We would like to thank the reviewers for their insightful comments and suggestions on our manuscript. The feedback has been most helpful in improving the quality and clarity of the current manuscript. Detailed responses to the reviewer comments can be found below.

Reviewer 1

It was a pleasure to read this very well written and clear paper. The only major general comment I have is related to the need for a clearer discussion of the nature and mechanisms leading to tropical-extratropical interactions as described in my comments 7 and 9 below. Otherwise, I have only minor comments listed below:

1.1) L. 39-40: it is interesting that the authors mention a reduction of surface fluxes due to weaker trade winds: in cases of extratropical dry intrusions investigated over the Noth Atlantic enhanced surface fluxes were found due to the supply of dry air (Aemisegger, et al. 2021, Villiger et al. 2022). Furthermore, Villiger et al. 2022 shows that extratropical dry intrusions provide a strong large-scale forcing for low-level cloud patterns and their related cloud radiative effects, which impacts the surface energy budget and the sea surface warming driven by the atmosphere. Depending on their interaction with the trailing cold front extratropical dry intrusions can arrive with a moist or dry anomaly in the western tropical North Atlantic.

This is an interesting observation, but there are significant differences between these cases and the GBR. The reduction in surface fluxes across the GBR was noted in our previous study (Richards et al. 2024) that did not include any dry intrusion analysis. The explanation for the reduced fluxes come from the formation of a moist and still surface layer (which can be seen in figure 7) that forms following the collapse of the trade winds into the doldrums. This causes a weakening of the latent heat flux. While we did not investigate the DI interactions with low levels clouds in this study, in Richards et al (2026), a general reduction in cloud cover and mainly low clouds is seen in the doldrums.

Richards, L. S., Siems, S. T., Huang, Y., Zhao, W., Harrison, D. P., Manton, M. J., & Reeder, M. J. (2024). The meteorological drivers of mass coral bleaching on the central Great Barrier Reef during the 2022 La Niña. *Scientific reports*, *14*(1), 23867.

Richards, L. S., Siems, S. T., Huang, Y., Harrison, D. P., & Zhao, W. (2026). Trade wind regimes during the Great Barrier Reef coral bleaching season. *Weather and climate dynamics*, *7*(1), 109-127.

1.2) L. 46: “the processes leading to their formation and persistence? are less clear”

Line changed

1.3) Paragraph L.55-L65: De Vries 2021 would be an important reference in this context

Reference added

1.4) L. 101: “influence on moisture transport ?»

Line changed

1.5) L. 133 : add Wernli and Davies, 1997 as the pioneering publication for LAGRANTO

Reference added

1.6) Section 2.4: Here the trajectory setup for investigating the relevance of dry intrusions could be optimised by running the trajectories backward and forward which may increase the number of identified dry intrusions especially considering that the dry intrusions are searched for throughout the troposphere.

Running both forwards and back trajectories would likely increase the number of dry intrusions identified. As identifying dry intrusions was not the key focus of this paper, the method was originally kept to just running back trajectories. While it is unlikely the extra identified trajectories would significantly change the results of the current paper, this technique would be useful for further investigation of the dry intrusions over the region.

1.7) L. 189: Here a reference to Fig. 5 would be needed otherwise the reader doesn’t understand where the statement about the warm conveyor belt comes from. In general, to me this is the most critical point in the paper: the aspect related to the potential existence of a warm conveyor belt playing the role of a stabiliser for the downstream blocking is too vague and not well supported by explicit analyses done in the paper. I would suggest to explicitly phrase this as a hypothesis and matter of future exploration. Even though, I think the authors could easily run trajectories to check for potential WCBs as they did for DIs. This would also reinforce the discussion of tropical-extratropical interactions in a mechanistic way via Lagrangian analyses.

A reference to figure 5 has been added to indicate the warm conveyor belt reference in the moist-ascending region. A new figure 6 has been added which shows the density of

warm conveyor belt structures. Further references in this paragraph have been made to the new warm conveyor belt figure, as well as details of the warm conveyor belt identification in section 2.4 of the methods.

