To the Editors of WCD,

Attached is our point-by-point response and manuscript updates to the reviewer reports of our article, *Diurnal cycles of cloud and rainfall over North-east Queensland during the coral bleaching season (egusphere-2026-1019)*. We would like to thank the reviewers for the time taken to review our paper.

The reviewers recommended major revisions to address areas of the manuscript that required further justification and for reproduction of figures. As documented in detail in our responses, we hope that we thereby address the reviewer comments.

In the responses below, the original reviewer reports are in black, while all our comments are in blue. We have also numbered all the reviewer comments and our replies for clarity. We have *quoted updates to the text from the manuscript in grey italics*.

We thank you and the reviewers for the time invested into our manuscript and hope that it will reach the high standards of *Weather and Climate Dynamics* upon revision.

Best regards,

Alanah Chapman (on behalf of all authors)

# Reviewer 1

## Major comments

**Comment 1**

1(a) The paper uses two sources of data for winds: the BARRA-R2 reanalysis and AWS observations. It is not clear why the paper mainly uses BARRA-R2 then switches to the AWS data for Fig 8. Why not use the observations to derive the wind regimes in the first place? I assume this is because there is no AWS at Willis Island (and there could also be missing data), but section 2 should clearly justify the choices of data sets.

We have added to the methods section clarification of our use of the 850 hPa BARRA-R2 wind fields rather than surface winds for defining the wind regimes which are representative of ‘background wind conditions’/synoptic-scale meteorology.

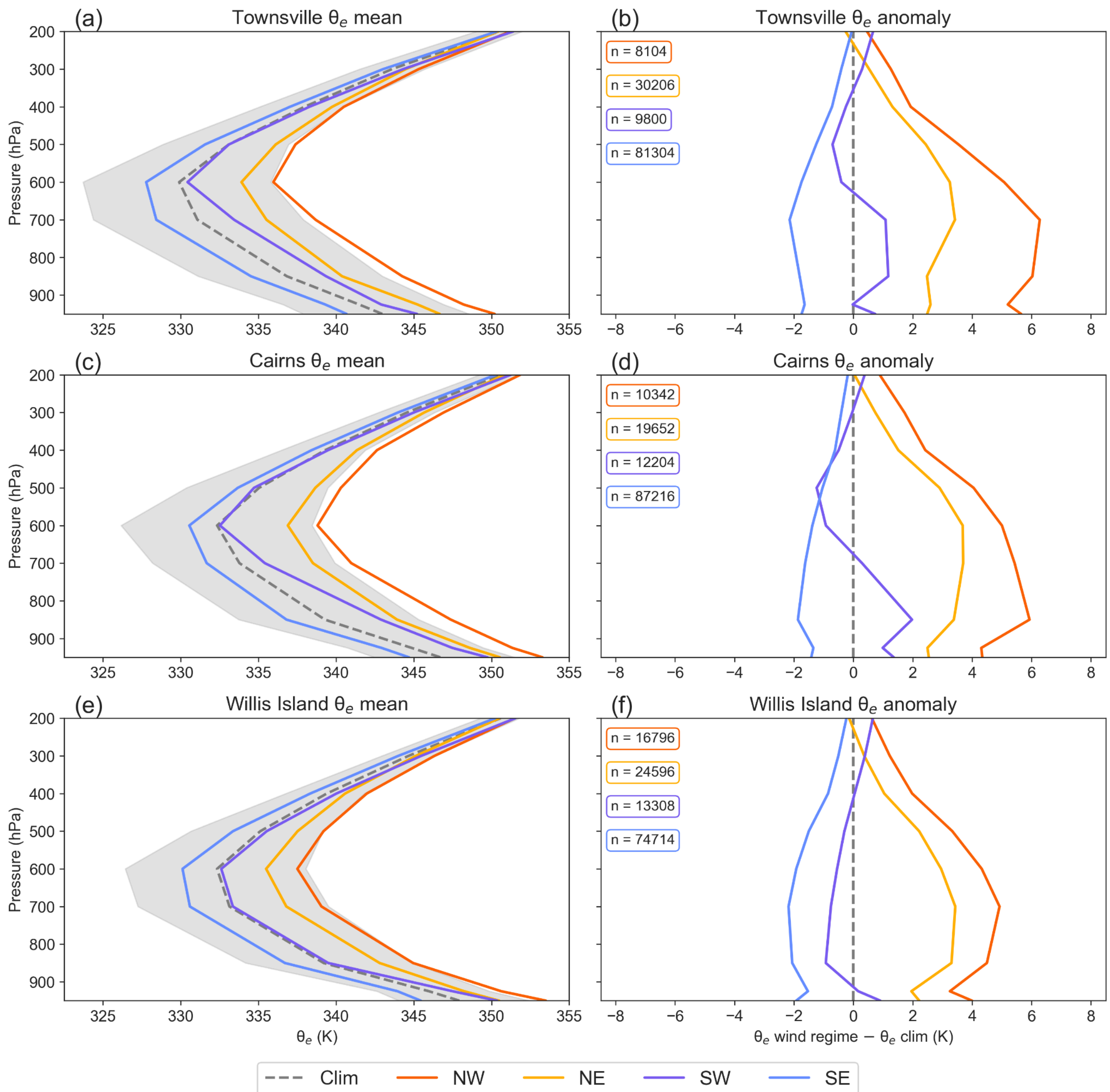
See relevant new text for wind regimes defined by BARRA-R2 (L132-133) and use of the station data for Fig. 8 (353-355):

> *Large-scale meteorological conditions were characterized by 850 hPa level winds, whose direction and strength relative to the coastline have been shown to influence the diurnal cycle of coastal precipitation (Aoki and Shige, 2024; Dao et al., 2025).*
>
> *We use station-based surface wind observations here rather than the BARRA–R2 reanalysis as the 12 km horizontal resolution of the reanalysis may be too coarse to reliably resolve these coastal circulations.*

**Comment 2**

1(b) L99 says the reanalysis is hourly. For Jan-Apr 1979-2024 this would give 132,768 data points for each site, but Fig 4 has an order of magnitude more than this (1,168,776 if you add up the number of samples across regimes). Please clarify.

We have fixed this error in the sample size number which had incorrectly included the data points across each pressure level in the stated ‘n’. We have changed the sample size numbers in Fig 4 to reflect only the number of time samples (and not included number of samples across each pressure level). Please see new Fig. 4 (and our responses to comment 3 and 5 which explain differences in the anomaly plots):




**Comment 3**

1(c) I believe you define the wind regimes hourly, so successive hours could be in different regimes. Have you tried categorizing the regime for the entire day based on, say, the daily mean winds or the winds at a particular time of day, or averaged over a particular few hours? When you compute composite diurnal cycles for each regime (Figs 5-8) you must be piecing together different parts of different days, rather than full diurnal cycles. Does this affect the results? It may make the precip in Fig 7 less noisy, for example.

We have changed our method of wind regime categorization to look at the mean 850 hPa wind direction for each site at 12 LT - and then assign the rest of that day (00-23 LT) to that same wind regime. This ensures that the composite diurnal cycles are constructed from complete daily evolutions within a consistent background flow regime.

