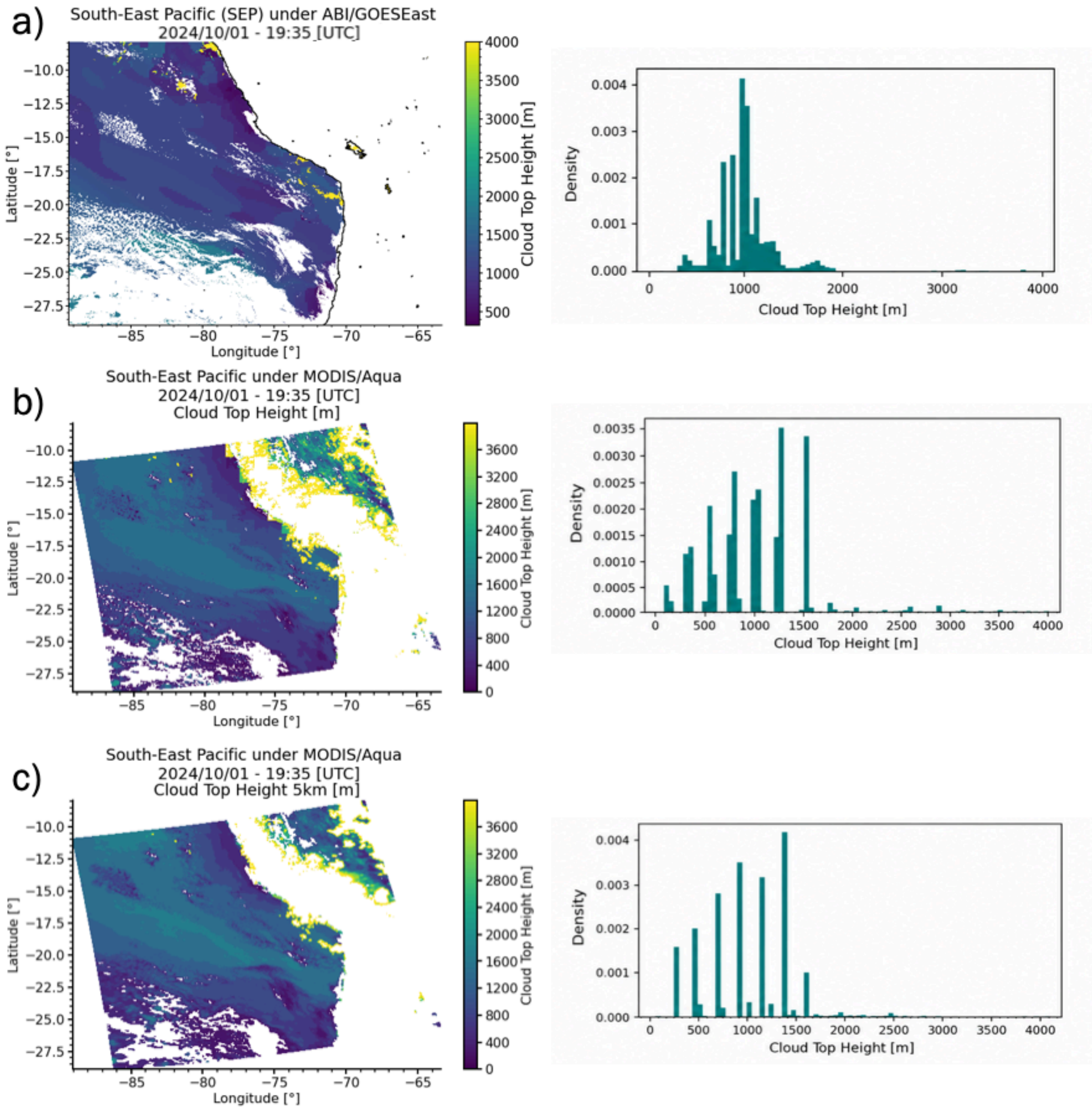



**Figure R4:** Cloud Top Height field (left column) and its corresponding values distribution (right column) using CTH from a) EUMETSAT NowCasting Product with GOES-East b) Cloud Top Height at 1-km resolution from LEOCAT (1km resolution) with Aqua and c) Cloud Top Height at Retrieved Cloud Top Pressure Level from MODIS (5km resolution) with Aqua.

