RC1

In this manuscript, the authors demonstrate the importance of extratropical disturbances in the development and persistence of South Pacific Convergence Zone events. The innovative methodology combines the objective identification of cloud band events and PV structures, providing a robust link between the extratropical forcing and the organized convective activity in the subtropics. The results also advance the understanding of the dynamic environment necessary to form and sustain persistent cloud bands and could be extended to other regions. However, the manuscript could benefit from a more in-depth discussion of the main findings in light of previous research. I also have some minor questions related to details in the methodology, with clarification of these points facilitating the future reproducibility of the results. Thus, I would recommend the manuscript undergo minor revisions before acceptance and publications.

Major Comments

1. Section 3.3. In this section, the authors propose a mechanism for the formation of the cloud bands based on the presence of PV structures. On the poleward side of the CB centre, cyclonic PV anomalies destabilise the lower troposphere and induces convection and ascending motion. On the other hand, the diabatic heat released by the enhanced convection contributes to the anticyclonic PV anomalies ahead of the cloud band axis. The mechanism is similar to that proposed by Van der Wiel et al (2015). According to those authors, the propagation of the Rossby wave trains forming the SPCZ is subjected by two main forcings: horizontal advection of cyclonic vorticity anomalies, dictating the propagation of the RW into the tropics, counteracted by anticyclonic vorticity tendencies from vortex stretching due to the enhanced convection and ascent at the SPCZ axis. Similar mechanisms were also observed in the SACZ region. Thus, it would be informative to discuss the results obtained by the authors with those from previous literature. This discussion could also be included in the conclusions.

We thank the reviewer for highlighting this connection. We agree that the PV dipole evolution observed in our composites is dynamically equivalent to the vorticity budget mechanism detailed by van der Wiel et al. (2015). To address this, we have revised Section 3.3 to link our observation of the diabatically generated anticyclonic PV anomaly to their findings on anticyclonic vorticity generation via vortex stretching. As suggested, we have extended our conclusions.

2. Zilli and Hart (2021) demonstrated that transient cloud bands over South America occur poleward of the persistent ones. They also showed that the relationship between the vorticity tendencies and the local environment are

different for these events when compared to the more tropical persistent cloud bands. In the manuscript, the authors also mentioned this in section 3.6.2. However, the main analysis in sections 3.3 and 3.4 does not take this into account. Results in Fig 5 also suggest a clear seasonality in the transient cloud bands, with austral spring and summer events occurring at similar latitude as persistent events, but austral fall and winter transient events located primarily south of 20°S. To what extent does the local environment of the cloud bands could increase the variability and affect the composite results in Figs 3 and 4?

We thank the reviewer for this insightful comment. We agree that transient cloud bands span a wider range of local environments compared to persistent cloud band events. Because long-lived cloud bands are strictly confined to the austral warm season, their composites in Figures 3 and 4 inherently reflect a specific, dynamically consistent summer environment. In contrast, short-lived cloud band events occur year-round, whose signal dissipate in these Figures 3 and 4.

To explicitly address this seasonal and spatial variability, we have extended section 3.5 and introduced a new Figure 6. This figure partitions the composites into austral summer (DJF) and winter (JJA) and separates the different local environments.

3. One of the findings of the manuscript is that "… long-lived cloud bands are distinguished [from transient events] by a higher frequency of upstream PV structures persisting throughout the cloud band lifecycle." (lines 323-333). The frequency of upstream PV structures depends on the upstream area of the cloud band, as illustrated in Figure A1. However, this area can vary massively among cloud band days, with the events in Figure 1 serving as a great example to that. Thus, what is the relationship between the persistence of the cloud band and the upstream area, and to which extend this can affect the frequency of PV structures associated with the events.

We thank the reviewer for raising this point. The reviewer is right to point out that the size of the adaptive upstream sector naturally scales with the size of the cloud band, and a larger search area statistically increases the probability of capturing a PV structure.

Firstly, we acknowledge that our terminology was imprecise and contributed to this confusion. We have updated the text and figures in Section 3.6.1 replacing "PV occurrence frequency" with the more accurate term: "Fraction of cloud bands connected to a PV structure" (the metric is not a spatial density count of multiple PV structures).