After further analysis we found that the difference in results from the hourly definition vs 12 LT wind regime definition have some small differences - but largely our results remain consistent with the initial manuscript. This is because the 850 hPa winds typically don't change mean wind direction in successive hours - rather these background winds tend to remain consistent over several hours to days. However, we agree it is sensible to assign an entire diurnal cycle to a particular regime to account for periods which may produce composite results which originate from different days.

Relevant new text in (difference document) L134-137:

> *The mean 850 hPa wind direction at 12 Local solar Time (LT) from BARRA–R2 was calculated across each radar domain for the full CBS from 2012 to 2022. For each day within the temporal domain the 12 LT mean wind direction was computed (by first averaging zonal and meridional components separately before computing wind direction), and each hour of that day (00-23 LT) assigned to the resultant wind regime. In this way composite results are reflective of a full diurnal cycle.*

**Comment 4**

1(d) You show in Fig 8 that the wind direction has a diurnal cycle. If you are selecting the wind regime hourly, certain regimes are more likely to exist at certain times of day. Therefore, there may be an aliasing effect when you produce composite diurnal cycles. For example, theta-e presumably has a diurnal cycle, especially near the ground (but I think Fig 4 is averaged over all times of day?). So are the differences between regimes really due to the wind patterns, or just a selective bias in the time of day that goes into each composite? This is another argument for assigning a single regime to each entire day.

Please see response to comment 3.

**Comment 5**

1(e) Similarly, you show that different regimes are more likely to occur at different times of the season (Fig 1b-d). You say (L184) that theta-e profiles are shown relative to the "seasonal composite mean". Is this a single mean profile over all of Jan-Apr? What if you show the anomalies relative to the corresponding month's mean? Or even use a smoothed mean seasonal cycle that varies at daily time resolution? This would remove any aliasing of the seasonal cycle onto your regime composites.

We had used the seasonal composite mean (single mean profile over all of Jan-Apr) relative to the wind regime results in Figure 4 b, d, f. We have now changed this as suggested, to a smoothed rolling mean varying at daily time resolution. This change has been applied to both Fig. 4 and Fig. 5, alongside the changes made to the wind regime categorisation (see comment 3).

Relevant new text below:

L150-151: *Analysis of theta-e anomalies associated with each wind regime was done by constructing a smoothed rolling mean climatology (for the period 1979-2024 Jan-Apr) to remove seasonal signals.*

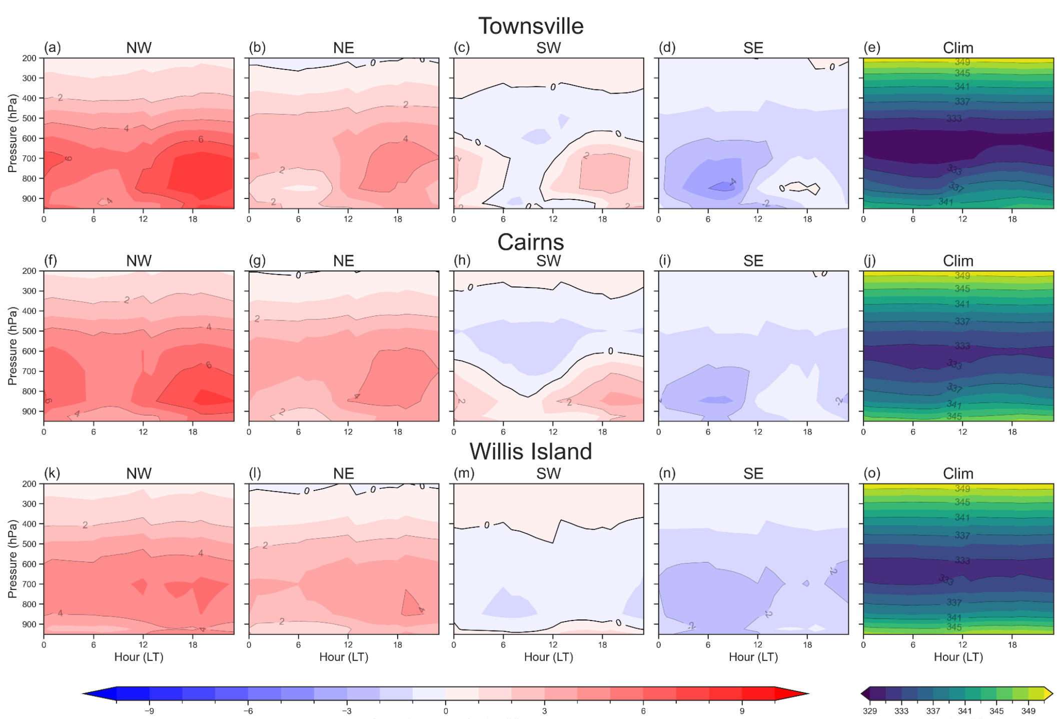
L212-213: *The CBS mean and wind–regime–averaged theta-e profiles for the three sites are shown in Fig. 4a, c, e, with anomalies relative to a smoothed rolling mean climatology shown in Fig. 4b, d, f. The four large-scale wind regimes can be interpreted in terms of two key controls on convective potential: (i) the moisture and temperature characteristics of the air mass (theta-e magnitude), and (ii) the vertical stability structure (theta-e gradient and inversion strength).*

L242: *The diurnal evolution of theta-e under each wind regime is shown in Fig. 5 as anomalies relative to a smoothed rolling mean climatology over the CBS.*

Fig. 4 caption: *Anomaly plots are calculated by subtracting a smoothed rolling mean climatology from each timestep within the wind regime-classified periods, then averaging across all time steps within each regime to remove seasonal signals.*

Fig. 5 caption: *Diurnal theta-e profile difference plots between wind regimes and a smoothed rolling mean climatology over the CBS for (a,b,c,d) Townsville, (f,g,h,i) Cairns, and (k,l,m,n) Willis Island.*

Please also see our response to comment 2 for the adjusted Fig 4. Figure 5 is shown below:



**Comment 6**

2). Much of this work is reminiscent of Dao et al. (2025; 10.1002/qj.70027) but surprisingly this is not referenced. The introduction should cite this paper and clearly explain how the present study differs from or builds upon it.

The two studies are scientifically related, but differ in scope, datasets, and emphasis.

- The Dao et al. (2025) focuses specifically on the wet season and examines rainfall propagation using a 180-day convection-permitting regional model simulation, complemented by radar observations around Townsville. Their study mainly investigates rainfall behaviour - particularly rainfall propagation and heavy rainfall days - and does not explicitly examine cloud characteristics.
- In contrast, our study takes a broader climatological perspective during the coral bleaching season, analysing multi-year observations across different wind regimes and regions. It examines both cloud and rainfall diurnal cycles using Himawari-8, BARRA-R2, and rainfall observations from three coastal radars.
- Scientifically, Dao et al. (2025) focuses more on the dynamical mechanisms controlling rainfall propagation - especially the interaction between land-sea breezes and large-scale background winds - whereas our study focuses more broadly on the coupled evolution of clouds and rainfall, including regional and environmental contrasts.
- Our study therefore extends beyond rainfall propagation alone to include cloud timing, cloud-top characteristics, and their relationship to convection across coastal land, coastal ocean, and open ocean environments.