Please reference the data sources for the points listed from lines 95 to 112.

To avoid overloading the text, we prefer to keep information in the Data Availability. Nevertheless, we add a sentence that highlights this point. "Reader may refer to the

"Data Availability" part for references of the data sources" in Line 94 of the revised manuscript.

It’s clear that the AR follows the cell width in Figures 6 and 8, but why not at least show the cycles of CTH as well? This would allow the reader to better gauge the contributions of CTH and lambda. Same for Figures 13 and 14.

In Lines 314-315 of the revised manuscript we calculate the relative evolutions of MBL depth, indicating the evolution of AR is primarily constrained by the cell size. We believe these lines give the information and adding more Figures would overload the manuscript.

The reader can also see the diurnal cycle of MBL in Appendix.

The figures do a good job in showing that the temporal evolution of AR is driven primarily by cell size. Could you set up a similar argument when looking at the spatial fields in Figure 9, where we could also compare the spatial distributions of Lambda and CTH? This would allow us to see whether the spatial distributions are also driven by cell size.

We show here the large-scale situation for the cell size (Figure R5) and the MBL depth (Figure R6) for both the nighttime and afternoon plateau. We put it only in the review in order not to overload the manuscript.

The conclusions we made regarding the diurnal cycle of AR are shown here as well, with a spatial organisation of the cell scale that really look alike the one from the AR, with a general amplification of the value across the basin rather than a change in large-scale pattern. The two situations of MBL are really similar.