With regard to the area bias, although long-lived cloud bands have larger upstream sectors, this geometric bias is partially offset because they occur at equatorward latitudes where PV intrusions are climatologically rare.

Nevertheless, to ensure that our conclusions regarding sustained dynamical forcing are not simply an artefact of the size of the search area, we have specifically analyzed area-independent properties, i.e., PV depth (Figure 7e) and equatorward-most latitude (Figure 7f). While a larger upstream sector might artificially increase the likelihood of simply finding a PV structure, it cannot artificially make that structure vertically deeper, nor can it force the structure further into the deep tropics.

Finally, since long-lived cloud bands are consistently associated with PV structures that are significantly deeper and extend further equatorward (before cloud band genesis) than those of short-lived events, we are confident that this relationship reflects a genuine, robust dynamical forcing mechanism rather than a geometric artefact.

We have added a concluding clarification to Section 3.6.2 to ensure this distinction is more clear to the reader.

Minor Comments

1. The analysis considers ERA5 data since 1959. How reliable are ERA5 data, and in particular OLR, for the years before 1979? Would the same results be obtained if considering only data after 1979?

We thank the reviewer for raising this important concern regarding the reliability of ERA5 data prior to 1979. Our initial sensitivity tests between the 1959–2021 and 1979–2021 periods yielded similar patterns. However, Soci et al. (2024; DOI:10.1002/qj.4803) evaluated the ERA5 global reanalysis back to 1940 and explicitly noted that for the Southern Hemisphere during the early period, the reanalysis representation exhibits high ensemble spread due to sparser conventional observations compared to the Northern Hemisphere. Furthermore, as the other reviewer also noted, the introduction of satellite data in 1978 marked an improvement in data quality and the accurate capture of synoptic variability in the Southern Hemisphere (Tennant, 2004; https://doi.org/10.1029/2004GL019751).
We agree that utilizing pre-satellite data may introduce some uncertainty. Consequently, we have narrowed the focus of this study entirely to the satellite era (1979–2021). All results and figures in the manuscript have been updated accordingly.

2. The caption suggests that the contours in Fig 2a represent the number of days with cloud bands. However, a maximum of ~50 days per year is below what would be expected for the region. Previous papers (Haffke & Magnusdottir, 2013) estimated that the SPCZ is present in 30-40% of the NDJFMA months, with would be equivalent to ~70 days per year. From Figure 6a, the detection algorithm identified ~3000 cloud band events which, considering 63 years

(1959-2021), results in 48 events per year, on average. This number is close to the numbers represented by the contours in Fig 2a. Thus, I would like to double-check if the information in Figs 2a and 2b are cloud band days or cloud band events? Reference: Haffke, C., & Magnusdottir, G. (2013). The South Pacific Convergence Zone in three decades of satellite images. Journal of Geophysical Research: Atmospheres, 118(19), 10,839-10,849. https://doi.org/10.1002/jgrd.50838

We thank the reviewer for their careful attention to these numbers. We can confirm that the contours in Figure 2a represent the average number of cloud band days per year at a given grid point, as stated in the caption, rather than the total number of events. We have updated the caption to ensure this distinction is completely clear.

Additionally, to be absolutely certain, we calculated the exact daily counts directly with our detection algorithm. We found the basin as a whole experiences a cloud band on ~197 days per year (i.e., ~54% of the days in a year). Because Figure 2a maps spatial density, the maximum contour of ~50 simply indicates that the most active grid points intersect these cloud bands roughly 50 days out of the year.

Regarding the comparison to Haffke & Magnusdottir (2013), our maximum of ~50 days/year per pixel is consistent with their findings, when comparing the same spatial regions. The reviewer notes their 30-40% estimate (~54-72 days over their 181-day NDJFMA season). However, Haffke & Magnusdottir (2013) state that SPCZ activity is present less than 30% of the time in the subtropics (< 54 days), which aligns with our number (taking into account the whole year).
Moreover, our algorithm incorporates geometric criteria, requiring the cloud band to extend continuously into the mid-latitudes. Because we isolate this specific diagonal, extended configuration, our frequency of occurrence is expected to be a lower, subset fraction of the broader SPCZ climatology reported by Haffke & Magnusdottir (2013).