We have adjusted the introduction to include this citation, see relevant Introduction text changes (L78-91):

> *In the context of clouds and rainfall over the GBR, Zhao et al. (2022) provided a broad characterization of seasonal cloud properties which are influenced by local-scale forcings across large spatial scales. More recently, Dao et al. (2025) investigated wet-season rainfall propagation over northeast Queensland using a 180-day convection-permitting regional model simulation complemented by radar observations around Townsville. Their results demonstrated that interactions between land–sea breezes and large-scale background winds play a key role in regulating coastal rainfall patterns, particularly the offshore and onshore propagation of heavy rainfall systems. These studies highlight the importance of multiscale interactions in shaping coastal convection in the region. However, a more integrated understanding of diurnal cloud and rainfall evolution across coastal and marine environments remains needed to better characterize regional variability and its implications for coral bleaching risk. Building on this scope, we examine the diurnal variability of both clouds and rainfall during the Coral Bleaching Season (CBS) using multi-year Himawari-8 observations, BARRA-R2 regional reanalysis, and rainfall observations from three coastal radars. Unlike Dao et al. (2025) which focused primarily on rainfall propagation, we consider the coupled evolution of cloud and rainfall systems across coastal land, coastal ocean, and open ocean environments under different prevailing wind regimes, with a particular emphasis on the*

*spatial and temporal variability of cloud-top characteristics, rainfall timing, and cloud–rainfall relationships associated with convection across northeast Queensland including the GBR.*

**Comment 7**

3(a) Throughout the paper, I found the discussion of the figures (particularly 4-7) to be very descriptive, going through each wind regime in turn and describing every feature of the plots. A better discussion would focus on the story that the authors want to tell. For example: In the discussion of Fig 7 (section 3.4) a lot is said about inland propagation, but the title of the paper and the motivation in the introduction relate to coral bleaching, so a key point is cloudiness over the reefs. Therefore, is inland propagation really relevant to the aims of the paper?

We appreciate this suggestion. We agree that the original discussion of Figs. 4-7 was somewhat overly descriptive in its regime-by-regime presentation, which may have obscured the broader physical interpretation. In the revised manuscript, we have substantially revised and restructured these sections to place greater emphasis on the overarching physical story before discussing the individual regime details. We now first highlight the common features across regimes and then interpret how the different wind regimes modulate these behaviours. At the same time, we note that for a process-oriented meteorology study involving multiple wind regimes and Hovmöller analyses, some degree of structured regime-by-regime discussion remains scientifically necessary and appropriate.

Regarding Sections 3.3-3.4, where inland propagation characteristics are discussed, our intention was not simply to describe convection over land, but to characterise the diversity of cloud and convection regimes associated with different wind patterns across the broader GBR region. In this context, inland and coastal convection remain relevant because reef cloudiness can be closely linked to surrounding mesoscale circulations, moisture transport, and the spatial evolution of nearby convective systems.

Our broader motivation is therefore not only to document cloud cover directly over the reefs, but also to improve understanding of the environmental and meteorological conditions under which different cloud systems may influence the GBR atmosphere and potential reef shading. To clarify this connection, we have also expanded the Discussion section to more explicitly discuss the implications of the observed cloud and convection regimes for the GBR environment, including how differences in cloud propagation and spatial organisation under different wind regimes may influence reef cloudiness and potential shading (L413-420).

**Comment 8**

3(b) You return to the point about coral bleaching in the final paragraph of the paper, but very little has been said about this previously. Could you shift the focus of the results and discussion to the locations of the reefs? Do you have SST data that you could composite by regime? That would demonstrate whether these regimes really do have an impact.

Coral bleaching is discussed in the paper primarily as part of the broader motivation for understanding GBR cloud and precipitation variability, rather than as a direct focus of the analysis itself. As noted in the Introduction, previous studies (Huang et al., 2024; McGowan and Theobald, 2023; Richards et al., 2024, 2026; Zhao et al., 2022; Leahy et al., 2013) have demonstrated strong relationships between cloud cover, surface radiative fluxes, and SST variability over the GBR region. Building on this context, our study focuses specifically on improving understanding of the diurnal behaviour and spatial organisation of cloud and precipitation systems under different wind regimes. We have strengthened the Introduction to clarify this framing (L78-91).

We agree that compositing SST by regime would be a valuable extension and could provide additional insight into the potential environmental impacts of these cloud regimes. However, we feel that robustly attributing SST variability or coral bleaching responses to the identified atmospheric regimes would require a more comprehensive coupled ocean-atmosphere analysis, including consideration of oceanic processes and temporal lags, which is beyond the scope of the current study.

In response to this comment, we have revised the last paragraph of the manuscript to better clarify the relevance of the identified cloud regimes to GBR environmental conditions, while also being more careful not to overstate a direct causal connection to coral bleaching (L445-455).

**Comment 9**
4) There are occasional speculative comments which should be better evidenced or else removed: (a) L222-223: "likely reflecting the reduced influence of orographic uplift and elevated terrain heating compared to Cairns"

We have updated Fig 7 to show the Cairns orography (new Fig. 7 shown in response to comment 35). We believe this addition more clearly illustrates the complex terrain surrounding the Cairns region and provides useful geographical context for the interpretation of the results. In particular, it helps support the discussion regarding the potential role of orographic lifting in shaping the observed cloud and precipitation patterns.

**Comment 10**
4(b) L327: It is not very encouraging that you admit some of the features of your concluding schematic diagram are "speculative". You say this is a limitation of observations, but can you not use BARRA to investigate processes that are not observed? There is nothing about gravity waves in the paper so this should not be included in Fig 9 (although you could make references to other papers on gravity waves in the text as a possible explanation for the offshore convection).

We thank the reviewer for highlighting the need for clearer separation between established results and more speculative interpretation. Figure 9 was intended primarily as a conceptual synthesis to summarise the key regimes and provide a framework for interpreting the observed cloud and precipitation behaviour, rather than as a definitive mechanistic conclusion.

We agree that the inclusion of gravity-wave-related processes in the original schematic was not sufficiently justified within the scope of the present analysis. In the revised Fig. 9, we have retained gravity waves only as a schematic, literature-based mechanism for offshore convection, and we now clearly distinguish them from processes directly supported by our analysis.

To address the reviewer's concern, we have revised the text to clarify the purpose of this diagram (text shown below) and revised the figure using a different visual style (e.g., dashed and lighter symbols) to explicitly indicate that these processes are hypothesised and not diagnosed in this study. We have also revised the caption and text to clarify that gravity waves are included only as a previously suggested mechanism in the literature, and should not be interpreted as a conclusion drawn directly from the present results.

