Reviewer 1:

I thank the authors for their thorough answer to my comments and concerns, and for revising the manuscript accordingly. I am left with very few comments, which I detail below.

1) Regarding the new naming: I agree with these, except WETC, since they contain (if I understood correctly) all ETC, I don't like that they should be named "Weak". How about just ETC?

The ETC category includes all the cyclones defined as cold core cyclones in this study, i.e. the ones with negative values for both $-V_l$ and $-V_u$, for their whole lifetime, or the ones with a short-duration positive value for $-V_l$ only (less than six hours). This ETC category is further divided into two: the 15% of ETC with the lowest Sea Level Pressure (SLP) values were classified as "Strong ETC" (SETC). By contrast, the remaining cyclones are thereafter labeled as "Weak ETC" (WETC). So no, the WETC category doesn't include all the ETC cyclones; it excludes the most intense ones. We thought the appellation WETC would be simpler than "Not Intense ETC" (NIETC), but we could consider changing this labeling.

2) I thank the author for carefully addressing my concerns, running additional analyses, and implementing my suggestions and/or coming up with solutions. I only have one concern left in that matter: I appreciate the idea to use only the 10% highest values of each snapshot to compute the boxplots; However, I am not sure this makes fully sense for SST (Figure 4) and Vertical Wind Shear (VWS) where it is not clear that we are looking for high values necessarily.

We performed a sensitivity test by selecting, in addition to the top 10% of values, the top 5%, 25%, and 100% of the values within the 10° × 10° box (see Table 1). In all cases, the differences in SST and VWS between the IETC and DWCC groups remained significant at the 95% confidence level.

We chose to present the figure based on the top 10% of values because the 10° × 10° domain is typically too large for Mediterranean cyclones. Using a smaller area fixed relative to the cyclone center is also not appropriate, as different variables display their strongest anomalies in different regions (e.g., wind speed is usually highest in the southwest quadrant, precipitation peaks north of the center, and PV intrusions occur in the northwest quadrant).

	p-value			
Variable	5% highest values	10% highest values	25% highest values	All the values (100%)
Precipitation	0.0004	0.0004	0.0053	0.0232
Surface wind	0.2508	0.2713	0.8041	0.9015
SST	0.0028	0.0023	0.0016	0.0025
PV	0.6432	0.5630	0.3521	0.1243
Total PI	0.0003	0.0003	0.0000	0.0000
Climatological PI	0.0011	0.0009	0.0001	0.0000
Wind shear	0.0020	0.0017	0.0009	0.0007
Upper wind	0.0018	0.0014	0.0005	0.0001

Table 1: p-values for a two-sample t-test comparing the mean values of the 5% (left column), the 10% (center left column), and the 25% (center right) highest variable values, and all values (right column) of DWCC and IETC in a 10° by 10° box centered on the cyclones. The sensitivity to the percentage considered is really small. Cells highlighted in red are the ones for which the means are not statistically significant at a 95% confidence level.

3) When you say that DWCC must have positive -Vu & -Vl for six hours, does that mean that 1 or 2 six-hourly time steps must verify this criterion?

The ERA5 data we used to compute -Vl & -Vu are hourly so we mean that 6 hourly timesteps must verify a positive value for -Vl and -Vu. We have clarified this in the text lines 140-142.

Reviewer 2:

The study offers potentially interesting findings related to Mediterranean cyclones and their tropical-like characteristics. However, the authors have not addressed key issues that could make this study potentially publishable in WCD and not a merely very small advance in the knowledge of "medicanes". Once these issues are undertaken, I could provide a more detailed review.

Suggestions for Improvement:
- *Clearly articulate the novel contributions of this work, highlighting how it advances or diverges from previous studies.*
- *Strengthen the discussion by providing a thorough comparison with key prior research.*
- *Improve the introduction and engagement with TT literature, ensuring readers have an adequate foundation for understanding of these phenomena and put them into the context of international community on cyclones and cyclone transitions.*
- *Consider revising the title to better match the content and scope of the study.*

Major Comments:

1) The manuscript does not provide a comprehensive comparison with significant prior works, particularly those by Tous & Romero (2013), Gutiérrez et al. (2024), Zhang et al. (2021), and De la Vara et al. (2021). This omission weakens the justification of the novelty of this work.

