Final author comments for egusphere-2025-1949

Clear-air turbulence derived from in situ aircraft observation – a weather feature-based typology using ERA5 reanalysis

by Ming Hon Franco Lee and Michael Sprenger

23 September 2025

We are very grateful to both reviewers for their encouraging and constructive feedback and to David Schultz for the suggestion of additional relevant literature. These comments help us to further improve the current manuscript. Based on the suggestions, we will implement the following major changes in the manuscript:

- Two new subsections will be added to the Discussion section to (i) address the non-local generation mechanisms of CAT and other possible causes of turbulence; (ii) discuss the potential implications of the results to forecasting applications.
- Additional relevant studies will be referenced in the manuscripts.
- Grammatical errors will be corrected, and unclear phrases will be replaced.

This document presents the reviewers' comments in **blue** and our reply in **black**.

Reviewer 1

General comments:

This is a welcome and well-presented addition to the literature on the synoptic patterns associated with clear-air turbulence. The authors revisit this topic, which has been largely neglected for decades, with new and improved ways of identifying and analyzing both the synoptic patterns and CAT. The key results of the classification are two main synoptic modes that account for 70% of all moderate-or-greater CAT in the study, which are useful for both practical forecasting considerations and future research on CAT mechanisms. The primary, but minor, weaknesses are a relative lack of discussion of non-local generation of CAT near (and not-so-near) convection for the second mode (II), and some missing literature references that would provide a broader context for the research and the results.

Thanks a lot for your positive evaluation of the manuscript and the recognition of the relevance of this study. We are very grateful for your thoughtful comments and suggestions on additional references. We agree that the discussion of non-local generation mechanism can be extended. We hope the revised version of the manuscript will be sufficient to address this weakness.

Specific comments: (comments are numbered for easier reference)

1. Line 39: Did Hislop and Bannon specifically connect the Richardson number with CAT? Rustenbeck, in the April 1963 Monthly Weather Review, lists other researchers but indicates that there was up to that point no consensus on whether or not low Ri correlated with CAT. https://journals.ametsoc.org/view/journals/mwre/91/4/1520-0493_1963_091_0193_taorcw_2_3_co_2.xml

Thanks a lot for the question and bringing in the study of Rustenbeck (1963). We are particularly grateful for the suggestion of early publications of CAT, as they were scattered across different scientific journals, technical reports, and other weather forecasting guidelines, making the review on early results of CAT research challenging.

Concerning the connection between Richardson number (Ri) and CAT, we first clarify the conclusions of the cited papers, then we discuss a revised version of the sentence in the current manuscript to avoid confusion. Hislop (1951) concluded in his study that *"… in the turbulent regions it has been shown that the observed horizontal gradient of temperature implied the existence of a high vertical wind gradient. Also that this vertical wind gradient, when coupled with the observed temperature lapse rate satisfies the Richardson criterion for turbulence, within reasonable limits of experimental accuracy."* As the quote suggests, Ri was found to be low in these turbulent regions. In contrast, the vertical wind shear was found small in smooth air, implying a higher Ri (Hislop, 1951). However, as stated by Hislop (1951), *"Since only three cases were investigated, the result must be accepted with caution, but it is at least interesting and suggestive."*

Regarding the results from Bannon, we apologise for an error here. The publication, Bannon (1952), cited did not discuss the relationship between Ri and CAT. Rather, the relevant publication should be Bannon (1951), which estimated the values of Ri and computed the frequency of turbulence in certain ranges of Ri. Bannon (1951) concluded *"Thus it seems that bumpiness in the upper troposphere, though it occurs with all values of Ri, is not related directly to Ri when Ri is greater than or equal to 10. Small values of Ri and the occurrence of bumps are definitely related as would be expected."*

Therefore, both Hislop and Bannon related CAT occurrence to low Ri, with Bannon (1951) more explicitly showing the enhanced turbulence frequency in low Ri conditions. However, as Rustenbeck (1963) suggested, there were other studies showing that low Ri does not correlate well with CAT occurrence. To address this, we will modify the sentence in line 39, correcting the reference to (e.g. Hislop 1951, Bannon 1951), and include the reference to Rustenbeck (1963) to reflect the disagreement among studies at the time.

2. Lines 80-89: Is the RWB referred to specifically cyclonic RWB, i.e. LC2, or is the RWB of both types? LC1 events (anticyclonic RWB) can concentrate low PV, so are they indicative of WCB events? What is the reasoning or the literature trail recommending identifying WCBs as the defining synoptic feature, over a pure RWB interpretation?

Thank you for the questions. In our study, the use of PV streamers as a proxy for RWB does not distinguish between cyclonic and anticyclonic wave breaking (LC2/LC1). Therefore, RWB referred to includes both types. While RWB and WCB sometimes co-occur and interact, we consider the two weather features separately, as they possess notable differences. RWB occurs in dry dynamics and the proposal of LC1, LC2 RWB originates from dry, idealised baroclinic wave simulations (Thorncroft et al., 1993). In contrast, WCBs are significantly influenced by moist processes, with condensation leading to cross-isentropic ascent and possibly with convection embedded (Oertel et al., 2020). Therefore, WCBs are intrinsically more closely tied with moist dynamics compared to RWB. This significant linkage to moist processes is also the major reason to consider the relationship between WCB and CAT, since recent case studies repeatedly demonstrate the importance of moist convection in triggering CAT (lines 74-77, and references there). In particular, Trier et al. (2020) found the convective outflow from an extratropical cyclone led to a large number of turbulence reports. Therefore, we decided to examine WCB as an individual weather feature, as it pinpoints the role of moist processes in CAT occurrence (lines 87-89).