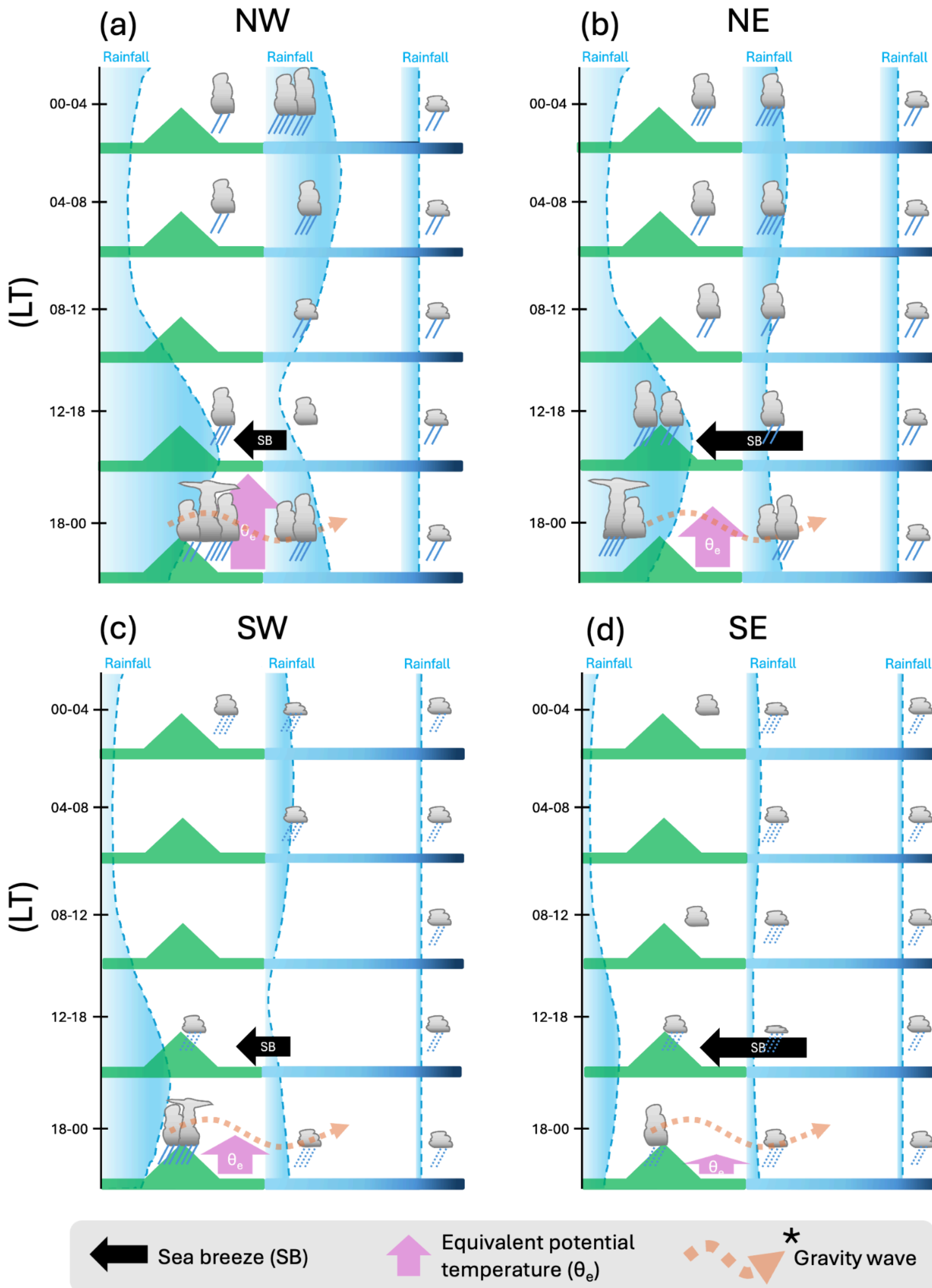
L377-387:

> *Figure 9 provides a conceptual summary of the contrasting diurnal cycles of cloud and rainfall, along with the proposed mechanisms of convection initiation and propagation. It is intended as a framework for interpreting the observed diurnal cloud and rainfall behavior, rather than a complete or definitive description of all underlying processes. Some elements are necessarily interpretative, particularly where direct observational constraints are limited, and are therefore presented as hypotheses consistent with the observed patterns and existing literature. The schematic spans five key time periods — nighttime, early morning, morning, afternoon, and evening — and outlines the general evolution of convection and rainfall across the three sites. We also include gravity waves within Fig. 9 which are not fully evidenced in our analysis given the limitation of observations — instead inferred from our estimated convection propagation speeds which are consistent with gravity waves as a mechanism for offshore convection in previous literature (Yokoi et al., 2017; Mapes et al., 2003). These diagrams synthesize well-supported findings from this study with more tentative, literature-informed mechanisms to identify key processes warranting further investigation in future work.*

Relevant Fig. 9 caption:

> **Mechanism inferred from estimated propagation speeds consistent with gravity wave propagation in literature - dashed orange oscillating arrows indicate the nighttime offshore propagation of convection induced by gravity waves.*

Adjusted Fig. 9:




In the revised manuscript, we have revised both the figure and accompanying discussion to more clearly distinguish between well-supported findings and more tentative hypotheses.

Regarding the suggestion of using BARRA-R2 to further investigate these processes, we agree that the reanalysis provides valuable large-scale environmental context. We have now incorporated the analysis of Convective Available Potential Energy (CAPE) and vertical velocity cross-sections from the BARRA-R2 reanalysis in Section 3.2, providing additional insight into the spatial patterns of instability and large-scale vertical motion associated with each regime. However, we note that the horizontal resolution of BARRA-R2 (12 km) is not sufficient to explicitly resolve key coastal and mesoscale processes relevant to the GBR, and we therefore use these diagnostics primarily for contextual interpretation rather than detailed process attribution in this study.

**Comment 11**
(c) The only mentions of orographic uplift in your results section are also speculative, but you use this as an explanation in the discussion (L336). This should also be removed unless you can make some relevant arguments. For example, if you mark orography on Fig 7 somehow, could you deduce an impact of uplift?

Please see the response to comment 9.

**Comment 12**
5) You argue that there is a lag between precipitation and BT. This is a well-known phenomenon but I struggle to see it in Fig 6. Please be quantitative about the times of day of minimum BT and maximum precip, to make the lags clear (L225, L249).

We have adjusted the text as suggested to be more quantitative.

Relevant new text:

> L279-280: *A notable feature is that peak rainfall at 16 LT precedes the coldest BTs by 3 hours, suggesting that the minimum BTs likely reflect the later anvil-dominated stage of convection rather than the peak rainfall phase.*
>
> L298-299: *The most intense rainfall over land peaks at 17 LT, whereas peak rainfall frequency and cold BTs lag by 3 hours, again reflecting a transition from active convection to mature and stratiform/anvil-dominated cloud.*

**Minor comments**

**Comment 13**
L19: "influences" -> "influence"

We have updated.

**Comment 14**
L28: "interact with" -> "interacts with" to be consistent with "accounts for" earlier in the sentence.

We have updated.

**Comment 15**
L33: Cite something more recent here regarding state-of-the-art models' ability to represent the diurnal cycle.

Relevant new text in (difference document) L36-45:

> *However, climate models continue to systematically underestimate mean convection and rainfall in the tropics, often due to deficiencies in how they represent key processes controlling the diurnal cycle. In particular, many models have difficulty capturing the timing, amplitude, and propagation of convection because of limited spatial resolution and parameterized convection schemes that cannot fully resolve the interaction between boundary-layer evolution, surface heating, moisture convergence, and cloud–precipitation feedbacks (Tang et al., 2021; Covey et al., 2016; Xie et al., 2019; Christopoulos and Schneider, 2021). Recent advances in convection-permitting modeling, including emerging regional and a small number of global-scale simulations, have demonstrated substantial improvements in representing the diurnal cycle of convection and its propagation, although such approaches remain at the frontier of computational capability and are not yet widely available for long-term or ensemble climate applications (Clark et al., 2016; Hohenegger et al., 2008; Huang et al., 2026; Kendon et al., 2019; Pearson et al., 2014; Stevens et al., 2019).*

**Comment 16**
Fig 1: The Cairns radar and AWS are not easy to see. Suggest drawing them on top of the Hovmoller transect.

