Review of "A composite-based analysis of the dynamical linkage of Atmospheric Rivers, Warm Conveyor Belts, and Extratropical Cyclones" by T. Ferreira et al.

General comments:

In this manuscript, Ferreira et al. present a comprehensive, composite-based analysis of the relationships between atmospheric rivers, warm conveyor belts, and extratropical cyclones. Overall, this is a well-conceived and highly relevant study. I believe this manuscript has strong potential to make a meaningful contribution to our understanding of mid-latitude weather systems.

However, there are a few important points that should be addressed to further improve the quality and presentation of the paper. First, there is a recurring tendency to interweave new results with broad literature discussions, which dilutes the impact of the new findings. I suggest moving these interpretive points to a dedicated discussion section to keep the results section focused. Second, the authors must be careful regarding selection bias: by filtering only for rapidly intensifying cyclones and pre-filtering certain temporal analyses to only include AR-WCB-coupled cases, some conclusions regarding universal coupling are overgeneralized and require more nuanced phrasing. Lastly, there are several instances of contradictory terminology, and, while the paper is generally well-written, a close proofreading is needed to eliminate grammatical errors and typos.

I encourage the authors to address these points, which will substantially strengthen the overall clarity and impact of their work.

Specific comments:

1) References in the introduction: Please add 1–2 references to the original, seminal literature on the conveyor belt model and ARs, supplementing the review by Dacre (2020) and the studies currently cited on line 64. Furthermore, references for the Norwegian Cyclone Model should be added.

2) The exact temporal range (start and end years) of the ERA5 data used for the analysis is absent from the manuscript. Please explicitly state these years in Section 2. Furthermore, if your analysis period extends past 2016, explicitly noting this will help justify the use of ELIAS— highlighting how the machine learning approach allows you to efficiently extend the analysis into recent years without the need to compute WCB trajectories for that timeframe.

3) Section 2.2 (AR Detection via IPART): The explanation of the IPART method is scientifically sound, and I appreciate that the authors define what the image components represent (e.g., background vs. transient IVT). However, the technical workflow is somewhat difficult to follow on a first reading because the mathematical/image-processing jargon is introduced before the physical motivation is explained. Consider reordering the narrative flow of this paragraph. If you move the explanation from lines 152–155 (explaining that IPART detects ARs based on spatial "spikiness" rather than absolute magnitude thresholds) to the beginning of the section, it will provide the reader with the necessary physical context before they encounter technical terms like "greyscale reconstruction by dilation". Additionally, the distinction between "background IVT content" and "non-negative IVT distribution" on line 143 is jargon-heavy and potentially confusing. Please consider explicitly stating that the "non-negative IVT distribution" refers to the raw/total observed IVT field, from which the smoothed large-scale environmental background is subtracted to isolate the transient AR anomalies (if I understood that correctly). This will make the methodology much more intuitive and accessible to a general meteorological audience.

4) On line 156, you introduce a "new axis identification method" and list its advantages. However, it is not clear from the text whether this is a novel modification introduced by the authors in this study,

or if it is a pre-existing feature of the Xu et al. (2020) IPART algorithm. Please clarify this distinction and briefly explain how this axis is determined.

5) Section 2.3 (WCB Identification via ELIAS): This paragraph is dense and difficult to follow because it interweaves the physical description of WCB stages, the Lagrangian training data, and the machine learning architecture all at once. I suggest splitting this into two shorter paragraphs to improve readability:
    a. Paragraph 1 (the physical targets): Define what WCB inflow, ascent, and outflow footprints are based on the traditional Lagrangian criteria (e.g., the 600 hPa ascent in 48 hours, and the > 800 hPa, 400-800 hPa, and < 400 hPa vertical layers).
    b. Paragraph 2 (The ML method): Introduce ELIAS as the Convolutional Neural Network (CNN) tool used to predict these footprints. Briefly explain for a general meteorological audience that the CNN recognizes these spatial patterns using 2D atmospheric fields, and then list the 5 predictors clearly.