This choice is also pragmatically justified by the results obtained in this study. First, a relatively high occurrence frequency of WCB ascent is found near the turbulence events (Fig. 4b). Moreover, among the 3493 turbulence events concurrent with RWB or WCB ascent (i.e. the sum of type I and II events), only 546 (15.6%) are concurrent with both (lines 244-247). Therefore, using LC1 RWB events to indicate WCB events would leave out a significant portion of WCB events (963 type II events, 63.8%).

3. Lines 91-93: Rather than citing later, post-PIREPs-use literature, a better source for the limitations of PIREPs is Schwartz, 1996 Weather and Forecasting, https://journals.ametsoc.org/view/journals/wefo/11/3/1520-0434_1996_011_0372_tquopi_2_0_co_2.xml

Thank you for this suggestion. We agree and we will include this reference in the revised manuscript.

4. Lines 169-170 and 276-279: the definition of "clear" used permits quite moist conditions. While this is consistent with how CAT is defined operationally, it likely retains quite a bit of convectively generated CAT that propagates beyond the cloud layers, both vertically and also horizontally. The research on near(and not-so-near)-cloud turbulence, e.g. Lane et al. 2011 Bulletin of the AMS (https://journals.ametsoc.org/view/journals/bams/93/4/bams-d-11-00062.1.xml) and follow-ons, should be cited and incorporated.

Thanks for the suggestion. We agree that some of the events are probably near-cloud turbulence and we will incorporate a reference to Lane et al. (2012) to inform readers the possible underlying causes of type II events in the paragraph (lines 274-285). This will be discussed again in an additional section in the discussion to address the non-local generation mechanisms (see reply to comment 6).

5. Line 274 or so: In Figure 6b, the notable extent of the deformation pattern to the north for type II cases could imply that these occur on the equatorward flank of the jet stream, as would be expected for low-to-negative PV situations.

This is a plausible explanation to connect the patterns in Fig. 6d and 7d. We will incorporate this in one or two sentences after line 295, to briefly discuss this implication. Thanks for the comment.

6. Lines 276-279 and 294-295: More generally, the triggering of CAT locally via non-local means, e.g. gravity wave propagation, is not discussed other than in one paragraph early on (lines 70-78). CAT is likely to be, in some and perhaps many instances, the product of dynamical adjustment processes taking place on the synoptic-or-smaller scales distant from the CAT event itself, ultimately ending up as KHI locally. While this study focuses on synoptic patterns, the authors should incorporate more caveats indicating that what happens locally may have non-local triggers. This is definitely true for type I cases, but also possible for type II cases; see Thompson and Schultz 2021 Geophysical Research Letters (10.1029/2021GL092649) for a modeling study of inertial instability in the jet stream and its connections to both gravity wave generation and CAT.

Thanks a lot for this important reminder and we agree that the non-local generation mechanism is not discussed in detail in this manuscript. We do not discuss this aspect in the result section as the focus is on synoptic weather features or systems. However, we agree with you that readers should be informed about the non-local triggers and we will include a subsection in the Discussion, which address this issue, for both type I and type II events. We also appreciate the suggestion of the additional reference from Thompson and Schultz (2021), which is relevant to this discussion.

7. Lines 307-319: With regard to the Ellrod indices, it would be interesting to see how TI3, which includes divergence *tendency* (Ellrod and Knox 2010 Weather and Forecasting, https://journals.ametsoc.org/view/journals/wefo/25/2/2009waf2222290_1.xml; see Lee et al. 2023 Geophysical Research Letters for a recent application, at https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2022JD037679), would perform. The inclusion of divergency tendency was intended to capture adjustment processes characterized by more divergent flows. Would TI3 capture type II events even better? This is not required, and could be "future work."

We also find this being an interesting question and we follow Lee et al. (2023) to compute a divergence tendency term by comparing the divergence at the event time with that at 6 hours earlier. We also find the scaling factor of 0.01 for the divergence tendency term reasonable to match the magnitude of the original Ellrod turbulence index (TI1). The resulting composite is shown in Fig. R1.

With the divergence tendency term is added, the averaged value of TI3 for type II events is increased compared to that in Fig. 11 for TI2. However, the value of TI3 for type II events is still comparatively lower than that of type I events. As already stated in the reviewer comment, we also consider this analysis on TI3 as additional. Therefore, we will not include it in the revised manuscript, but have it documented here for reference.