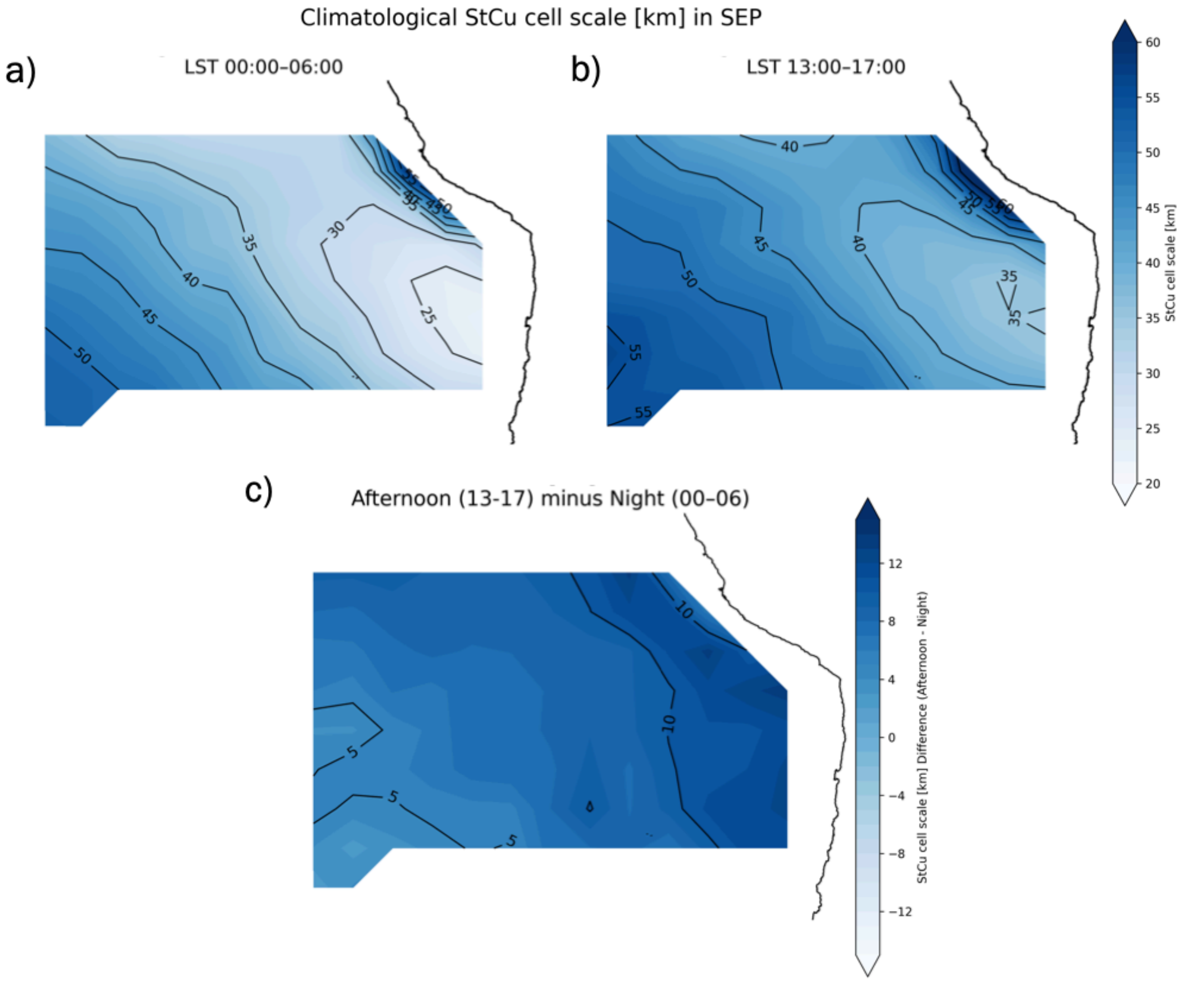



**Figure R5:** Same as Figure 9 of the preprint but for λΔTb.

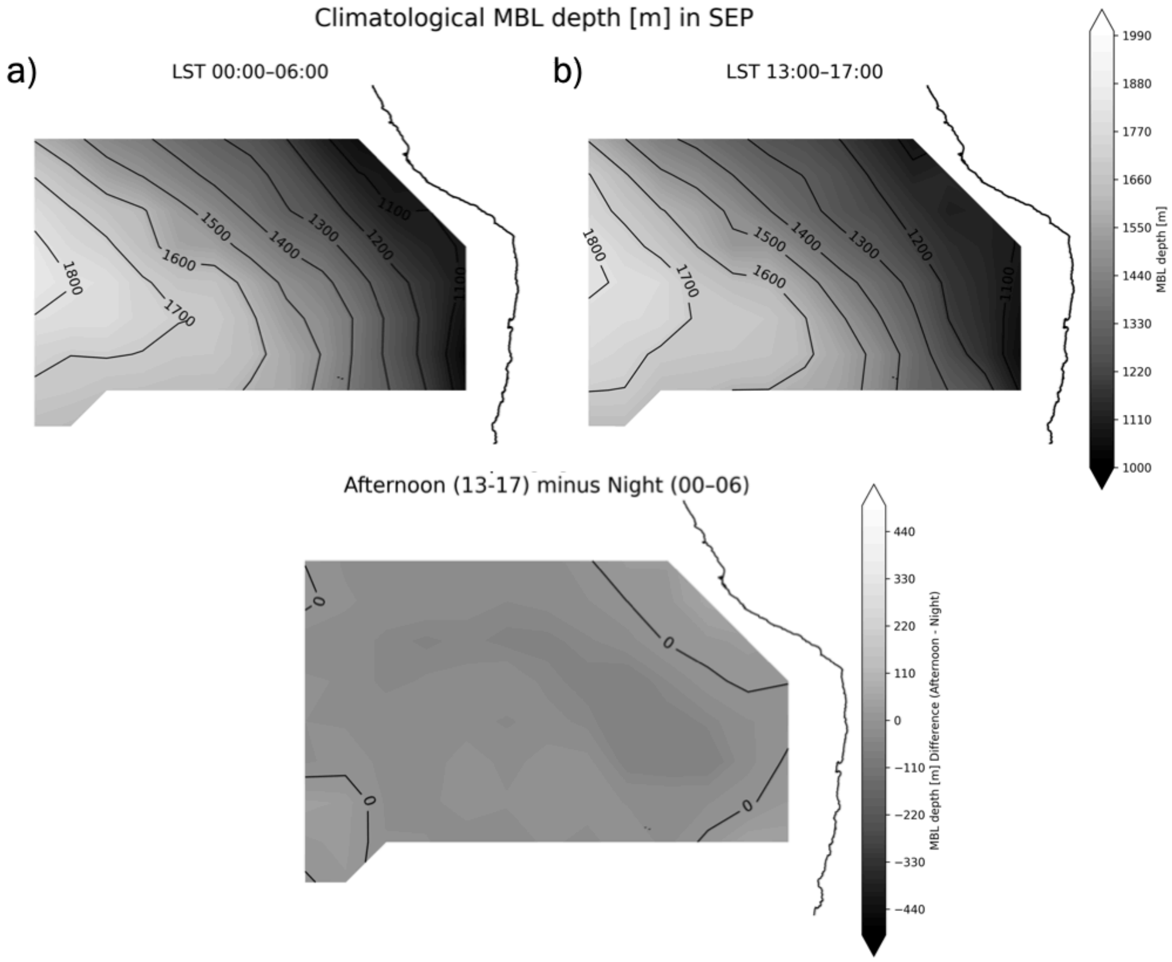



**Figure R6:** Same as Figure 9 of the preprint but for MBL depth.

Regarding CF in Figure 11: How is CF retrieved? The jump in CF near sunrise and sunset has the look of a low-sun-angle bias in visible data. Is it possible that visible channels are being used for the daylight CF values, but not for night? This could cause the spurious jump in CC during sunrise/sunset. I don't believe that these jumps have been seen in corresponding LWP data, or in any surface observations. They look non-physical.

The Cloud Fraction data we use are from the variable 'Effective Cloudiness' from the L2 Product CTTH: Cloud Top Temperature and Height, computed by the Nowcasting Satellite Application Facility (NWC SAF) from EUMETSAT.

These data are available free of charge, after creation of an account: https://www.nwcsaf.org/ctth_description

The description of the Product indicates Radiances and Brightness Temperatures only in IR have been used for computation. Nonetheless, some low infrared/ near-visible channels are used to compute the product (6.2, 7.0 and 7.3 microns) and their radiances could be responsible for the jumps we perceive at sunrise and sunset. We believe it is due to enhanced 3D radiative effects and dependent on the viewing angle of the satellite .

In fact, many figures show clear non-physical behavior between sunrise and 8 LST, then from 16 LST to sunset. I recommend omitting these distracting, non-physical features, and leaving the figures blank during those times. No data is preferable to wrong data.

