This study examines the relation of surface cyclones and anticyclones and upper-tropospheric Rossby wave breaking. To do so, identification and tracking algorithms are applied on these systems and their relation examined using composites. A main result is the description of the (local) arrangement of anomalies in the vicinity of wave breaking that constitute latitudinal jet shifts in a larger-scale (e.g., zonal average) sense.
Overall, the manuscript is well prepared and straight forward to read. I have only minor comments regarding the presented analysis. My main reservation with this manuscript is that it did not become sufficiently clear to me how this work relates to previous work and the motivation of the study and the novelty of the insight remained somewhat unclear to me. After clarifying revisions, this manuscript will be well suited for publication in WCD.

Main comment:

Relation to previous work: Motivation and novelty of insight
A large body of literature exists on Rossby wave breaking (RWB) as part of a baroclinic life cycle (as noted by the authors in the introduction) and on the role of RWB in modifying the larger-scale jet pattern. I understand that bringing together tracked cyclones and identified RWB events has not been done in the way that it is done in this study. In this sense, there is an obvious novelty to the study. But scientifically, what are the open questions that are being addressed? In L114, the authors write: “Apart from the above mentioned studies, the intrinsic relation between the low-level cyclones, anticyclones, and RWB events has not been studied much, to the best of our knowledge. Here we highlight the fundamental relation between low-level weather systems and upper-level wave breaking events, focusing on the North Atlantic region.” What is meant with “intrinsic” in the first sentence and with “fundamental” in the second? The first sentence disregards the large body of (synoptic-scale dynamics) literature that has studied baroclinic (cyclone) life cycles, which inherently involves the evolution of the upper-level trough, and work that has studied AWB (PV streamers) as precursors to cyclones. These studies often put case-study results in the context of conceptual/ idealized models. In what sense does the average over many real-atmospheric cases (with the caveats inherent in automated identification and processing of a large number of cases) yields more “intrinsic” and “fundamental” results than idealized experiments, which attempt to retain the essence of the problem? In what sense more “intrinsic” and “fundamental” results than an aggregation of decades of case studies? For the sake of the argument, I have phrased these questions somewhat provocatively. I do not mean to say that this study would not contain novel insight or make a valuable contribution. What I mean to say is that being (much) more specific about the open questions that motivate this study and about the new insights that this study contributes would be very helpful to better appreciate the authors’ work.
A conclusion (or discussion) section usually gives a good opportunity to put results into context. The authors conclusions do not include a single reference to previous work. A quick look at Thorncroft et al. (1993) revealed that much of the described rearrangement of anomalies during RWB is consistent with that idealized study. Furthermore, the “anomalous” cyclones forming during AWB have extensively been studied previously also (more generally than in the context of Mediterranean cyclones only), and the authors results seem to be very much consistent with these studies. The reader needs more guidance to be able to identify the new insight generated by the current study, and this guidance should be given when summarizing the results, and not only when presenting specific results in the main body of the manuscript.

The authors highlight the modification of the jet pattern by rearrangement of the associated anomalies in a summarizing schematic. It should be noted that the arrangement of anomalies is a standard argument in the "blocking" community, i.e., to describe the weakening of the jet in the core of the block and the poleward/ equatorward deflection of the jet. RWB, jet structure and blocks are, of course, tightly related and the authors' composite most likely contain blocking situations also. The authors description put forth in the schematic thus seems to be a variant of a well-established argument.

Minor comments:

Anomalous life cycle

The authors introduce their definition of "anomalous" rather late in their manuscript and rather in passing. This leaves much room for confusion beforehand. Note that Simmons and Hoskins had denoted the cyclonic life cycle as "anomalous". In addition, the last paragraph in section 2 indicates that the "anomalous" cases are as frequent as other cases. I would thus suggest defining your meaning of the term at first use, and avoid using the term without definition, e.g., in the abstract.

I believe that the definition of "anomalous" is important (and non-standard) enough to move material from the supplement to the main text to introduce the definition. A more careful introduction could then include a discussion of existing knowledge of this "class" of cases (see main comment above. Note that there is a recent review paper on Mediterranean cyclones)

- Flaounas, E., Davolio, S., Raveh-Rubin, S., Pantillon, F., Miglietta, M.M., Gaertner, M.A., Hatzaki, M., Homar, V., Khodayar, S., Korres, G. and Kotroni, V., 2022. Mediterranean cyclones: Current knowledge and open questions on dynamics, prediction, climatology and impacts. *Weather and Climate Dynamics*, *3*(1), pp.173-208.

Intensity of systems in the composite analysis

The authors note in passing that the feature on which a composite is centered will be more intense than the other features in the composite, a well-known artefact of composites. Still, the authors use the intensity of composite systems in their subsequent arguments and conclusions. This is my main methodical issue with this study. A more careful analysis and discussion of the intensity of systems is needed. This could be based on benchmark composites of cyclones and anticyclones, and again I'd suggest in this case moving material from the supplement to the main text.