Finally, we also corrected Figuure 6a, which shows correctly now the right number of tracked cloud bands, which amount for ~3900 (the sum of short-, long-lived and transitional cloud band events, shown on the figure.)

3. A suggestion for figure 2c: repeat the contours from Fig 2a here, for reference.

Thank you for the suggestion, we have updated the figure.

4. The caption in Fig 3 refers to more panels than included. Also, the sentence “The white contour represents the 210 Wm−2 cloud band detection threshold.” refers only to panels a-c, so it would be better to place it with the description of these panels.

Thank you for noticing this error. The figure caption has been corrected.

5. In Figures 3 and 4, the composites for day +2 are centred in the centroid of the cloud bands objects detected in day +2? If so, I would expect to have some OLR values below 210 W.m-2 in Fig 4f. Furthermore, what is the centroid considered for those events persisting only one day? If not, what is the reference considered? Please, clarify this in Session 2.3.2.

We thank the reviewer for pointing out this ambiguity. In Figures 3 and 4, the spatial reference frame is strictly fixed to the location of the cloud band centroid at genesis (day 0). Therefore, the composites at lag -2 and lag +2 display the atmospheric conditions at this static genesis location, instead of tracking the cloud band's propagation. This explains the absence of OLR values below 210 W m-2 at lag +2: the persistent cloud band has propagated away from its genesis location.
To make it perfectly clear, we have updated the caption for Figure 3 and explicitly clarified the distinction between our fixed-reference and moving-reference composite methodologies in Section 2.3.2.

6. In section 2.1.3, cloud band events lasting less than 3 days are called “transient” while those persisting 4 or more days are called “persistent”. However, in some parts of the manuscript, “transient”, “short” or “short-lived” events are used interchangeably. See, for example, the caption of figs 3 and 4, as well as the label of the first row in Fig 4. Similarly, “persistent” events are also referred to as “long” or “long-lived” (as in line 332). Consider using only one of the terms.

We have homogenized how we refer to the cloud band events, i.e., only short-lived and long-lived.

7. Figure 6: please, include a description of the significance tests used here (Wilson score and the bootstrap confidence interval) to the methodology section.

We thank the reviewer for this proposition. We have added such description at the end of section 2.3.2.

RC2

Review of “ The role of Rossby wave breaking in the formation and maintence of tropical-extratropocal cloud bands over the South Pacific” By Pilon et al

Summary

This study studies the relationship between TE cloud bands and RWB and shows that there exists as robust link between PV structures and TE events, which was established by means of composite analysis and statistical significance. As reviewer I believe that this paper warrants publication; there are however matters of significant concern that should be addressed by the authors before it can be accepted.

Major comments

1. This manuscript is too brief and the analysis could be expanded and many sections for example 3.5 require more expansion and discussion. There is a lot of room for that in the paper, so that the analysis is at the level of papers we see in this journal.

We have expanded Section 3.5 and added composites for the austral summer and winter (now Figure 6). This addition strengthens the manuscript by investigating how seasonal background states dictate cloud band lifecycles.
Additionally, we have moved the Appendix A2 to the main text (now section 3.6.3 and Figure 8) to better support our findings). Integrating this into the main text provides visual and spatial corroboration of our findings.
We think that the length of the manuscript is within the range of papers published in this journal.

2. Figure 3 appears to be incomplete which makes it very difficult for readers to assess the discussion in Section 3.3, please look into this. Information about the vertical motion is completely missing because it should be showing in panels (j) to (i) which are not presented.

We apologize for this oversight. During the revision process of the figures, the vertical-meridional cross-sections were removed, since they do not bring much more information, while the text referencing them was left in the manuscript. We brought the panels back in the figure (4).

3. In Line 340 claims that “-extending Rossby wave breaking events facilitate the cloud band genesis”. Evidence of the fact that the RWB presented in the composites has this morphology is missing, as far as one can tell. It the opinion of this reviewer that the authors should demonstrate why they are saying this. If anything, the green contours representing the 500 hPa anomalies in Figure 3 suggest that the RWB is poleward (Peters and Waugh 1996, Barnes et al.