We believe these retrieval biases are still part of our results. The fact the peaks are seen only for Q1 curve, that corresponds to more Cumulus like regime enhances the robustness of the evolution for Stratocumulus like regimes.

More of a comment for future work: An obvious place to start looking at modulating factors is upper-level humidity. If the radiative balance at cloud top sets the AR, then increased upper humidity may somewhat mimic an increase in sun angle, since both add downwelling radiation to the cloud top.

Thank you for the insights, that is highly relevant. We indeed plan on working on cloud controlling factors and their impacts on the spatial metrics of StCu patterns in our next paper.

This may be more of a future work comment again, but can you address possible differences in MCC types? Figure 3 shows a pocket of open cells embedded within closed cells. Do open cell aspect ratios behave in the same way as closed? This would be interesting to study given the differences in mechanisms. I recommend mentioning this possibility somewhere in the paper.

Previous studies have shown quite similar behaviour between the two patterns of closed and open cells (Wood and Hartman, 2006 Glassmeier and Feingold, 2017).

It would be indeed really interesting to investigate our diurnal results using precise classification that would work day and night. We have started to work with a deep learning method (Wu et al. 2025) that could work, and aim to investigate it in future studies.

We added a sentence regarding these various cloud regimes around Line 510 of the revised manuscript.

Figure 9 needs latitude-longitude lines.

We adapted Figure 9