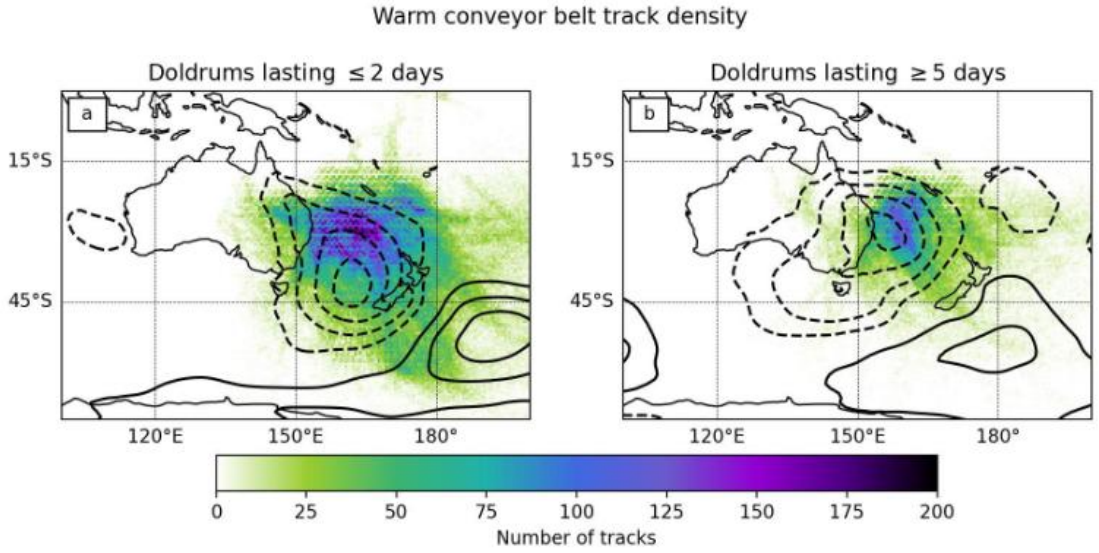


Figure 6. Warm conveyor belt track density (shaded) for short and long doldrum cases showing only the first day of each event. The contours represent the MSLP anomalies (1 hPa intervals, 0 contour removed, dashed is negative, solid is positive

1.8) L. 194: cease ◊ disappear?

Line changed

1.9) L. 196-200: I think there is a break in narrative flow here leading over to 3.2 tropical dynamics. The statement "The longer doldrums also see larger dry, descending region forming over the GBR and north-eastern Australia…" gained from the Eulerian composites in Figs. 4 and 5 would call for the Lagrangian analysis that however comes only in 3.3. I recommend switching the order of Sections 3.2 and 3.3. Since the Eulerian analysis in Section 3.1 identifies a significant dry, descending region, the manuscript should immediately follow with the Lagrangian analysis of those air parcels (currently 3.3) to maintain structural coherence.

This is a nice suggestion. Results have been switched to 3.1 extratropical, 3.2 dry intrusions, 3.3 tropical

1.10) Following this, the authors should incorporate a transition that defines the tropical-extratropical coupling as an important driver of event persistence (?), thereby providing a clear rationale for the subsequent tropical wave analysis. I understand that the suppressed phases of the MJO and ER wave must align with stalled Rossby wave packets to prevent the return of trade winds. But I am left hanging with respect to the nature of the mechanistic link between the tropical

and extratropical dynamics. I think this aspect could be strengthened. Maybe my earlier comment about the missing explicit warm conveyor belt analysis could help.

The original rational for looking at tropical waves during the doldrums was due to the doldrums and Great Barrier Reefs tropical location. This is explained at the start of section 3.3.

The discussion later ties in the Rossby wave analysis and tropical wave analysis from a tropical-extratropical interactions perspective. The main mechanistic link between the systems does appear to be moisture, which is strengthened by identifying the warm conveyor belts. This is discussed in paragraph 3 lines 321-333 of the discussion.

1.11) L. 205: OLR as a measure for the strength of tropical convection?