Figure 1 has been updated so that these features are clearer.

**Comment 17**
L150: Replace "composite" with "histograms of"?

Text updated.

**Comment 18**
L154: I'm not convinced that the increase in SE through the months is delayed at Cairns and Willis Island compared with Townsville. The three sites look very similar in this respect.

We have removed this from the sentence.

**Comment 19**
L154: Replace "stable" with "similar"?

Text updated.

**Comment 20**
L158-159: Remove "on average" and "on a seasonal mean basis"

We have updated this line as per reviewer suggestion.

**Comment 21**
L160: "variation" -> "variation in"

We have updated this line.

**Comment 22**
L162: "[NE]'s decline coincides with the strengthening south-easterlies, indicating a shift in dominant flow". But SE is always the dominant regime, in all months shown.

The south-easterly regime is indeed the dominant flow throughout the period shown, and does not represent a seasonal transition into a new dominant state.

What we intended to highlight is that the relative frequency of the NE regime decreases as the south-easterly flow strengthens, reflecting a redistribution of occurrence probabilities rather than a change in the dominant regime itself. We have revised the text to make this clearer and avoid implying a shift in the prevailing background flow:

> L198-199: *This regime's seasonal decline coincides with the strengthening south–easterlies, indicating weakening of tropical influences.*

**Comment 23**
Fig 2: Gray "hatching" (actually shading) is too faint.

We have updated the figure for clarity, removing the hatching entirely, using darker grey shading.

**Comment 24**
L164-170: The SW and NW regimes are so uncommon, do you really need this paragraph?

We thank the reviewer for their comment. However, we chose to retain the paragraphs describing the SW and NW regimes despite their relative rarity, as they exhibit systematically different cloud and precipitation characteristics compared to the more common easterly regimes.

Results from satellite derived brightness temperature and radar derived rain rate show the westerly regimes differ significantly from easterly regimes in terms of cloudiness, rainfall intensity and rainfall frequency. Even though they are less frequent, these regimes provide important contrast conditions that help contextualise the full range of observed variability and support later interpretation of the results.

**Comment 25**
Fig 3: Caption says (a) shows the wind regimes, but you actually divide up the direction into 16 bins on the plot, whereas there are only 4 regimes. I take it the polar plots are histograms. What are the tick labels on the radial axes? Are they percentages? I would use the same tick labels on each plot and use integer values (rounding errors make it look like the ticks are unevenly spaced). There is no explanation of the colors. Have you ordered the values within each histogram bin by speed and colored them accordingly? In panels b-d, consider changing the color for either NE or SW, as red and green together should be avoided.

Fig 3 has been updated as per reviewer suggestions:

- Wind rose sections adjusted to show the wind regimes (4 bins rather than 16)
- Consistent radial axes between each wind rose (with ticks showing %)
- Increasing the font size of the wind rose colors and including an explanation of the colors in the caption
- Adjusting the colors used in panels b-d to be colorblind friendly

Changes to Fig 3 caption:

> *(a) Polar histograms over the CBS for BARRA–R2 850 hPa wind regimes defined in Section 2.2. Bar length indicates wind direction frequency, with colours indicating corresponding wind speeds in m $s^{-1}$. (b,c,d) shows the monthly regime frequency for Townsville, Cairns and Willis Island. BARRA–R2 850 hPa winds are averaged over each site's radar domain (see Fig. 1) for the CBS over the period 1979–2024.*

**Comment 26**
L177-181: Move the explanation of theta-e to section 2.

The theta-e explanation has been moved to section 2.

**Comment 27**
L179: Remove "typically".

We have updated this line.

**Comment 28**
L195: What is meant by "energetically favorable air"?

Updated text L238-239:

> *This structure is consistent with monsoonal or tropical intrusions that inject warm and moist air masses, providing both moisture supply and reduced stability (Berry and Reeder, 2016).*

**Comment 29**
L214: More useful to quote the wavelength than the Himawari channel number.

We have updated the manuscript as suggested.

**Comment 30**
L214: How do you define rainfall frequency? Rainfall rate above some threshold?

We have now updated section 2.2 to include the definition of rainfall frequency. See L174-175:

> *Rain frequency was defined as the percentage of points in a given time step where rain rate was greater than 0.001 mm $h^{-1}$.*

**Comment 31**
L215, L308, L313 and Fig 9 caption: Fix figure references.

We have updated these lines to display the correct figure references.

**Comment 32**
Fig 5: Suggest subtracting the daily mean (as a function of p) from each panel to make the diurnal variations clearer.

We have adjusted Fig 5 to show difference plots from a smoothed rolling mean (using a 30 day window), to make the diurnal variations clearer for each regime (also see the response to comment 5).

**Comment 33**
L240: "BTs peak": better to say something like "BTs are largest" since you invert the y-axis (which I agree is sensible), so the "peak" actually appears as a trough.

We have updated this line.

**Comment 34**
Fig 6: Can remove "hourly means". Again, quote wavelength of satellite channel and define rainfall frequency.

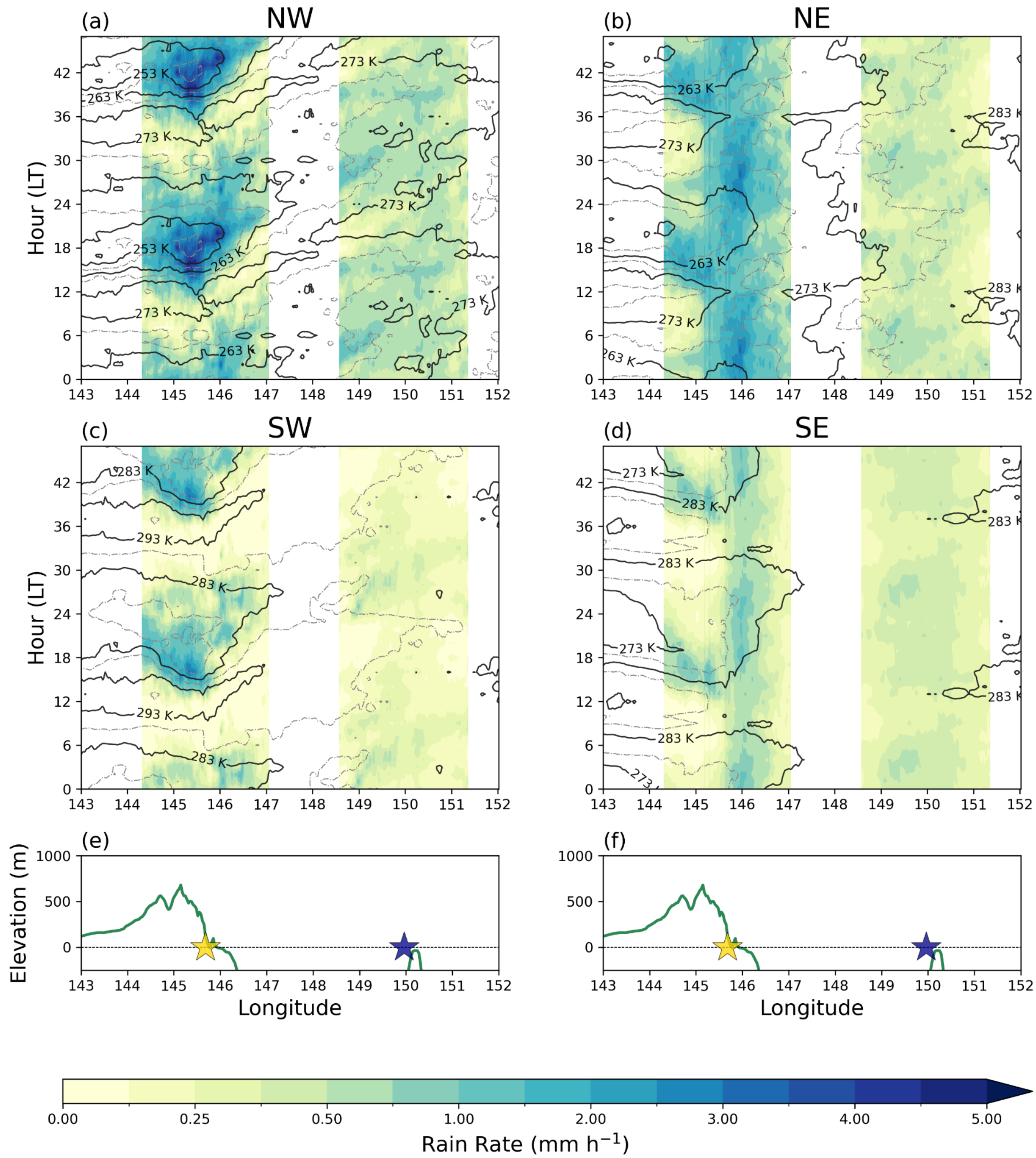
We have updated the Fig 6 caption as suggested.