2025). This is major issue that needs to be addressed. The RWB need to be characterised properly before compositing to determine whether they are anticyclonic and poleward or anticyclonic and equatorward, because this has profound implications on the jet streaks that materialise for these categories, and therefore the transverse circulations that these jet streaks induce.

We thank the reviewer for this comment and for raising the important dynamical distinction between poleward and equatorward anticyclonic RWB morphology.
The primary aim of our study is to quantify the lifecycle and maintenance of cloud bands in relation to the general presence, depth, and persistence of upstream PV structures, rather than to classify the specific RWB archetype. Performing a separate morphological categorization of the RWB prior to compositing falls beyond the intended scope of this current work.
However, upon carefully re-evaluating our composite figures, and the cited studies by the referee, we agree with the referee that we cannot ensure whether RWB associated with the CBs are primarily equatorward anticyclonic or poleward anticyclonic RWB. Therefore, we have removed any inferences from the text referring to equatorward RWB.

Minor comments

1. Lines 41 – 44: Several studies have considered the morphologies of RWB in idealised models and reanalyses and should be referenced here. As the issue of morphology appears to be a critical issues as suggested above, the authors should be a bit more explicit about this.

We agree that the specific morphology of Rossby wave breaking provides important dynamical context. We have updated the introduction to reference literature on this topic (e.g., Thorncroft et al., 1993; Peters and Waugh, 1996; Barnes et al., 2025). To ensure clarity regarding the scope of our study, we explicitly state in the introduction that categorizing these distinct morphologies falls outside the framework of our analysis, which instead focuses on the general properties of the PV intrusions.

2. Lines 74 – 75: The ERA 5 data should be described properly please, including the pressure levels used in the study, the variables etc. A very critical issue: this is a SH study yet the authors consider the pre-satellite era, please justify this and consider the pitfalls of doing this (Tennent 2004, GRL). This needs to be properly justified.

We have expanded the description of the ERA5 data.
Regarding the critical issue of using pre-satellite era data in the southern hemisphere. Our initial sensitivity tests between the 1959-2021 and 1979-2021 periods yielded similar patterns and results. However, we fully acknowledge the pitfalls highlighted in Tennant (2004). These limitations are also corroborated by recent evaluations of the

ERA5 dataset itself, which show that southern hemisphere synoptic descriptions prior to 1979 remain mainly statistical due to the scarcity of conventional observations during that period. Recognizing this issue, we have decided to revise the study to exclusively utilize data from the post-satellite era (1979–2021). We have updated the text and figures throughout the manuscript to reflect this change.

3. Lines 130 – 139: This, of course is a good way to extract the most robust signal. However, the authors should also consider what happens when the streamers occur downstream of the CBs. Surely there would be such cases. If there are none, please be explicit about the fact this was tested and none such cases were found. To illustrate the importance of this point, if a RWB is downstream and cyclonic, the vertically upward motion it would induce would be the strongest of all four categories of RWB because of the jet streak orientation that would materialise. Again, this why it is important to consider the issue of morphology before composting.

We thank the reviewer for this point and agree that downstream cyclonic RWB can induce intense localized vertical motion.
However, our specific research objective is to quantify the dynamic drivers that initiate and maintain diagonal tropical-extratropical cloud bands. The established dynamical framework for these cloud bands (e.g., Knippertz, 2007; Kiladis, 1998) demonstrates that they are fundamentally triggered and steered by an upstream upper-level trough or stratospheric PV intrusion.
This study takes the perspective of cloud bands, and links their occurrence to RWB represented by stratospheric PV structures. While we cannot exclude that in some cloud band cases PV structures may occur downstream of cloud bands, the composite figures provide compelling evidence that stratospheric PV structures occur with increased frequencies upstream/poleward of cloud bands and with reduced frequencies downstream/equatorward of cloud bands (e.g., Figs 3, 4 and 6 of the revised manuscript). For this reason, our collocation methodology is deliberately designed to connect cloud bands with upstream PV structures consistent with the expected upstream midlatitude forcing. To make this methodological choice clearer to the reader, we have added two sentences of clarification to Section 2.3.1.

4. Lines 178 – 180: These cases are very carefully chosen, again is there no possible of downstream occurrence (as questioned above).