Line changed to 'As a measure of the strength of tropical convection, Hovmöllers …'

1.12) L. 246: Given that this is a section on a Lagrangian analysis: please make clear that at this point in the text you are arguing based on a Eulerian perspective.

We have added to following sentences to the start of the section 3.2.1 "Until this point, this study has explored the doldrums formation from a Eulerian perspective. For this section we adopt a Lagrangian perspective to identify and investigate dry intrusions over the Davies Reef region."

1.13) L. 328: This cooling and moistening of extratropical dry intrusions upon entry into the boundary layer has been previously noted in Fig. 15 in Aemisegger et al. 2021 (about 6 K in total) and Fig. 8 in Villiger et al. 2022 as being likely linked to cloud hydrometeor evaporation (and partly also radiative cooling induced just above the cloud layer). Please refer to these papers and add the magnitude of the strong cooling you find.

References to the cooling and moistening in Aemisegger et al 2021 and Villiger et al. 2022 have now been included in paragraph 4 of the discussion.

"… The properties traced along the dry intrusions associated with doldrum events act similarly to previous studies where diabatic cooling and moistening is observed as the parcel descends s (Raveh-Rubin, 2017; Aemisegger et al., 2021; Villiger et al., 2022). However, in this study, some parcels that descend into the boundary layer cool up to 10 K over 48 hours, which is far more than has previously noted 6 K by Aemisegger et al. (2021). Notably, the parcels experiencing the strongest diabatic cooling are found closest to the surface, where increased moistening occurs as they pass over the Coral Sea (Fig. 8a-b) likely in a similar process described in Villiger et al. (2022). …"

1.14) Section 5: in the conclusions I miss a discussion of the limitations of this study and an outlook into the need for future research, in particular with respect to the coupling between tropical and extratropical dynamics.

A limitations and future work section has been added to the end of the discussion

“Lastly, while the results of this study highlight the many processes involved in the doldrums formation, it is important to address the potential limitations behind our definition of the doldrums. In this study, as in Richards et al. (2026), a single day is assigned a cluster based on the 0000 UTC profile at Davies Reef alone. Thus, many days would sit on the fringe of what is defined a doldrums day. This limitation is more prominent when determining the duration of the doldrums, as doldrums events can be broken up by a single non-doldrums day, meaning the duration of the events are potentially longer than shown in Fig. 1b. This clustering method also does not account for any diurnal changes and may not be representative of the doldrums in a nocturnal environment. While a basic wind speed definition can also be used, similar issues will likely arise as remain with low winds but not below the typical 3 m s-1 threshold.

This study has also focused exclusively on weather-scale features in opposition to the more typical climate variability approach to coral bleaching meteorology. Given the GBR's location in the tropical Pacific, the modes of interannual variability that affect the Pacific trade winds like the El Niño Southern Oscillation and the Interdecadal Pacific Oscillation could also influence some of the dynamical processes identified in this study. This approach may be more useful when addressing how the doldrums persistence differs on a year-to-year basis, connecting with the established research on modulations in Pacific trade wind strength (L’Heureux et al., 2013; England et al., 2014), which may provide further insights into what determines a coral bleaching or marine heatwave year on the GBR. Furthermore, given the study period of December - April, further research would benefit from investigating the timing of monsoon bursts and breaks within the Australian monsoon given their links to RWB (Berry and Reeder, 2016; O’Brien and Reeder, 2017). Further investigation of the Australian monsoon and the movement of tropical moisture may reveal more details on the tropical-extratropical coupling identified in the present study.”