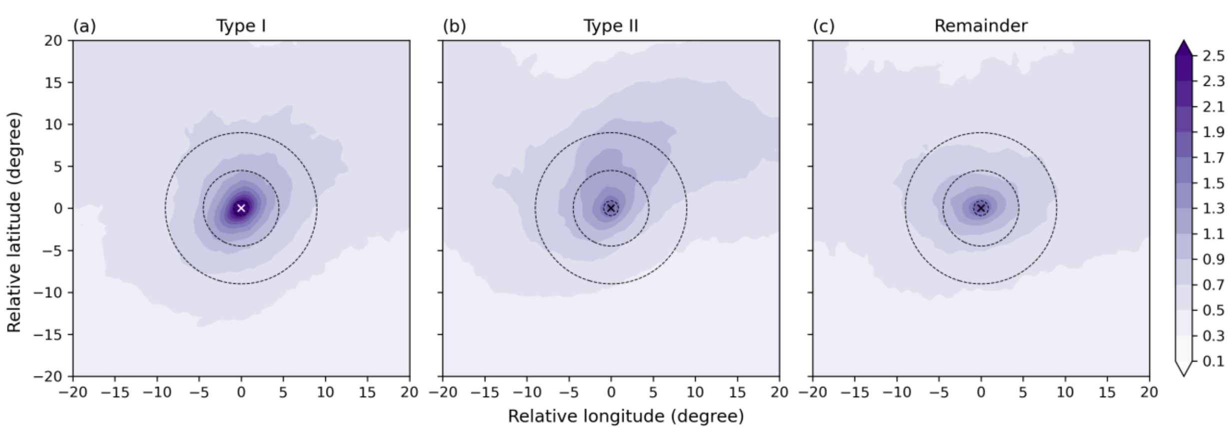

Fig. R1: Composite of Turbulence index 3 (TI3) on the isobaric surface of the MoG turbulence events for **(a)** type I, **(b)** type II, and **(c)** remainder events. The event location is denoted by the cross at the centre and the dashed circles indicate 100, 500, and 1000 km distance from it.

8. Lines 375-377, Figure 12d: This is arguably an important result, because it clearly portrays the unusually high frequency of moderate-or-greater CAT (or at least turbulence) in negative-PV regions. Furthermore, this result is independent of the classification scheme used, although it does line up well with WCBs. The only similar plot (using absolute vorticity) I recall is from a relatively obscure source, using much less data: Sparks, W. R., S. G. Cornford, and J. K. Gibson, 1977: Bumpiness in clear air and its relation to some synoptic-scale indices. Geophysical Memoirs 121, 53 pp.

We appreciate your recognition of the importance of this result and this encouraging comment!

9. Lines 405 and Figure 13: I'm surprised that there aren't more type II events over the north Atlantic, for example in ridges/with WCBs. Is this related to undersampling and/or the threshold for the definition of an "event" (lines 537-546)? Also, what is the spatial distribution of the unclassified 30%?

Thanks for this question and we think this is related to undersampling. Fig. 13 is based on the “grid point EDR” data (lines 174-180) and for Fig. 13b, only MoG points with negative PV are extracted, which correspond to around 2 or 3% of the total MoG points as estimated from Fig. 12d. Furthermore, we do not show horizontal grid points with less than 5 such data points over the entire period (Jan 2019- Sep 2022). This requirement filters out most horizontal grid points over the North Atlantic, as aircraft have a larger freedom of flight tracks, leading to a lower “density” of reports. In comparison, there are grid points in the East China Sea which satisfy this requirement, probably due to aircraft constantly flying with the same route.

Regarding the North Atlantic in Fig. 13b, the definition of an “event” is not an issue as “grid point EDR” uses all reports without clustering. However, the low “density” of reports over the North Atlantic may cause CAT in this region more difficult to satisfy the minimum 10 reports of MoG intensity requirement (lines 544-547), and hence less likely to be identified as an event. This is reflected in the much higher number of events over the contiguous US (Fig. 5b). Consequently, undersampling is the primary reason for the apparent lack of WCB related turbulence over the North Atlantic.

The spatial distribution of the “remainder” events is displayed in Fig. R2 (corresponding to Fig. 5). The distribution of the “remainder” events in the extratropics roughly follows the overall distribution of events as shown in Fig. 2. Also, since the categorisation is based on weather features commonly found in the extratropics, most events in the tropics are in this “remainder” category. We will consider including Fig. R2 in the supplement.

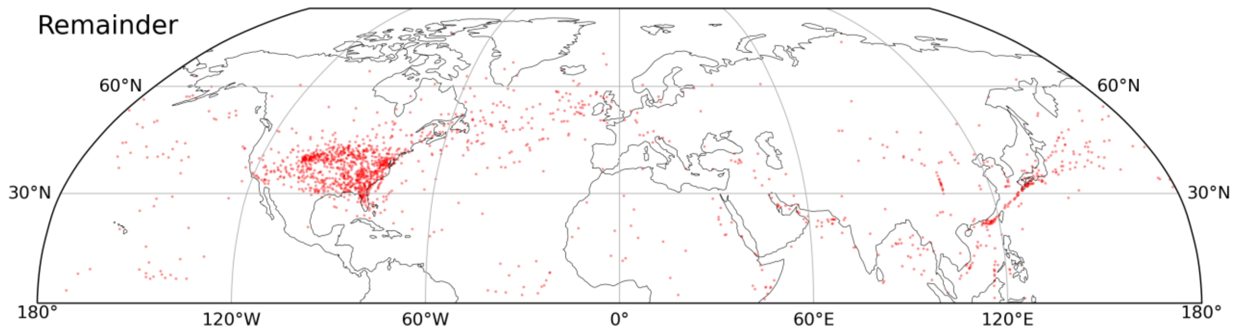

Fig. R2: Geographical distribution of “Remainder” events. The location of an event is indicated by a red dot.

