The manuscript entitled “Environmental characteristics associated with the development of tropical-like features in Mediterranean Cyclones” makes use of **ERA5** and the **Flaounas et al. Mediterranean cyclone track catalogue** to identify all cyclones from 1979–2020 and then classifies them using the **Hart cyclone phase space** into:

- ETC: cold-core extratropical cyclones
- DWCC: “deep warm-core cyclones” (their broader umbrella including medicanes / MTLCs)
- WETC / SETC: weak vs **strong** ETC (15 % deepest SLP)

They then compare environments and evolution of DWCC vs SETC and WETC:

- SST, climatological and instantaneous **Potential Intensity** (PI) and its decomposition into contributions from SST vs tropospheric temperature / humidity
- Upper-level PV anomalies and vertical wind shear
- Winds, precipitation, surface fluxes, and SST cooling along the track to discuss **intensification and decay mechanisms**

I don’t see any fundamental scientific error or contradiction with the existing medicane / MTLC literature. The contribution to the current Mediterranean cyclone literature though can be characterized as incremental: it quantifies, on a relatively large sample, how potential intensity, SST, wind shear and PV anomalies differ between deep warm-core Mediterranean cyclones and other strong Mediterranean cyclones, and it links decay to cold-wake feedback in a way that’s consistent with tropical-cyclone theory.

Main messages:

- DWCC and strong ETC reach **similar peak winds**, but DWCC produce stronger precipitation and stronger surface fluxes.
- 36 h before peak, DWCC tend to be over **warmer SST**, **higher PI**, and **lower vertical wind shear** than strong ETC, while **PV anomalies are similar** between the two groups.
- The **PI anomalies** that distinguish DWCC are mainly due to an **anomalously cold mid-troposphere** (linked to upper PV intrusion) plus a more modest SST anomaly; lower-tropospheric humidity and temperature anomalies are secondary.
- During the decay, DWCC experience **~3 K SST cooling** and a corresponding PI reduction; sensitivity tests with “only SST evolving” PI suggest the SST drop is the main contributor to the PI decrease, consistent with cold-wake feedback known from tropical cyclones.

They explicitly connect their results to:

- Classic medicane climatologies and environmental studies (e.g. Cavicchia et al. 2014; Ragone et al. 2018; Miglietta & Rotunno 2019)
- The new **CYCLOPs** framework and modified PI concept of Emanuel et al. (2024/2025).
- The 2024 GRL paper “A New Refinement of Mediterranean Tropical-Like Cyclones Characteristics”, which also uses ERA5 and CPS but focuses on cyclone *classification* rather than environmental PI decomposition.

What this paper adds to our understanding:

1. **Systematic comparison with strong cold-core ETC in the same basin.** Rather than studying MTLCs in isolation, they explicitly compare DWCC to the subset of “strong” ETC with similar intensity (in SLP and winds) and similar upper-level PV anomaly, and show that what *distinguishes* DWCC is essentially:
    - higher PI and warmer SST,
    - weaker shear,
    - and stronger precipitation at peak.
2. **Decomposition of PI anomalies for a large sample.**

    There have been case studies and theoretical arguments that upper-level PV intrusions and SST both modulate PI, but here they quantify for many events that the dominant contribution to PI anomalies near DWCC comes from a **cold mid-tropospheric temperature anomaly**, with a secondary contribution from SST, and very minor roles for near-surface humidity/temperature.

    That’s a meaningful clarification of *how* PI is raised in these events and links nicely to the CYCLOPs picture.

3. **Climatological cold-wake / decay discussion.**

    Cold-wake feedback on medicane intensity has been discussed mostly in case studies or coupled simulations. This paper makes a first attempt at a **climatological composite** showing that DWCC experience large (~3 K) cold wakes, and that PI decay in composites is mostly explained by SST evolution, reinforced by slower translation speed and shallow mixed layers in the Mediterranean.

Overall, I’d characterise the novelty as **moderate**. It does not contradict the literature, but it mostly tightens and quantifies patterns that had been hypothesised or shown in small samples. I would recommend some major revisions before recommending it for publication.

Major scientific and conceptual issues

1. The paper introduces **DWCC** as a thermal-structure class based purely on CPS warm core (−Vl, −Vu) over sea for ≥6 h, then often speaks about "tropical-like cyclones" in a way that suggests DWCC ≈ MTLC/medicane.