**Comment 35**
Fig 7: Not easy to see the precip contours on top of the BT. Consider removing the filled BT contours and just show the contour lines for 273 and 263K, since these are what you refer to in the text anyway. Then consider showing the precip as filled contours. It's not always clear what feature you think is propagating at the speed shown by the propagation lines, e.g. the 30 m s-1 line in (b). In the caption, replace "diagram" with "diagrams". Remove "mean daily"?

We have updated the formatting of Fig 7 as suggested by:

- Removing filled BT contours, only showing 253K, 263K, 273K, 283K and 293K contour lines
- Radar rain rate is shown in filled contours
- Updated caption

We would also like to inform the reviewer that updates by the Bureau of Meteorology to the underlying radar data (quality controls and an updated dataset since this paper was submitted) have been implemented in the new figure. Alongside these changes to the underlying radar dataset we have also implemented the new 12 LT categorisation of the wind regimes (however the resultant propagation and timing of peak convection/rainfall remains consistent with the original manuscript). The updates to the radar dataset explains why the radar rain rate colorbar now shows a larger rain rate magnitude in comparison to the original figure.




**Comment 36**

L318: "prior tropical studies" - which ones?

We have updated the sentence to include references.

**Comment 37**
Fig 8: How is average direction defined? (e.g., What is the average of 1deg and 359deg? Arithmetically it is 180deg but common sense dictates it is 0deg.) Or do you average the zonal and meridional components, then show the speed and direction of the result? This is what I would recommend. At 15LT at Cairns, (c) indicates the average is roughly easterly but (b) shows the wind is mostly from the south-east at this time.

Thank you for your comment. The AWS dataset includes wind direction with unit 'degrees'. Therefore for the average wind direction calculations we use circular mean statistics to calculate the resultant wind direction. To make this clear to readers we have included the following line in section 2.2:

> L119-121: *Hourly averaged wind direction was calculated using the circular mean, which is used for averaging angular data (i.e. taking the average of 350° and 10° to result in 0°).*

**Comment 38**
Fig 9: (a) Are the five images supposed to represent equal time periods, or particular times? This is not clear from the vertical time axis.

(b) The theta-e arrow is too faint.

(c) Consider indicating SB speed by the length of the arrow. Using thickness looks like the SB depth is changing.

For comment (a) we have adjusted the y-axis to show clearer separation of each time period. We have updated Fig. 9 as per reviewer suggestions (b) and (c) (see Fig. 9 in response to comment 10).

**Comment 39**
L462: This is the discussion version of the paper, not the published version in WCD (2026).

We have updated this reference.

# Reviewer 2

## General comments related to the manuscript

While the overall manuscript was a pleasure to read, I feel quite the number of elements could be added to bolster its quality. For example, the authors refer to the relevance of this study for the coral bleaching processes/season through influences on radiation, heat fluxes and ocean temperatures, but do not analyse any of these variables – further analysis of this, if possible as suggested in their future work component of the conclusions, would be highly beneficial and provide a nice bridge between motivation and research outcome. I additionally think, while the authors hypothesise many of the associations between thermodynamics and the convective environment, targeted analyses of a particular case study or case studies, as seen in the literature for the MC (for which they cite), could provide deeper understanding of the influences of the wind regimes on the diurnal cycles, and the additional links with orography/land-sea breeze circulations, for which they suggest. Determination of what the broader synoptic environment is doing through usage of the employed reanalysis dataset will provide further reasoning for why the diurnal cycles in each region are doing what they do. Coupling with additional analysis of satellite rainfall products, such as IMERG GPM, if possible, would also provide further validation of the results. In terms of presentation, more consistent referencing to (ideally reordered/replotted) subplots within the figures, as outlined below, will make the arguments flow much more smoothly. Generally, explanations for what each variable indicates in relation to the study (e.g. high and low BTs) should be more succinct, less repetitive, and confined to just a short portion of the methodology.

Therefore, I recommend major revisions for the manuscript to enable the authors to address the technical corrections, figure reproduction, and suggestions I have, though I believe the manuscript remains of a good quality. I have provided specific comments/concerns below along with the technical and/or grammatical corrections, with the relevant line numbers provided for the authors' benefit.

We would like to thank the reviewer for their comments and suggestions for improving the quality of our manuscript. General manuscript comments we address below:

- We agree that linking the results more explicitly to surface radiative fluxes, SST variability, and ultimately coral bleaching processes would strengthen the broader motivation of the study. However, we would like to clarify that the scope of the present paper is centred on cloud and precipitation structure and variability over the GBR and coastal Queensland, rather than a full surface energy budget or coupled ocean–atmosphere attribution analysis.

  Coral bleaching is therefore used primarily as a motivating context in the Introduction, rather than a target variable for quantitative analysis in this study. As noted there, previous work has already established strong links between cloud cover, surface radiative fluxes, and SST variability over the GBR (Huang et al., 2024; McGowan and

Theobald, 2023; Richards et al., 2024, 2026; Zhao et al., 2022; Leahy et al., 2013). Building on that foundation, our focus here is specifically on improving understanding of the diurnal behaviour and spatial organisation of cloud and precipitation systems under different wind regimes.

We agree that compositing SST by regime would be an interesting extension. However, attributing SST variability directly to the identified cloud/precipitation regimes would require a more comprehensive coupled analysis than is feasible within the scope of the current manuscript. We have therefore revised the Discussion and Conclusion to better situate our results within the broader literature, while avoiding any implication of direct causality between the regimes and SST or bleaching outcomes (L413-420 & 445-455).