10. Lines 485-489: Here, again, I encourage the authors to be careful to acknowledge that not all CAT is going to have an in situ trigger, but may be related to dynamical adjustment processes that are not local. In particular, there are likely to be type II events that turn out to be "near"-cloud turbulence events rather than CAT.

We appreciate this reminder on the non-local generation of CAT. As mentioned in the replies to comments 4 and 6, we will include a subsection in the Discussion to address this aspect. A corresponding amendment to the Conclusion (lines 485-489) will be made as well.

11. Lines 490-495: an update of the classic synoptic patterns for CAT (e.g., Fig. 12.1 in Sharman and Lane's Aviation Turbulence book), based on 21st-century data and tools, would be a very useful result from this line of research.

Thanks a lot for this suggestion. We will discuss this aspect together with the implications for forecasting applications. (Please refer to the general comment 2 by Reviewer 2)

12. Line 563: Some near-cloud turbulence could be screened out by requiring the aircraft and reanalysis temperatures much less (not just less) than the cloud-top temperature. Was this attempted?

Thanks for this suggestion as well. The major aim of this screening was to exclude events that are within convective clouds, which are usually indicated by high cloud tops. These in-cloud events are the primary target, while near-cloud events are still allowed (lines 169-

171). Therefore, we implement this simple “less than” requirement to distinguish between “clear-air” and “in-cloud” conditions for each report.

However, given this suggestion, we are also interested in whether this “much less than” requirement will remove some of the events that are likely near-cloud turbulence. In this test, we modified the requirement of clear-air condition of a single report to having its aircraft and reanalysis temperatures less than cloud top temperature by at least 1 K, 2 K, and 5 K. First, tightening the requirement definitely reduces the number of clear-air reports and increases the number of in-cloud reports. Therefore, less events will be classified into “predominantly clear-air”. The resultant numbers of predominantly clear-air events are tabulated in Table R1 for different temperature differences (with 0 K being the same as the one employed in the manuscript). The number of events reduces when the temperature difference required increases, as expected. The distributions in type I, type II, and remainder events are also given in Table R1. The percentage of type II events decreases when the requirement is tightened, and this is mostly compensated for by an increase in the percentage of type I events. It again demonstrates that type II events are more likely to be near-cloud events while type I events are mostly clear-air.

Table R1: The weather feature-based categorisation of predominantly clear-air events with different temperature difference required for the definition of clear-air reports (see text, Reviewer 1, comment 12). The percentages in brackets in each row correspond to the total number listed in the second column of that row.

Temperature difference required (K)	No. of predominantly clear-air events	No. of type I events	No. of type II event	No. of Remainder events
0	4,880	1,984 (40.7%)	1,509 (30.9%)	1,387 (28.4%)
1	4,772	1,972 (41.3%)	1,434 (30.1%)	1,366 (28.6%)
2	4,643	1,960 (42.2%)	1,343 (28.9%)	1,340 (28.9%)
5	4,152	1,894 (45.6%)	1,024 (24.7%)	1,234 (29.7%)

In addition, we should also check if this significantly changes the cloud ice water content composites (Fig. 8). The same figure as Fig. 8, but only with predominantly clear-air events satisfying the 5 K temperature difference requirement is shown in Fig. R3. When compared to Fig. 8, the cloud ice water content composites are similar for type I and remainder events. A slight decrease can be observed at the event locations of type II events, but the maximum nearby is still of comparable magnitude. Overall, the composites remain qualitatively similar and we, therefore, conclude that the simple “less than” requirement already fulfils its aim to remove “in-cloud” events.

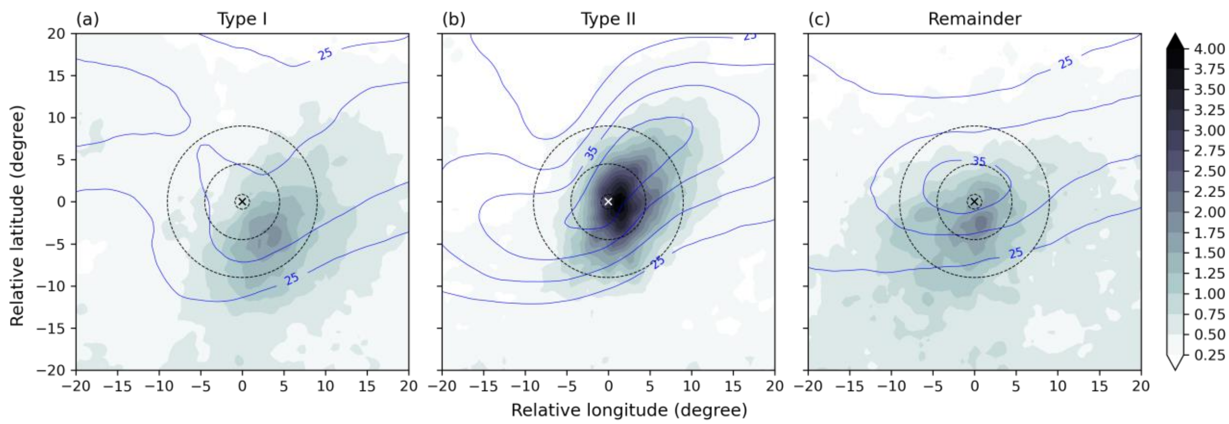

Fig. R3: As in Fig. 8, but for the predominantly clear-air events satisfying the 5 K temperature difference requirement.