Typos:

- L. 21 Zzone ◊ Zone
  fixed
- Doldrum is sometimes used in singular sometimes in plural, is this on purpose? (e.g. L7 abstract doldrums formation? or doldrum formation

Most instances of doldrum have been changed to doldrums

- L. 53: and cut-off low formation over eastern Australia (Richards et al., 2024))
  fixed
- L. 104: one-sided t-test?
  fixed
- L. 117 Missing word (method?): “The Tibaldi et al. (1994) was found to best capture southern hemisphere blocking”
  fixed
- L. 133: “lagrangian” ◊ Lagrangian throughout the manuscript
  fixed
- L. 219 double is: The actual co-occurrence of the suppressed MJO and ER wave is is modest at day
  fixed
- L. 299-300: repetition of “also”
  fixed
- Fig. caption 7: composited using only the days was a suppressed wave over the GBR.. ◊ where there was?
  Fixed

Reviewer 2

This is an impressive paper, and it was a pleasure to review it. It is well written, the figures are clear, and the quality is high. The different analysis steps are clearly outlined and scientifically sound, and the methods and assumptions applied are valid. The results are summarised nicely in a schematic overview figure. I have listed a few items below that might be helpful for a revised version of the paper.

Major comments

2.1) The article should make it clear right from the start how it differs from its predecessor paper in WCD (Richards et al, 2026). For example in their already published paper the authors write in their abstract: ” …., we find the formation of the doldrums is linked to the passage of a Rossby-wave train over eastern Australia……” and in the current paper abstract:”…. We show that doldrums formation is linked to the passage of a Rossby wave packet,….”. This sounds quite similar to me. In my opinion, the paper would benefit from a clearer distinction from its predecessor, even in the abstract. In the introduction section,

the authors could dedicate a short paragraph to explaining the results of their previous work and the specific questions that this follow-up paper addresses.

This line in the abstract has been adjusted to “Expanding upon previous links between the doldrums formation and Rossby waves, the cyclonic anomaly within the Rossby wave packet is found to disrupt the typical trade wind easterlies over the GBR through anomalous westerlies over eastern Australia.”

Paragraph 4 of the introduction has also been expanded to clarify the difference between the two papers. “Richards et al. (2026) later identified these cut-off lows as the start of a large Rossby wave packet that extends across the Pacific during the doldrums. However, no analysis was performed on how these Rossby waves connect to the doldrums formation and what other weather systems may be involved in their formation and maintenance.”

2.2) I wonder about the role of interannual variability and large-scale dynamics e.g. ENSO, how could they influence your results. A short discussion about their potential impact in section 4 would have been quite useful.

We have now included some discussion around ENSO and interannual variability in the new limitations section of the discussion

“This study has also focused exclusively on weather-scale features in opposition to the more typical climate variability approach to coral bleaching meteorology. Given the GBR's location in the tropical Pacific, the modes of interannual variability that affect the Pacific trade winds like the El Nino Southern Oscillation and the Interdecadal Pacific Oscillation could also influence some of the dynamical processes identified in this study. This approach may be more useful when addressing how the doldrums persistence differs on a year-to-year basis, connecting with the established research on modulations in Pacific trade wind strength (L’Heureux et al., 2013; England et al., 2014), which may provide further insights into what determines a coral bleaching or marine heatwave year on the GBR.”

2.3) I also miss a critical discussion of the limitations of the current study, what is currently missing, what might be potential error sources and what could be improved in further studies.

Along with the section noted above, we have included discussion around the studies limitations which mainly involve the method of doldrums identification

“Lastly, while the results of this study highlight the many processes involved in the doldrums formation, it is important to address the potential limitations behind our definition of the doldrums. In this study, as in Richards et al. (2026), a single day is assigned a cluster based on the 0000 UTC profile at Davies Reef alone. Thus, many days

would sit on the fringe of what is defined a doldrums day. This limitation is more prominent when determining the duration of the doldrums, as doldrum events can be broken up by a single non-doldrums day, meaning the duration of the events are potentially longer than shown in Fig. 1b. This clustering method also does not account for any diurnal changes and may not be representative of the doldrums in a nocturnal environment. While a basic wind speed definition can also be used, similar issues will likely arise as remain with low winds but not below the typical 3 ms threshold. “

Specific comments

- Line 21: Zone instead of zZone

fixed

- Line 78: As mentioned above the link to the previous paper could be a bit more elaborated

As described above, changes have been made to lines 54-46 to help clarify this difference

- Line 86: How dependent are your results from the selection of the time period, do you see a trend there?