L59: Why do you not consider all four types of RWB? Please clarify.

L240: I am not sure I understand. What do you mean with "just signatures of the large-scale flow"? A barotropic Rossby-wave teleconnection pattern? Can you be more explicit about your possible alternative explanation/ hypothesis, such that the result that the anomalies are averages of synoptic-scale systems becomes more significant?

L255: In the average sense, a tripole emerges, but you do not analyze if the tripole does indeed co-occur often with AWB. Please substantiate the evidence or weaken your statement.

L259: I do not understand this paragraph. What is the additional information? Why switching back to physical space? Why do you not consider the pdf in the composite? Why now including

stronger systems only? Please clarify and provide more motivation for this approach. (Furthermore, note that several of your figure references have not been resolved.)

L276: From our understanding of cyclones that are associated with PV streamers (AWB) one would not expect them to propagate eastwards. I therefore do not see the concept of "hindering" appropriate for these cases.

Section 4: Does the analysis in this section use the closest (anti)cyclone as described in Sect. 2? Please clarify?

Location of upper-level anomalies poleward and equatorward of the jet.
The authors often refer to the location of anomalies relative to the jet. For a single jet, I would expect that upper-level anomalies are generated due to (synoptic-scale) deflections of the jet. In this simple case one would expect ridges to be located equatorward and troughs poleward of the (instantaneous) jet. In L309, the authors emphasize this seeming "standard" case with "importantly". How would the relative locations be reversed? Does it require a double-jet structure? Can the authors be more explicit about the significance of the relative location of the anomalies?

L366: I would always expect negative meridional velocity (northerly winds on the NH) between a ridge and trough downstream. In what sense is this velocity favored in this flow configuration?

Section 5: I have difficulties to see what we have learned from the „anomalous" life cycles. The section is mostly "show and tell". In the last paragraph of section 5.1: How would the shear enforce the trough? Evidently, we'd need some equatorward displacement of the PV contour to form the trough. How is the cyclonic shear doing this? How do we see the momentum fluxes? What, quite generally, do we learn in section 5.2? The statement in the last sentence of this section has long been realized and Mediterranean cyclones are a very active field of research.
In fact, the number of „anomalous" cases is of the same order of magnitude than other events. RWB looks quite different for the. „anomalous" cases. Can you clarify what we have for RWB? That RWB occurs with two distinct "flavors"?

e.g., **L494**, rotation of upper- and lower-level anomalies: I believe that work along the line of the following reference is relevant here.

- Rivière, G., Arbogast, P., Lapeyre, G. and Maynard, K., 2012. A potential vorticity perspective on the motion of a mid-latitude winter storm. Geophysical Research Letters, 39(12).

Second last sentence in the conclusions: There is a large body of literature that examines the relation of RWB - or more generally scale interactions - and weather regimes. What specific aspects would the authors like to "examine more deeply"? What are the open questions that their approach could address? In the current version, the statement is so general that it seems rather meaningless.

Editorial comments:

Abstract
I did not find the abstract to be informative, because I could not identify the main motivation (open question) and main results of the study (see also main comment above).

"Storm"-relative
The authors describe composites centered on cyclones and anticyclones as "storm-relative". This misnomer did create some confusion while reading. I suggest finding a more appropriate wording.

Use of supplementary material
For me, for a research article – such as this manuscript is – there is too much use of supplementary material. Above, I have suggested how the manuscript can be strengthened and clarified by including some material in the main text. Personally, as a reader, I am usually confused by supplementary material. On page 11, e.g., there is a whole paragraph spent on describing material in the supplement. If this is important, why not including it in the main text? Do I need to consult the figure in the supplement? Or not? As a reader, I personally would like the authors to make this decision for me. I am aware, however, that I may be a minority with this opinion.

L71ff: I found this sentence hard to read. It seems that what you say is that Orlanski's results are consistent with the above, but from a PV flux perspective. I suggest simplifying the presentation here for readability (of the otherwise excellently written intro).

L92ff: In general, I dislike the use of parentheses to condense the presentation. (I know, I know, it is often used …). While this may be convenient for the authors, but as a reader I find it often very cumbersome to read. The specific sentence here contains many parentheses because you define also a few acronyms. Please consider rewording for improved readability. I'd further appreciate if you'd minimize use of parentheses to condense sentences throughout the text.

L118-120: Sounds like a contradiction. Please clarify.

L233: "breaking maximum in a relatively mature and developed stage". Stage of what? Wouldn't one expect a maximum being related to a mature and developed stage?

L281ff: Are all of the statements in this paragraph supported by a figure? Please clarify.

L330: Is this shown in Fig. S6 or in the main text? I was confused, please clarify.

L505: Why merely?