6) Section 2.4: The deepening rate threshold applied here effectively limits the analysis to rapidly intensifying ("explosive") cyclones. While focusing on these high-impact systems is a valid research choice, it introduces a substantial selection bias, as ordinary, moderately deepening cyclones also frequently feature ARs and WCBs. By discarding non-explosive systems, the subsequent composites likely overrepresent extreme moisture fluxes and precipitation rates. Therefore, this specific focus needs to be made transparent to the reader. Please explicitly state in the abstract/introduction/conclusion that this study focuses only on explosive cyclones. Additionally, please include a brief discussion addressing how this filtering choice influences the generalizability of the composite results compared to a standard, all-inclusive cyclone climatology.

7) Section 2.5: In step (3), if no cyclone is found within 2000 km, the search radius is expanded to 4000 km. A 4000 km threshold spans almost an entire ocean basin and risks pairing an AR with a completely unrelated cyclone. Please provide a clear justification for using a 4000 km radius, and state what percentage of your total associated AR events fell into this 2000–4000 km window. If a significant portion of ARs are being linked to cyclones at these extreme distances, please discuss how these potentially unphysical pairings impact your analysis.

8) The number of events in each category (16'852 vs. 3'883) is currently not mentioned until Section 4.2. These sample sizes should be introduced much earlier in the manuscript, ideally right where the event classes are introduced in Section 2.7. Furthermore, these proportions differ significantly from those found by Sodemann et al. (2020), which you note in the introduction (23% of the AR overlap with WCBs, 40% occur in isolation). Please add a brief discussion somewhere in the manuscript addressing this discrepancy.

9) Section 3.1: In this section, the presentation of the new climatological results from Fig. 2 is blended with a broad literature review and general dynamical discussion. The immediate inclusion of citations to explain basic synoptic features (e.g., lines 275–286) dilutes the focus on the authors' actual findings and makes it difficult to distinguish new insights from well-established theory. Please restructure Section 3.1 to follow a more logical narrative. First, clearly and objectively describe the specific spatial patterns, peak frequencies, and overlapping fields revealed by Fig. 2. Once the results are clearly laid out, use a separate paragraph (or discussion section) to contextualize these findings with the existing literature. Additionally, when discussing the climatologies, please also include a discussion on existing literature that compares AR and WCB climatologies (in particular Sodemann et al. 2020), either here or earlier in the introduction.

10) Section 2.5: You mix up "peak intensity" and "Maximum Deepening Point" (MDP). MDP is the time of peak intensification (maximum pressure drop per time interval), which occurs before a cyclone reaches its peak intensity (minimum SLP). Please clarify how you are defining MDP and

correct the terminology throughout the text to match the metric used.

11) Line 340: “WCB-influenced ARs occur closer to cyclones” – This can be expected, as WCBs are by definition associated with extratropical cyclones. I would briefly mention this.

12) The current titles for the two event classes, “WCB-influence” and “WCB-absent,” are somewhat awkward and imprecise. This nomenclature becomes particularly confusing in Figure 5b and on line 369 (“As per the WCB-absent cases (Figure 5b), the inflow is substantially reduced occurring at a maximum of 10%.”), as it is contradictory for a “WCB-absent” case to still exhibit non-zero WCB inflow frequencies. I would suggest to rename these two categories throughout the manuscript and figures to more intuitive, descriptive titles that explicitly clarify that you are sorting based on the presence or absence of WCB ascent colocation. And please revise the text (particularly line 369) to reflect this updated terminology, and briefly explain why inflow frequencies can still be non-zero in the non-colocated composite.

13) Fig. 5: I’m not sure I understand the units, is it the percentage of ARs that have a WCB (inflow/ascent/outflow mask) at the specific grid point?

14) Line 367: The authors state that “convergence occurs above 40%” when describing Fig. 5a. However, Fig. 5 shows the relative frequency of WCB occurrence, and not wind or moisture convergence. Please correct this terminology.

15) Lines 373-374: “On Figure 5d, the WCB-absent ARs are generally located close to the lowest SLP values of Figure 4d … “ – I don’t understand what you mean here. There are no SLP contours in Fig. 4d.