Specific trends in doldrums duration were not analysed in this manuscript. In Richards et al. (2026) the duration events were split into coral bleaching and non-bleaching years however the differences were not significant. The doldrums trends extending past 1996 have not been studied in any detail in any of our previous studies either.

- Line 144: Did you develop this method or employ it? For the first a short explanation, for the latter a citation is missing.

Citation added here. “We modify the Raveh-Rubin (2017) method…”

- Line 319: better a bit more specific here e.g.: over the tropical Atlantic

Line adjusted to ‘GBR doldrums are associated with dry, descending air as found in other regions, such as the tropical Atlantic …’

- Line 324: This process is similar to studies of other tropical dry intrusions. Aemisegger et al. found for example ….

Line has been re-written to include new reference “Studies of tropical dry intrusions with extratropical origins have noted similar interactions where RWB precedes the dry intrusion formation. In Barbados, RWB also weakened the trade winds prior to the dry intrusion forming (Aemisegger et al. 2021), while Villiger et al. (2022) noted the dry intrusions were steered towards the region by upper-level RWB before descending into the cold sector of an extratropical cyclone from ~500 hPa. …”

• Line 327: “to the dry intrusions from Raveh-Rubin (2017)“ ? unspecific

This line was re-written to “The properties traced along the dry intrusions during doldrum events act similarly to previous studies where diabatic cooling and moistening is observed as the parcel descends s (Raveh-Rubin, 2017; Aemisegger et al., 2021; Villiger et al., 2022).”

• Line 355: …. the formation and persistence of GBR doldrums in Austral summer and autumn, highlighting…

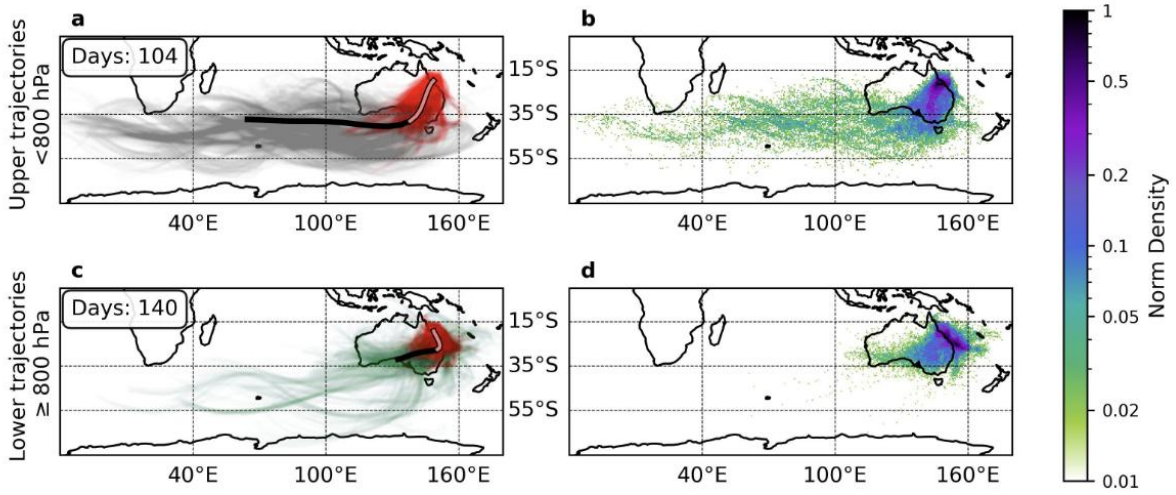
Changed to “.. over the December to April period ..”

• Figure 8: I am not sure if Hovmöller composites is here the right word?

Changed to just composites

• Figure 9: The mean trajectory path in panel c) is difficult to read, maybe another color?

A black outline has been added to the mean line and the colours have been adjusted to improve the contrast.



• Figure 10: The red line lies with 50 hours so the test is a bit confusing

The red line was placed to the left of the violin plot to not look like an extension of the plot lines.

- References - Some links were not working, I listed them below.

All links have now been checked and corrected