- Regarding case studies, we agree that targeted analysis can provide valuable process insight. However, given that the study already examines four distinct regimes, adding individual case studies for each regime would substantially expand the scope and move the manuscript away from its intended climatological/composite focus. For this reason, we have chosen to retain the regime-based composite framework, which is better suited to the objectives of the paper and provides a more robust statistical description of the system.

- We agree that the distinction between synoptic forcing and our wind-regime framework could be clarified further in the manuscript. The regimes used in this study were deliberately defined based on the background large-scale flow, given the tropical to subtropical nature of the GBR region, where convection and diurnal organisation are strongly influenced by the prevailing low-level winds and associated moisture transport. In this setting, a background wind-regime framework is likely more appropriate for characterising the large-scale environmental controls than a discrete synoptic classification approach more commonly applied in midlatitude regions.

  The identified wind regimes therefore already represent the large-scale dynamical environment diagnosed from the reanalysis and capture many of the key controls on the observed cloud and precipitation variability. We have revised the text to make this rationale clearer (L131-134). However, we have now incorporated the analysis of Convective Available Potential Energy (CAPE) and vertical velocity cross-sections from the BARRA-R2 reanalysis in Section 3.2, providing additional insight into the spatial patterns of instability and large-scale vertical motion associated with each regime.

- With respect to the suggestion of incorporating additional satellite rainfall products such as IMERG, we have considered this carefully. However, in the GBR coastal context, satellite precipitation products are known to have limitations due to their relatively coarse spatial resolution and reduced reliability in representing coastal and orographically influenced precipitation compared to the radar-based dataset used here. For this reason, we have chosen to rely on the higher-resolution radar observations for the primary analysis.

- We agree that the presentation and flow of the manuscript can be improved through more consistent referencing and organisation of the figure subplots. We have revised the text and figure ordering accordingly where appropriate.

**Specific comments**

**Comment 1**

L17 and 72-74: Make it clearer how this links to the coral bleaching season – is this the focus of the work? Or a mere motivation? Could provide further reiteration of the dynamics associated with the coral bleaching season and how extensively this is covered in the literature?

Updated L16-18 text:

> *Our findings emphasize the need for high-resolution simulations to better understand the convection initiation and propagation processes that shape cloud cover and rainfall variability over the GBR during the coral bleaching season, when cloud-radiation interactions may influence reef heat stress.*

Updated L78-98 text:

> *In the context of clouds and rainfall over the GBR, Zhao et al. (2022) provided a broad characterization of seasonal cloud properties which are influenced by local-scale forcings across large spatial scales. More recently, Dao et al. (2025) investigated wet-season rainfall propagation over northeast Queensland using a 180-day convection-permitting regional model simulation complemented by radar observations around Townsville. Their results demonstrated that interactions between land–sea breezes and large-scale background winds play a key role in regulating coastal rainfall patterns, particularly the offshore and onshore propagation of heavy rainfall systems. These studies highlight the importance of multiscale interactions in shaping coastal convection in the region. However, a more integrated understanding of diurnal cloud and rainfall evolution across coastal and marine environments remains needed to better characterize regional variability and its implications for coral bleaching risk. Building on this scope, we examine the diurnal variability of both clouds and rainfall during the Coral Bleaching Season (CBS) using multi-year Himawari-8 observations, BARRA-R2 regional reanalysis, and rainfall observations from three coastal radars. Unlike Dao et al. (2025) which focused primarily on rainfall propagation, we consider the coupled evolution of cloud and rainfall systems across coastal land, coastal ocean, and open ocean environments under different prevailing wind regimes, with a particular emphasis on the spatial and temporal variability of cloud-top characteristics, rainfall timing, and cloud–rainfall relationships associated with convection across northeast Queensland including the GBR.*
>
> *This study therefore addresses two primary scientific questions:*

*(1) What are the characteristics of the diurnal cloud and rainfall cycles over coastal and open ocean regions of north–east Queensland? (2) How do the background wind fields interact with local–scale processes to shape these diurnal patterns?*
*Focusing on the CBS period (January–April), when most CBEs in the GBR occur, we combine satellite, radar, and regional reanalysis datasets to characterize the diurnal variability of cloud and rainfall and examine the thermodynamic and dynamical factors that modulate them. Through this integrated analysis, we propose plausible mechanisms underlying the observed cloud and precipitation variability, providing a foundation for future targeted process studies.*

**Comment 2**
L31: Why might the diurnal cycle be misrepresented? Outline this here.

We have updated the text L36-45 to:

*However, climate models continue to systematically underestimate mean convection and rainfall in the tropics, often due to deficiencies in how they represent key processes controlling the diurnal cycle. In particular, many models have difficulty capturing the timing, amplitude, and propagation of convection because of limited spatial resolution and parameterized convection schemes that cannot fully resolve the interaction between boundary-layer evolution, surface heating, moisture convergence, and cloud–precipitation feedbacks (Tang et al., 2021; Covey et al., 2016; Xie et al., 2019; Christopoulos and Schneider, 2021). Recent advances in convection-permitting modeling, including emerging regional and a small number of global-scale simulations, have demonstrated substantial improvements in representing the diurnal cycle of convection and its propagation, although such approaches remain at the frontier of computational capability and are not yet widely available for long-term or ensemble climate applications (Clark et al., 2016; Hohenegger et al., 2008; Huang et al., 2026; Kendon et al., 2019; Pearson et al., 2014; Stevens et al., 2019).*

**Comment 3**
L194-195: Could you provide support for these dynamical/thermodynamical arguments through plots of cross sections and lat-lon plots using the BARRA reanalysis, rather than make sole inferences from the vertical profiles? Generally throughout this manuscript, more indications of what the broader synoptic environment is doing will be beneficial to both the reader in terms of understanding, and the authors in terms of reduced ‘over-hypothesising’ which I feel they may trip up on sometimes – which may purely be a limitation of data availability.

Thank you for this suggestion. To complement the theta-e analysis, we have further analysed corresponding Convective Available Potential Energy (CAPE) and vertical velocity cross-sections from the BARRA-R2 reanalysis, providing additional insight into the spatial structure of instability and large-scale vertical motion associated with each regime. The results of this analysis have been incorporated into Section 3.2.

**Comment 4**
L198-209: See my comments below for suggested changes to Figure 5. This paragraph is generally hard to follow, and it would be more useful to discuss each region, or each regime in turn, with the associated labels for each panel altered so that it follows the order of discussion, for which it is necessary you refer back to, otherwise the reader will lose their positioning. If there are diagnostics you could plot representing the intensity of convection/rainfall which follow the regions of high/low equivalent potential temperature, this would also bolster your argument linking the thermodynamics with convection.

We thank the reviewer for this suggestion. We agree that presenting the results in a strictly region-by-region or regime-by-regime sequence could improve local readability for individual figures. However, we have deliberately structured the discussion of this figure around physical processes rather than repeating the same sequence across multiple figures.

Specifically, organising the analysis by regime or region in a fixed order for each figure would lead to a high degree of repetition, as the same set of comparisons (regimes × sites × levels) would need to be re-stated multiple times with only minor variations in emphasis. Instead, our current approach aims to highlight the dominant physical contrasts (namely land–sea differences and regime-dependent thermodynamic structure) within a single coherent narrative.