<u>Reviewer 2</u>

Overview:

This paper uses four years of EDR measurements from commercial airliners and ERA5 reanalysis data to construct a climatology of different synoptic patterns influencing clear-air turbulence (CAT) in the vicinity of the jet stream. Though the EDR data are mostly confined to midlatitude continental regions, the analysis provides a novel and much-needed preliminary climatology (mostly over the CONUS) showing how different synoptic weather types including large-scale Rossby Wave Breaking (RWB) and warm-conveyor belts (WCB) within midlatitude cyclones are conducive to CAT by different mechanisms. Some interesting results are presented on how turbulence over the western U.S. is more commonly associated with RWB and near the eastern U.S. is most likely to be influenced by diabatic heating within cloudy WCBs with CAT occurring either above or horizontally displaced from clouds. The paper is well written, and I recommend it be published with minor revisions. I have only two general comments that are meant mainly to encourage a little more discussion of potentially relevant topics or applications of the work. The remainder of the comments are minor, and concern suggested changes to English grammar or request some minor clarifications.

Thank you for your positive assessment and for affirming the need for this line of research. We appreciate your constructive and detailed comments a lot. We will incorporate the suggested clarifications and changes to improve our manuscript.

General Comments:

1. The in situ commercial aviation EDR data used in this study was obtained between 20000 and 60000 ft MSL and the measurements are understandably grouped together to perform the climatological analysis. Can you comment on the percentage of these EDR measurements are from the lower stratosphere versus the upper troposphere? Are the more favorable of the two locations (i.e., above or below the tropopause) different for the RWB and WCB environments?

Thank you for raising this aspect and giving us a chance to provide further details. In our analysis, we use the 2-PVU isosurface to define the dynamical tropopause. Adhering to this definition, Fig. 12b provides a first estimate for the proportion of EDR measurements in the upper troposphere or the lower stratosphere. Note that the data used in Fig. 12 is the "grid point EDR" (lines 174-179), which is not exactly the raw EDR reports. After "gridding" the reports, there are 43,608,284 grid points with assigned EDR values in clear-air. 68.6% of them has PV < 2 PVU while 31.4% of them has PV > 2 PVU (lines 368-370). It hence indicates an approximate 70-30 division of EDR measurements in upper troposphere-lower stratosphere region.

The second part of the question concerns the distribution particularly in RWB and WCB environments. To provide an estimate, we do a similar computation for the RWB points (12,996,555 in total) and the WCB points (2,191,826 in total). For the RWB points, 42.7% of them has PV < 2 PVU (upper troposphere) and 57.3% has PV > 2 PVU (lower stratosphere). This is consistent with the intuition that RWB, especially with PV streamers as a proxy, indicates intrusion of stratospheric air to lower altitudes and latitudes, which results in a higher percentage of lower stratospheric measurements. For WCB points, 76.0% is in the upper troposphere while 24.0% is in the lower stratosphere. The distribution is similar to the overall distribution, with a slight enhancement in the upper troposphere. This is again consistent with WCB, which usually feeds into the downstream ridge that has an elevated tropopause.

In case only the turbulent measurements are of interest, we compute the same set of statistics for MoG points, RWB-MoG points, and WCB-MoG points and the numbers are tabulated in Table R2. Also, a study dedicated to the relative locations of aircraft measurements to the lapse rate tropopause can be found in Kaluza et al. (2022).

While the extra numbers are interesting to look at, we would like to keep this discussion only in this reply. We prefer not to include this in the manuscript since it may distract from the main focus of the manuscript on the linkages of CAT to weather features. Also, we would consider a more robust analysis, like the one performed by Kaluza et al. (2022), is needed to address this question comprehensively. This probably involves the calculation of tropopause heights, which exceeds the scope of the current study.

Table R2: The distribution of grid points in upper troposphere (PV < 2 PVU) and lower stratosphere (PV > 2 PVU). The percentages in brackets in each row correspond to the total number listed in the second column of that row.

	Total No.	No. with PV < 2 PVU (upper troposphere)	No. with PV > 2 PVU (lower stratosphere)
Grid point EDR	43,608,284	29,917,967 (68.6%)	13,690,317 (31.4%)
RWB points	12,996,555	5,546,177 (42.7%)	7,450,378 (57.3%)
WCB points	2,191,826	1,664,698 (76.0%)	527,128 (24.0%)
MoG points	409,471	284,417 (69.5%)	125,054 (30.5%)
RWB-MoG points	131,658	59,835 (45.4%)	71,823 (54.6%)
WCB-MoG points	72,469	59,052 (81.5%)	13,417 (18.5%)

2. There are interesting results in this paper, but can you provide some summary discussion on how the relationship of these two different synoptic environments and CAT might be used in forecasting applications (e.g., route planning by airlines)?