We thank the reviewer for pointing out the need for a more explicit comparison with prior work. We have now expanded the discussion in the revised manuscript. Below, we summarise the key points of comparison and clarification.

In short, while previous studies investigated what characteristics are associated with the development of warm core cyclones (which mostly are characteristics of generic cyclonic environments, such as cold upper level intrusions), our work aims at finding key characteristics that differentiate the evolution of the initial baroclinic disturbances. For this reason, rather than focusing on the fields during the warm/cold phase of the cyclones, we investigate the fields that precede the development of the deep warm core phase.

In the following sections, we also discuss previous studies in detail and highlight the connections to our work.

Zhang et al. (2021) investigate the hydrological impacts of Medicanes, with a specific focus on their contribution to extreme precipitation. Their work is highly complementary to ours. They use the same Hart-parameter-based criteria, and they also do not distinguish between warm-seclusion cyclones and truly tropical-like systems. Notably, their composite precipitation patterns closely resemble those we present in Figure 6 of our manuscript. The key distinction between the two studies lies in the objective: while Zhang et al. quantify the contribution of Medicanes to Mediterranean extreme precipitation (estimating that they account for 2–5% of all extreme-precipitation events), our work highlights that deep warm-core cyclones exhibit a precipitation structure that differs substantially from that of extratropical cyclones (ETC). While extreme precipitation is the main argument of Zhang et al., it's only a justification for our study. We mentioned the study of Zhang *et al*. (2021), lines 23 and 271-272, in the new manuscript.

De la Vara et al. (2021) focus on methodological aspects of the Cyclone Phase Space (CPS). They show that the traditional CPS—typically computed from temperature and wind data at 13 vertical pressure levels (300–900 hPa)—can be reliably approximated with a reduced set of only three levels (900, 700, and 400 hPa). A similar method had been used in Ragone et al 2018. Their study relies on CPS criteria identical to those used here: positive upper and lower warm-core metrics (–VTU, –VTL), and B < 10 for at least one 6-hourly time step. Most of their analysis is dedicated to validating the reduced-level method by comparing cyclone numbers, spatial distribution, intensities, and warm-core duration against the full CPS. One result beyond methodological validation is their identification of an increasing number of warm-core time steps in their tracks. In contrast, the purpose of our climatological and spatial analysis is not methodological validation but to reveal that deep warm-core cyclones behave differently from the most intense cold-core cyclones—an aspect not addressed in De la Vara *et al*. We cited De la Vara *et al.* (2021), lines 348-349.

Gutiérrez *et al*. (2024) highlight an important limitation of the CPS framework used in our study and in the works discussed above: the CPS does not distinguish between diabatically driven tropical-like structures and warm-seclusion processes. We fully agree, and we explicitly acknowledge this in our manuscript (line 337): *"We also stress that CPS diagnostics does not differentiate between diabatically driven warm cores and warm seclusions (Mazza et al., 2017; Fita et al., 2007; Gutierrez et al 2024, Miglietta et al., 2025), which are both present in the DWCC class."* Gutiérrez et al. propose combining several structural metrics—such as the radius of maximum winds, the Coupling Index, and the Baroclinicity Index—to differentiate true tropical-like cyclones from warm-seclusion events. Our manuscript takes a different but complementary direction: we show that Potential Intensity is a powerful diagnostic for characterizing warm-core cyclones and for interpreting their thermodynamic environment. While our present analysis does not attempt to distinguish tropical-like cyclones, CYCLOPs, and warm seclusions, it highlights the potential of PI as a physically grounded tool that could contribute to such a separation in future work. We cited Gutiérrez *et al*. (2024) several times in the manuscript, especially concerning the warm seclusions (lines 146-150n and 350-351).

The study by Tous & Romero (2013) is one of the closest antecedents to our work, as it seeks to identify the atmospheric conditions favouring medicane formation. However, differences in methodology, datasets, and cyclone identification limit direct comparison and, crucially, justify the additional insights our study provides.

Tous & Romero identify medicanes using satellite-based visual criteria and analyse only 12 cases over 1982–2003 using the coarse-resolution ERA40 reanalysis. In contrast, our work relies on the composite tracks of Flaounas et al. (2023), which provide a physically consistent representation of Mediterranean cyclones by avoiding the biases of any single tracking algorithm and by centering tracks on strong dynamical features. Using these tracks and a cyclone-phase-space–based definition of warm cores (Hart, 2003), we analyse 142 cyclones (including 48 systems at −36 h before minimum SLP) and perform sensitivity tests on an additional 84 systems at −24 h.