We therefore retain the current structure, as we believe it provides a clearer synthesis of the key thermodynamic behaviours while avoiding unnecessary repetition. In addition, and in response to Reviewer 1's broader comment regarding clarity and physical interpretation, we have revised this section to further strengthen the emphasis on underlying physical mechanisms and to improve the overall flow of the discussion. However, we have revised the text to improve signposting and referencing of specific panels to help guide the reader through the figures more clearly.

**Comment 5**
L218 onwards/Section 3.3/Figure 6: Given you discuss each regime in turn and how they differ between Townsville, Cairns and Wallis, would it potentially be more useful to have the subpanels done for each regime, as in Figure 5, and then overlay different coloured/styled lines for T, C, W, and for Land v Ocean? This would make your argument flow better with reference to your provided figures.

We have updated Fig. 6 as suggested (subpanels indicating wind regime rather than site) to improve the flow of section 3.3.

**Comment 6**
Section 3.4: Similar, discuss the regimes in turn with respect to the ways in which you plot the panels. When you refer to values of BT, instead of specific values, could plot thresholds in which reflect convection or certain intensities (e.g. where deep vs shallow convection is present)?

We have adjusted the text in this section to match the order in which the Fig 7 panels are shown.

Regarding BT thresholds: studies in the deep tropics typically require additional analyses (such as cloud top height retrievals or additional BT wavelengths rather than the 10.4 µm BT alone) to reliably distinguish deep convective cores (Lopez-Bravo et al., 2023). We also note (L279–280, L298–299) that the coldest BTs in our analysis tend to lag the heaviest rainfall, suggesting they reflect anvil remnants rather than active convective cores.

Rather than citing specific BT values in the text, we have added labelled contours to Fig. 7 at 253, 263, 273, and 283 K (with lighter contours every 5 K). These are referenced throughout Section 3.4, with colder contours (253, 263 K) indicative of deeper or more intense convection and warmer contours (273, 283 K) indicative of shallower or less frequent convection.

**Comment 7**
L291-293: This statement is hard to justify, particularly as changes to BT with the overlays of precipitation don't distinctly show that propagation, compared to say the northwesterly regime in Figure 7a! How do the propagations link with one another? Can Cairns convection propagation reach Wallis? This is where the usage of IMERG GPM could provide some utility.

We have removed L291-293. With regards to the use of IMERG GPM please see our response to the general manuscript comments (dot point #4).

**Comment 8**
Figure 7: This figure could be improved by labelling the precise regions of the Cairns and Willis radars. Much of the BT propagation is hard to see with the current colorbar choices, and the black contours indicating the 273K threshold, and the lines representing the directions of rainfall/cloud propagation, and the coastline are also very hard to see. I would suggest plotting where the raised elevation over NE Australia is too, to give indications of links with orography. I would also be interested to see how these plots would appear with low level equivalent potential temperature plotted in addition, which would give some further indications of the links between the thermodynamic and convective environments. Overall, reformatting of this figure is recommended.

We have updated Fig 7 as suggested by labelling the locations of Cairns and Willis Island radars, adjusting the figure formatting for clarity, and adding orography. We have chosen to not include theta-e in this figure to maintain readability and clarity.

**Technical corrections**

**Comment 9**
L20-21: Sea Surface Temperatures should be in lower case.

We have updated this line.

**Comment 10**
L30: You mention extensive research on MC diurnal cycles but do not put the references here – I would suggest placing some of the key MC diurnal cycle citations here (you already mention quite the few, so this should be an easy fix).

We have included references here as suggested.

**Comment 11**
L35: ‘afternoon-to-early evening’.

Text updated.

**Comment 12**
L40: ‘can extend up to 700 km away from the coast’?

Text updated.

**Comment 13**
L63-L67: This should be a separate paragraph, before then a paragraph outlining what the study aims to do.

We have updated as suggested.

**Comment 14**
L79: What resolution was the BT data regridded to? The same 2km spatial resolution?

We have adjusted the text to include this detail, see L103:

> *The BT data was regridded for the study domain at 2 km spatial resolution, …*

**Comment 15**
L103-104: Move these lines, perhaps, to the start of Section 2.2, and then removed the ‘as described Section 2.2’ portion?

We have updated as suggested.

**Comment 16**
L113: Briefly outline how equivalent potential temperature was derived using T and q.

We thank the reviewer for this comment. See relevant text updated in L144-146:

> *Profiles were constructed at the BARRA-R2 grid point nearest to each radar site (Townsville, Cairns, and Willis Island), using temperature and specific humidity fields across all pressure levels, calculated using the open-source MetPy Python package.*

**Comment 17**
Figure 1: The scatter points for radar and AWS are a bit hard to see., similar with the boundaries of the radar coverage (though this a bit more pedantic). How were reef locations defined/determined for plotting? I’d also put the NOAA (2006) citation for ETOPO2 into the figure caption. ‘Hovmoller analyses were conducted?’

Fig 1 has been updated with the reef location dataset citation which was sourced from the Great Barrier Reef Marine Park Authority.

**Comment 18**
Figure 2: The large grey circle outlining the radar range is hard to see in panels a-c.

We have chosen to remove the large grey circle to improve readability of the BT contours.

**Comment 19**
Figure 3: Could the r axis on the wind-rose diagrams be made uniform for the three radar regions?

This has been updated.

**Comment 20**
L176-182: Portions of this should go into the methodology where you first introduced theta_e.

The theta-e explanation has been moved to section 2.

**Comment 21**
L189-191: Rephrase this sentence for clarity, otherwise this section reads well.

We have rephrased this sentence.

**Comment 22**
L191: 'over land, there are modest positive...'

We have updated this line.

**Comment 23**
L200: Stronger diurnal modulation is primarily for the lower-troposphere? Make this clear.

We have updated the text for clarity.

**Comment 24**
Figure 5: Change the colormap and potentially the levels you plot, with specific contours as lines, to really highlight the differences for each region and regime as it is currently hard to decipher the results.

We have updated Fig 5 to show theta-e anomalies for each wind regime relative to a day-of-year climatology - to remove seasonal signals which may have been present. Using the difference plots also allows us to better highlight the differences between each regime. We also show the diurnal climatology across the CBS for reference.

**Comment 25**
L215: Figure A1?

We have updated the text.

**Comment 26**
L220: 'by 19 LT'

We have updated the text for clarity.

**Comment 27**
L234: 'BT and rainfall diurnal cycles, in the south-easterly regime, over Cairns...'

We have updated the text.

**Comment 28**
L308: Figure A2?

We have updated the text.

**Comment 29**
L313: Which Figure are you referring to here?

We have updated the text for clarity.

**Comment 30**
Figure 8: As suggested for Figure 3. Small edit.

We have changed as suggested for Fig 3.

**Comment 31**
Line 331: Coastal enhancement of what?

We have updated the text L390:

> *Coastal enhancement of convection is persistent, driven by the combined effects of sea breeze convergence and orographic uplift.*

**Comment 32**
Figure 9: Maybe state that the gravity waves are inferred from the analysis presented? If the analysis isn't wildly modified. Which supplementary Figure are you referring to – Table A1?

We have updated the Fig. 9 caption:

> *The afternoon land sea breeze is shown by black arrows with relative maximum wind speeds indicated by arrow length (see supplementary Fig. A1). *Mechanism inferred from estimated propagation speeds consistent with gravity wave propagation in literature - dashed orange oscillating arrows indicate the nighttime offshore propagation of convection induced by gravity waves.*