We are very thankful for this suggestion, which will bring the perspective of aviation weather forecasting into the Discussion section. We will add a short subsection in the Discussion to address this aspect. First, we have to admit that a direct application of the results (the relationship between the two synoptic environments and CAT) to the current automated CAT forecasting strategy is difficult. Rather, we think the results are useful to provide further understanding and interpretation of the automated forecasts for forecasters and pilots. As suggested by Reviewer 1 (comment 11), the schematics showing typical synoptic patterns associated with CAT occurrence can be updated with modern data and tools. Continuation of this line of research may thus provide renewed schematics for easy communication to pilots or forecasters, facilitating the transfer of knowledge from academia to forecasting offices. The results also provide the characteristic environmental conditions and possible generation mechanisms of CAT under these synoptic situations, allowing forecasters to better interpret the signals from automated CAT forecast products.

Another potential contribution relates to the higher predictability of synoptic patterns, compared to that of CAT in current forecasting systems. While CAT forecasting remains in the short-range, usually with a lead time of less than a day, synoptic dynamics can be reasonably predicted with a lead time of several days. If these relationships can be incorporated into CAT forecasting strategy, a longer lead time may allow airlines to respond earlier (as suggested, even in route planning stage). However, much more research effort has to be undertaken before these benefits can be realised.

Minor Comments:

1. Line 90. Therefore, a revisit to the topic ...→ Therefore, revisiting the topic ...

Thanks, we will implement this change.

2. Line 120 and elsewhere in the paper. In the current usage "as a proxy to RWB" should be changed to "as a proxy for RWB".

Thanks, we will implement this change.

3. Line 142. on the opposite → in contrast

Thanks, we will implement this change.

4. Line 166. has to be taken care of → must be realized

Thanks, we will implement this change.

6. Lines 272-273. This final sentence of the paragraph seems redundant (with the previous sentence) and can be removed.

Thanks for the suggestion. However, we think this sentence serves as a concluding sentence for this paragraph. We will shorten it to make it more concise.

7. Lines 276-277. Consider simplifying "is more isolated from the background, which in general increases equatorward" to "is enhanced from background values, which increase equatorward".

Thanks for the suggestion, we will implement this change.

8. Line 283-284. There are few (if any) studies that directly link turbulence to inertial instability, so I think this statement needs to be better qualified. The negative PV or vertical component of absolute vorticity can be widespread in certain synoptic patterns, but the turbulence is typically much more localized. It's not clear what is happening in these cases, but perhaps the inertial instability results in horizontal accelerations that modify the environment in ways that can support more localized KH or static instability leading to turbulence in such events? There also other possible influences on turbulence including gravity wave emission in inertially unstable flows.

Thank you for this thoughtful comment and we would like to address this suggestion in two parts. Firstly, we would like to clarify that the sentence referred to, "Hence, the majority would be the result of the presence of inertial or symmetric instability, ..." (lines 283-284) was meant to explain the origins of the negative PV found. We would like to state that the negative PV is not mainly due to static instability (lines 281-283), but negative absolute vorticity (inertial instability) or symmetric instability. The second part of the sentence, "… which has been suggested to be a cause of CAT in previous studies (e.g. Knox, 1997).", was then referring to a possible connection between negative PV and CAT via inertial or symmetric instability, as has been suggested by Knox (1997), and more recently by Thompson and Schultz (2021) (as suggested by Reviewer 1). To make our meaning precise and clear, we will restructure the sentence into the following (lines 283-285):

Hence, most negative PV values would be the result of the presence of inertial or symmetric instability. Since inertial or symmetric instability has been suggested to be a possible cause of CAT in previous studies (e.g. Knox, 1997), WCB ascent may potentially favour the occurrence of CAT by creating an environment conducive to this instability.

Secondly, there is also the concern regarding other possibilities of turbulence generation, including localised static instability or KHI. We agree that there are other possible linkages between negative PV and CAT and the presence of localised static instability or KHI has been pointed to in the next paragraph (lines 286-295), though it is not very explicit. As shown in Fig. 8, the wind speed is enhanced near type II events, which would possibly increase vertical wind shear. We drew a link to the case study by Trier et al. (2020), which showed that localised sheared layer and KHI created by the convective outflow of an extratropical cyclone is the most probable cause of a group of reported turbulence. Within these sheared layers, static instability may locally be found. Later in the paragraph, we also discuss that the presence of negative PV is consistent with an enhanced upper-level flow. Therefore, it may be an alternative or another connection between negative PV and the occurrence of CAT. We would consider changing some parts in lines 287-291 to emphasise this. Furthermore, the possibility of having gravity wave emission in inertially unstable flows will be included in the new subsection in the Discussion, as in the reply to Reviewer 1, point 6. We hope these changes will allow the readers to recognise the different possibilities of the underlying cause of turbulence.

9. Line 289. the shear layers → the vertical shear layers

Thanks, we will implement this change.