A key difference is the stage of cyclone evolution examined. Tous & Romero evaluate environmental fields only at the time of maximum intensity, when the storms have already developed their warm core. Therefore, their analysis reflects conditions *produced by* an already mature system rather than the conditions that *lead to* warm-core development. By contrast, we examine cyclones while they are still in their cold-core or developing phases, before warm-core features appear. This design allows us to isolate the environmental drivers of the development of tropical characteristics rather than their consequences.

As we have a larger and more physically coherent dataset than that of Tous & Romero, we obtain statistically stronger distinctions between cyclone families. For example, we find a clear and significant separation in potential intensity (PI) between DWCC and IETC systems—contrasting with the “modest performance” of environmental variables reported by Tous & Romero.

Finally, while Tous & Romero associate medicanes specifically with strong PV intrusions, our analysis shows that such intrusions characterise *all* intense Mediterranean cyclones. What distinguishes tropical-like from extratropical systems is instead the thermodynamic environment—namely, the climatological modulation of

SST that shapes PI. This integrated framework linking physical structure, PI, and upper-level forcing was not available in the earlier study.

In summary, although Tous & Romero (2013) provided important early insights, our broader and higher-resolution dataset, combined with a physically grounded classification of cyclone phases, enables a more robust statistical analysis and reveals a clearer physical pattern: strong PV intrusion combined with sufficiently high PI is what allows certain cold-core cyclones to develop tropical-like characteristics eventually. We mentioned the study of Tous & Romero (2013) different times in the discussion of the manuscript, lines 340-348.

The introduction of the manuscript has been rewritten, to more clearly relate to the existing literature and explicitly highlight the novelty of the present work.

2) Information about PI appears to revisit established knowledge without sufficient differentiation or clarification regarding its novelty.

We thank the reviewer for raising the important issue of clarifying the novelty of our use of Potential Intensity (PI) in the context of Mediterranean cyclones. We agree that PI has been discussed previously in relation to Mediterranean cyclones and we have revised the manuscript (lines 377-381) to differentiate our contribution from the existing literature more explicitly.

To our knowledge, only three studies examine PI in the context of Mediterranean cyclones: Emanuel (2005), Tous & Romero (2013), and Emanuel et al. (2025). We have already discussed the similarities and differences with Tous & Romero (2013) in the preceding section, and we now clarify more explicitly how our work differs from the two studies by Emanuel.

Emanuel (2005), who also introduced the term *medicane*, presents a single numerical example to illustrate that under certain synoptic conditions—specifically when a cold, moist upper-level cutoff moves over relatively warm Mediterranean waters—the resulting thermodynamic environment can locally produce high PI values. The goal of that seminal paper is to show that even though climatological PI over the Mediterranean is generally low, short-lived upper-level forcing can create transient pockets of very large PI conducive to hurricane-like development.

This idea is elaborated further in Emanuel et al. (2025), where the authors introduce the term *cyclop* to refer to cyclones—both in the Mediterranean and elsewhere—that locally generate the potential intensity that supports them. In that framework, PI is not viewed as a pre-storm environmental characteristic, but as an emergent quantity associated with the evolving storm structure itself.

Our study differs from both works in scope and purpose. Emanuel (2005) and Emanuel et al. (2025) use PI primarily to articulate and illustrate the physical plausibility of tropical-like development in midlatitude basins, focusing on case studies and conceptual mechanisms. Building on the insight that PI can be informative for Mediterranean systems, our contribution is to examine PI systematically across a large set of Mediterranean cyclones and to use it as a discriminating metric. Specifically, we show that PI provides a robust criterion to distinguish between intense Mediterranean cyclones that develop a deep warm core and those that do not. In doing so, we move beyond demonstrating the presence or possibility of high PI and instead demonstrate its diagnostic value in classifying cyclone evolution.

We have introduced a paragraph in the manuscript to present those concepts (lines 417-424).

3) The paper lacks meaningful engagement with the tropical transition (TT) literature. TT is insufficiently introduced and contextualized.

We appreciate the reviewer's suggestion to deepen the theoretical grounding of our work. We have revised the discussion to contextualize better the warm core transition of Mediterranean cyclones within the broader framework of the tropical transition process (TT).