16) Section 4.2, lines 376–388: The second half of this paragraph shifts away from describing the figure into a broad discussion (e.g., orographic comparisons and forecasting implications). To maintain a clean narrative, please move these interpretive points into a dedicated discussion section, keeping Section 4.2 strictly focused on the objective description of the Fig. 5 composite fields.

17) Lines 412–413: Concluding that ARs, WCBs, and ETCs are an “integrated, co-evolving system rather than independent features” is too generalized, as this section only analyzes a subset of pre-filtered ARs that include WCB ascent. Please rephrase this statement to clarify that this tight temporal coupling applies specifically to this coupled population, rather than generalizing to all AR events.

18) Lines 422-425: The sentences defining IVT, contrasting it with precipitable water, and invoking the Clausius-Clapeyron relation are generic introductory concepts. To keep the focus strictly on the analysis of Fig. 7, I would suggest removing these sentences or moving them to the introduction.

19) Lines 425-427: The number of cases should be introduced much earlier (at the beginning of Section 5, where the composite framework is first presented).

20) Section 5: The units “mm/AR” to describe precipitation amounts are non-standard. Please explain how “mm/AR” is calculated (e.g., is it an area-average over the AR mask, or an accumulated total?) to ensure clarity.

21) Lines 446–447 state that “the highest decrease in SLP occurs between -6h and +6h of the maximum IVT.” However, the purple line (ΔSLP/6h) is nearly horizontal from -24h to +6h, with only a very minor, barely visible minimum at t = 0h. Framing this almost-flat behaviour as a meaningful intensification window overinterprets a very weak signal. Please adjust the text to

clarify that the deepening rate remains largely uniform during this period.

22) Section 5.3 and Fig. 9: The text and axis labels refer to the purple line in Fig. 9 as the "Maximum Deepening Point (MDP)." Because the MDP is a single point in time, it cannot evolve continuously over a 48-hour window. It appears the authors are actually plotting the 6-hourly SLP change. Please correct this terminology throughout the section and figure caption.

Typos and wording:

Please note that the examples listed below are not exhaustive; please carefully revise the entire manuscript to ensure all remaining minor typos and grammatical errors are addressed.

23) Lines 58, 118: IVT (vertically integrated vapor transport), ECMWF (European Centre …)
The spelled-out word should come before the parentheses, with the abbreviation inside.

24) Lines 63, 121, caption Fig. 4, etc.: Please use the abbreviations "ARs", "IVT", "SLP" here and throughout the manuscript, as they have already been defined earlier (check also for other abbreviations).

25) Terms such as "WCB inflow" and "WCB ascent" (lines 20, 98, 99, etc.) are used inconsistently with and without hyphens. Please standardize these throughout the text (preferably without hyphens, unless they are used as modifiers preceding a noun).

26) Research question 2: The phrasing "time of maximum deepening point (MDP)" sounds slightly redundant. I suggest omitting "point (MDP)" and changing it to "time of maximum deepening".

27) Line 173: "climatological" – Typo: "climatology"

28) Line 192: "degree.latitude" – "degree latitude"

29) Lines 240, 335: "composite" – typo: "composites"

30) Line 276: "inflow highest climatology" – replace by "highest inflow frequency"

31) Line 311: "coinciding" – change to "coincides"

32) Caption Fig. 4: Please specify what the purple circle stands for.

33) Lines 337, 338: do you mean "eastward" instead of "westward"?

34) Line 353: "rotate" – typo: "rotated"

35) Line 396: "How does … varies …" – change to "How do … vary"

36) Lines 405–407: This sentence contains repetitive wording, using the phrase "high precipitation" twice. Please revise.

37) Lines 490-492: Please rephrase for clarity. For example: "In AR-only cases, precipitation is substantially weaker (under 5 mm) and forms a diffuse northeastward extension toward the cyclone center. This weaker precipitation may be driven by synoptic-scale uplift, frontal processes, or residual ascent from prior WCB activity that was too weak to be detected by the ELIAS model."

38) Line 547: "has showed" – change to "has shown"