10. Line 298. It implies → This implies

Thanks, we will implement this change.

11. Line 298. particular type → particular synoptic type

Thanks, we will implement this change.

12. Lines 311-312. multiplying with vertical wind shear → multiplying this difference by the vertical wind shear

Thanks, we will implement this change.

13. Lines 316-318. This is a long and awkward sentence. Please split into 2 sentences with the 2nd sentence starting immediately after the Sharman et al. 2006 reference. Also, on line 317 please change “but negative PV …” to “but diagnosing negative PV”.

Thanks, we will implement this change.

14. Line 323. Since it is the vertical heating profiles associated with deep convection that is most likely responsible for generating the negative PV in these events, consider changing “… near-cloud events” to “… induced by deep convection”.

Thanks for the suggestion. We prefer to keep “near-cloud events” as it is restating the result discussed earlier (line 278). The connection to deep convection will be made in line 325 when the next comment (comment 15) is addressed.

15. Line 325. To be more specific please change “outflow associated with the WCB” to “outflow associated with organized deep convection occurring within the WCB”.

Thanks for the suggestion. We will change the phrase instead to “outflow associated with embedded deep convection in WCB” to keep the terminology used in the manuscript consistent (see line 292).

16. Line 327. but at the same time less stable → but less statically stable

Thanks, we will implement this change.

17. Lines 327-328. The different properties of the two types of events …→ These differences in the environments of the two types of events …

Thanks, we will implement this change.

18. Line 385. It is consistent with → This possibility is consistent with

Thanks, we will implement this change.

19. Line 402. Weak large-scale ascent present in the WCB does not directly influence turbulence but instead provides a favorable environment for organized deep convection (e.g., squall lines) that would likely be more directly responsible for negative PV and turbulence, through the mesoscale modification of the WCB environment. This idea is discussed in the conclusions but probably should be clarified here as well.

Thanks for the suggestion. The terminology "WCB ascent" is used mainly to be consistent with the weather feature mask naming (lines 137-138). We will insert a bracket "(possibly with embedded convection)" after WCB ascent in line 402. The choice is to make it consistent with the main conclusion in lines 487-489, but keep it as a postulate, as we do not identify convection within WCB directly in this analysis.

20. Line 488. You mention symmetric and inertial instabilities, but it seems that these processes could also result in localized static instability and that could also play a role in the onset of turbulence.

Thanks a lot for this careful assessment of the conclusion. As discussed in the reply to comment 8, we stated explicitly symmetric and inertial instabilities as we checked that static instability only accounts for less than 3% of the negative PV at the event location (lines 281-284). Therefore, negative PV indicates either inertial or symmetric instability. This refers to the grid box average (at the ERA5 reanalysis resolution), and we cannot eliminate the possibility of localised static instability. However, we cannot prove or disprove this statement with the ERA5 reanalysis data, so we keep it as it is here. We will nevertheless include this perspective in the new subsection in the Discussion as mentioned.

David Schultz

I thank the authors for an interesting analysis of the synoptic conditions under which clear-air turbulence (CAT) occurs.

The authors present some of the early research on CAT from the 1950s, which is admirable. This is a nice summary of the older literature. My concern is that the literature review is incomplete. Some papers have examined the synoptic conditions under which inertial instability, symmetric instability, and/or negative PV occurs in the upper-troposphere, but are not cited. These include the following articles.

We are very grateful for your comment and for bringing in your expertise in inertial instability, symmetric instability, and other related topics. We appreciate the suggestion of an extensive collection of literature. As the focus of this study is on CAT and the connection to negative PV is mainly via WCBs, we will include the references with the highest relevance in the revised version of the manuscripts.

Chen, T., M. K. Yau, and D. J. Kirshbaum, 2018: Assessment of Conditional Symmetric Instability from Global Reanalysis Data. *J. Atmos. Sci.*, **75**, 2425–2443, https://doi.org/10.1175/JAS-D-17-0221.1.

Ciesielski, P. E., D. E. Stevens, R. H. Johnson, and K. R. Dean,1989: Observational evidence for asymmetric inertial instability. J. Atmos. Sci., 46, 817–831, https://doi.org/10.1175/1520-0469(1989)046,0817:OEFAII.2.0.CO;2.

Harvey, B., Methven, J., Sanchez, C., and Schäfler, A.: Diabatic generation of negative potential vorticity and its impact on the North Atlantic jet stream, Q. J. Roy. Meteor. Soc., 146, 1477–1497, 2020.

Hu, B., P. Hui, J. Ding, and J. Tang, 2023: Clear-Air Turbulence (CAT) Encounters on 13 November 2019 over Central and Eastern China: Numerical Simulation and Generation Mechanism. *Wea. Forecasting*, **38**, 1643–1660, https://doi.org/10.1175/WAF-D-23-0015.1.

Lojko, A., Winters, A. C., Oertel, A., Jablonowski, C., and Payne, A. E.: An ERA5 climatology of synoptic-scale negative potential vorticity–jet interactions over the western North Atlantic, Weather Clim. Dynam., 6, 387–411, https://doi.org/10.5194/wcd-6-387-2025, 2025.