   But CPS warm cores in ERA5 at this resolution can include **warm seclusions and hybrids** that many in the community would not call "medicanes" in the strict sense (highly axisymmetric, deep convection around an eye). The recent GRL paper by Gutiérrez-Fernández et al. explicitly argues for refining that distinction.

   Suggestion: Make the taxonomy very explicit up front: DWCC is a CPS-based class that includes medicanes, CYCLOPs, and some warm seclusions, *not a 1:1 medicane list*. Also, be consistent in using "DWCC" for results, and reserve "medicane/MTLC" for when they refer to the historical literature or clearly defined subsets. Finally, add a short quantitative estimate of how many DWCC correspond to known medicane events or to stricter MTLC definitions, if possible.

2. **Dependence on CPS and ERA5 resolution:** The authors do acknowledge the limitations of CPS and ERA5 (resolution, humidity and wind biases, structure of convective systems). But in the actual interpretation, those limitations are sometimes underplayed.bIssues to highlight in the text:

   CPS limitations: Only −Vl and −Vu are used (no B), which is sensible given known issues with asymmetry, but that accentuates the risk of misclassifying warm seclusions as "tropical-like". The authors should be even more explicit about what kinds of storms might be included in DWCC and how that might bias conclusions (e.g. any tendency to favour systems near strong baroclinic zones).

3. **Causality vs correlation in the decay phase:** The authors argue in Lines 424-426 that DWCC decay is "typically linked to a drop in SST, which drives the decrease of PI over time", aligning with TC cold-wake feedback theory.
   But what they show is:

- A composite PI time series that decreases from −36 to +36 h.
- A composite SST time series and a PI sensitivity experiment in which letting only SST vary reproduces most of the PI decrease.

- A composite drop in translation speed that would itself enhance cooling and exposure to cooled water.

This is **strong circumstantial evidence but not a direct causal demonstration**.

I suggest to soften statements from “is linked to / drives” to “our composites are consistent with the SST-controlled PI feedback known in TCs” and be explicit that other factors (changes in shear, upper-level PV environment, baroclinic forcing) are not ruled out.

And/or: If feasible within the scope, they could add a simple scatterplot / correlation across individual DWCC between:

- magnitude of SST cooling vs change in PI vs change in intensity,
- controlling for translation speed and initial PI.

4. How different are DWCC and SETC?

From the response to reviewers and the boxplots, it’s clear that:

In many metrics (SST, PI, shear, PV, precipitation), the **distributions of DWCC and SETC overlap strongly**, with statistically significant differences in means but large intra-class spread.

The paper currently emphasises the mean differences but does not make it crystal clear to a non-expert reader that: These variables are *not* sufficient to cleanly classify an individual cyclone as DWCC vs SETC; they only indicate tendencies that favour DWCC development.

I would suggest to adde near the end of Section 3.2–3.3, a short paragraph highlighting this, for example:
“*Despite statistically significant differences in mean SST, PI and shear between DWCC and SETC, the spread and overlap in the distributions is large (see boxplots). None of these environmental parameters alone can be used as a reliable classifier; instead, they highlight the conditions that favour the development of deep warm cores.”*

5. Make a better relationship to CYCLOPs and modified PI

Even though the authors cite Emanuel 2024/2025 and the modified PI definition, they explicitly argue that many DWCC in their sample are probably “cyclops” in the Emanuel sense (PI anomalies mostly due to tropospheric cooling rather than basin-wide high SST), and Emanuel et al. motivate a modified PI metric precisely for such systems.

The choice of the classic PI instead of the modified one should be better explained.

General comments and space for presentation improvement and clarity:

- **Introduction:** In Lines 59-63, I see the main motivation to conduct such a study, but is this what you really show? From my understanding, your results show how the environments of deep warm-core Mediterranean cyclones differ from those of other strong Mediterranean cyclones. You also show some thermodynamic factors that contribute most to raising PI in DWCC and investigate the cold wake and its influence during the DWCC decay.

- Check consistency of terminology: “medicane”, “MTLC”, “tropical-like cyclone”, “DWCC”. Right now they sometimes seem interchangeable.

- In the decay section, explicitly mention whether landfalling DWCC (the ~25 %) have a distinct PI/SST evolution compared to fully maritime ones; even a one-sentence statement that composites exclude or include landfalling systems would clarify.