We wish to reassure the reviewer that these cases were not selected to artificially isolate upstream PV structures or ignore downstream complexity. Rather, to avoid selection bias regarding the dynamic forcing, our case study selection was strictly cloud band centric.
The cases were chosen based on the characteristics and impacts of the cloud bands themselves, independent of their specific atmospheric PV configurations:

- The January 2017 event was selected due to its socio-economic impacts.

- The February 2021 event was selected specifically because it illustrates a splitting cloud band, which is explicitly not a 'clean', idealized single-trough scenario; it demonstrates multiple co-occurring PV intrusions interacting with the system.

5. Lines 187 – 188: This statement needs clarification (ie. Extratropical frequencies vs tropical and isentropic surfaces on which RWB are identified on. I thinks this is a bit misleading)

We thank the reviewer for pointing this out.
To ensure an adequate identification of RWB across latitudes and seasons, we consider PV structures on isentropic surfaces between 275-360K with 5K intervals. Thus, our climatology of RWB is not limited by considering a single or few isentropic surfaces only but presents a solid representation of RWB occurrences in space and time. We agree though that the sentence indicated by the reviewer was not clear. Therefore, we rephrase the sentence
"This aligns with the expected behaviour of Rossby wave breaking (RWB), occurs predominantly in the extratropics and reduces towards the tropics."
as
"This aligns with the previously demonstrated climatology of RWB with highest frequency occurrences in the extratropics reducing towards lower latitudes (Portmann et al., 2021; De Vries et al., 2024)."

6. Lines 201 – 231: This section is difficult to assess because the accompanying figure is incomplete as mentioned above. When addressing these issues, please clarify how exactly RWB drives moisture fluxes, please show the low-level qV fields etc or backward trajectories could be employed

We thank the reviewer for highlighting the need for a complete structural view of these interactions. We apologize if the previous version of the figures felt incomplete. To address this and provide a comprehensive view of the vertical dynamics, we have restored the vertical-meridional cross-section panels to Figure 4.

We deliberately use vertically integrated horizontal water vapor transport (IVT) to depict the moisture transport aggregated throughout the troposphere. By definition, consistent with physics, most of the atmospheric water vapor is contained within the lower troposphere where temperatures are highest. Therefore, the IVT field mostly reflects moisture transport in the lower troposphere. As stratospheric PV structures induce a cyclonic circulation, the moisture transport is steered in a poleward and eastward direction at the downstream flank of the region where stratospheric PV structures are increased. We clarify this by revising lines 216-217 of the original manuscript
"Near the equatorward edge of the PV structures, moisture
is steered poleward along the eastern flank of the intrusion."

by
“In the region downstream of increased PV structure frequencies, moisture transport is steered in a poleward and eastward direction, consistent with the cyclonic circulation anomaly that is induced by cyclonic PV anomalies”

While backward trajectories would provide great Lagrangian insights for individual case studies, computing them for the thousands of events in our extensive climatology falls outside the scope of our Eulerian object-based framework. We believe that the IVT fields clearly illustrates how RWB drives the moisture fluxes sustaining these systems.

7. Lines 250 – 251: The authors need to establish first from previous studies that have considered the CBs in this region that they actually occur right through the year, before making this statement. We know in other areas for instance that they are summer phenomena.

We thank the reviewer for highlighting this important regional distinction. The reviewer is entirely correct that in regions such as the South Atlantic, cloud bands are predominantly a summer phenomenon. While cloud band activity in the SPCZ decreases during the austral winter, it still has some acticity. We agree that our original phrasing jumped straight into the lifespan categorization without properly establishing this baseline seasonality. To correct this, we have revised the introduction to this section 3.5, before discussing the distribution of short- and long-lived events throughout the year.

8. Lines 340 – 341: As noted above, the issue of morphology needs to be carefully considered in this study.

We appreciate the reviewer emphasizing this point. As detailed in our response to your previous comment regarding this point, we agree that morphology is an important dynamical factor. However, performing a rigorous morphological classification of the RWB events prior to compositing falls outside the core scope of our object-based lifecycle analysis, which focuses on the general presence, depth, and persistence of the PV forcing.
As noted previously, we have reviewed the manuscript and systematically removed adjectives (such as "equatorward-extending") that inadvertently implied a specific morphological classification we did not explicitly measure.