Schumacher, R. S., and D. M. Schultz, 2001: Inertial instability:Climatology and possible relationship to severe weather predictability. Preprints, Ninth Conf. on Mesoscale Processes, Fort Lauderdale, FL, Amer. Meteor. Soc., 372–375.

Thompson, C. F., D. M. Schultz, and G. Vaughan, 2018: A global climatology of tropospheric inertial instability. *J. Atmos. Sci.,* **75,** 805–825, https://doi.org/10.1175/JAS-D-17-0062.1.

Thompson, C. F., and D. M. Schultz, 2021: The release of inertial instability near an idealized zonal jet. *Geophys. Res. Lett.,* **48,** e2021GL092649, https://doi.org/10.1029/2021GL092649.

Yang, Rui, Haiwen Liu, Kenan Li, and Shuai Yuan. 2024. "A Numerical Study of Clear-Air Turbulence over North China on 6 June 2017" Atmosphere 15, no. 4: 407. https://doi.org/10.3390/atmos15040407

There may be others that the authors should pursue, as well.

Thanks a lot for this list of suggested literature. First, as already suggested by Reviewer 1 (point 6), Thompson and Schultz (2021) will be included and discussed in the new subsection addressing non-local triggers of CAT. Also, we find Harvey et al. (2020) and Lojko et al. (2025) relevant to our discussion of the interaction of negative PV and upper-level flow in Sect. 3.2 (lines 286-295). Thompson et al. (2018) may provide a better justification to lines 275-277, as a general increase in the frequency of inertial instability is found towards the tropics. A reference to Schultz and Schumacher (1999) will be included in the introduction for the linkage between inertial and symmetric instabilities, as well as their relationships with negative PV. On the other hand, the two case studies on CAT by Hu et al. (2023) and Yang et al. (2024) are interesting simulation studies of CAT. However, they do not link CAT with inertial or symmetric instability. Therefore, we will not include them in the discussion.

References

(Please refer to the comment by David Schultz as well for a complete list)

Bannon, J. K.: Meteorological aspects of turbulence affecting aircraft at high altitude, Professional Notes No. 104, Meteorological Office, https://digital.nmla.metoffice.gov.uk/IO_9d123c55-1919-4ebd-8515-ba2f0cc13dab/, 1951.

Bannon, J. K.: Weather systems associated with some occasions of severe turbulence at high altitude, Meteor. Mag, 81, 97–101, 1952.

Hislop, G. S.: Clear air turbulence over Europe, J. R. Aeronaut. Soc., 55, 185–225, https://doi.org/10.1017/S0001924000132713, 1951.

Kaluza, T., Kunkel, D., and Hoor, P.: Analysis of turbulence reports and ERA5 turbulence diagnostics in a tropopause-based vertical framework. Geophys. Res. Lett., 49, e2022GL100036, https://doi.org/10.1029/2022GL100036, 2022.

Knox, J. A.: Possible mechanisms of clear-air turbulence in strongly anticyclonic flows, Mon. Weather Rev., 125, 1251–1259, https://doi.org/10.1175/1520-0493(1997)125<1251:PMOCAT>2.0.CO;2, 1997.

Lane, T. P., Sharman, R. D., Trier, S. B., Fovell, R. G., and Williams, J. K.: Recent advances in the understanding of near-cloud turbulence. Bull. Amer. Meteorol. Soc., 93, 499–515, https://doi.org/10.1175/BAMS-D-11-00062.1, 2012.

Lee, J. H., Kim, J. H., Sharman, R. D., Kim, J., and Son, S. W.: Climatology of clear-air turbulence in upper troposphere and lower stratosphere in the Northern Hemisphere using ERA5 reanalysis data, Geophys. Res.-Atmos., 128, e2022JD037 679, https://doi.org/10.1029/2022JD037679, 2023.

Oertel, A., Boettcher, M., Joos, H., Sprenger, M., and Wernli, H.: Potential vorticity structure of embedded convection in a warm conveyor belt and its relevance for large-scale dynamics, Weather Clim. Dynam., 1, 127–153, https://doi.org/10.5194/wcd-1-127-2020, 2020.

Rustenbeck, J. D.: The association of Richardson's criterion with high level turbulence, Mon. Weather Rev., 91, 193–198. https://doi.org/10.1175/1520-0493(1963)091<0193:TAORCW>2.3.CO;2, 1963.

Schultz, D. M., and Schumacher, P. N.: The use and misuse of conditional symmetric instability. Mon. Weather Rev., 127, 2709–2732, https://doi.org/10.1175/1520-0493(1999)127<2709:TUAMOC>2.0.CO;2, 1999.

Thorncroft, C. D., Hoskins, B. J., and McIntyre, M. E.: Two paradigms of baroclinic-wave life-cycle behaviour, Quart. J. Roy. Meteorol. Soc., 119, 17-55, https://doi.org/10.1002/qj.49711950903, 1993.

Trier, S. B., Sharman, R. D., Muñoz-Esparza, D., and Lane, T. P.: Environment and mechanisms of severe turbulence in a midlatitude cyclone, J. Atmos. Sci., 77, 3869–3889, https://doi.org/10.1175/JAS-D-20-0095.1, 2020.