While the classic paradigm of tropical cyclogenesis (e.g., Gray 1968) emphasizes development in environments devoid of strong baroclinic structures, research over the last two decades has highlighted the importance of alternative pathways. Davis and Bosart (2003, 2004) formalized the "Tropical Transition" (TT) paradigm, describing it as the fundamental dynamic and thermodynamic transformation of a baroclinic, cold-core precursor—such as a cutoff low or a frontal wave—into a warm-core tropical cyclone. This process represents a shift from a system driven by horizontal temperature gradients to one sustained by diabatic heating and the Wind-Induced Surface Heat Exchange (WISHE) mechanism (Emanuel 1986).

Climatological studies, most notably by McTaggart-Cowan et al. (2008, 2013), demonstrate that TT is not a rare anomaly but a significant contributor to global TC counts, with distinct "strong" and "weak" transition pathways depending on the intensity of the initial baroclinic forcing. It is increasingly recognized that the boundary between "pure" tropical cyclogenesis and TT is fluid; even systems classified as purely tropical often benefit from the "kickstart" provided by baroclinic disturbances, such as upper-level troughs, which can optimize the environment for development (McTaggart-Cowan et al. 2013).

In light of this literature, we use the Hart (2003) Cyclone Phase Space (CPS) to track this transformation objectively. In our study, "Tropical Transition" specifically refers to the evolution of a system from a baroclinic, cold-core state into a deep, thermally symmetric warm-core state (sustained for at least six hours). This diagnostic approach directly captures the "transformation" described by Davis and Bosart (2004) and aligns with the global climatological classifications of McTaggart-Cowan (2013).

Regarding TT in the Mediterranean, several studies (Chaboureau *et al*., 2012; Mazza *et al.*, 2017, Kouroutzoglou *et al.*, 2021) have previously examined warm-core transitions of baroclinic cyclones, generally through single-case or very limited case-study analyses and without systematic comparison to the broader population of intense Mediterranean extratropical cyclones. These studies have identified a diversity of pathways to TT, which Miglietta and Rotunno (2019) classify into three main categories based on the relative roles of baroclinic and diabatic processes in sustaining the transition.

As previously mentioned, the novelty of our approach lies in extending beyond isolated case studies to conduct a comprehensive and statistically robust analysis of an extensive dataset of deep warm-core cyclones. Instead of focusing on a specific transition pathway, our study emphasizes systematic synoptic-scale differences between large samples of intense cold-core cyclones and deep warm-core cyclones. This comparative, population-level perspective allows us to examine TT characteristics in a way that complements and extends the existing Mediterranean TT literature, which has primarily concentrated on singular events.

We have revised the manuscript to address these points, lines 456-481, strengthen the TT context, and clarify how our work offers a broader and more generalizable contribution to the understanding of TT in the Mediterranean.

4) The current title might be misleading; "deep warm core cyclones" would more accurately reflect the focus of the manuscript rather than referencing tropical-like cyclones. Because warm seclusions are not necessarily a “tropical-like” feature.

We can change the title to “Environmental characteristics associated with the development of deep warm core Mediterranean cyclones”.

References:

Chaboureau, J. P., Pantillon, F., Lambert, D., Richard, E., & Claud, C. (2012). Tropical transition of a Mediterranean storm by jet crossing. *Quarterly Journal of the Royal Meteorological Society*, *138*(664), 596-611.

Davis, C. A., & Bosart, L. F. (2003). Baroclinically induced tropical cyclogenesis. *Monthly Weather Review*, *131*(11), 2730-2747.

Davis, C. A., & Bosart, L. F. (2004). The TT problem: Forecasting the tropical transition of cyclones. *Bulletin of the American Meteorological Society*, *85*(11), 1657-1662.

De la Vara A, Gutiérrez-Fernández J, González-Alemán JJ, Gaertner MÁ. Characterization of medicanes with a minimal number of geopotential levels. Int J Climatol. 2021; 41: 3300–3316. https://doi.org/10.1002/joc.7020

Emanuel, K. A. (1986). An air-sea interaction theory for tropical cyclones. Part I: Steady-state maintenance. *Journal of Atmospheric Sciences*, *43*(6), 585-605.

Emanuel, K. (2005). Genesis and maintenance of" Mediterranean hurricanes". *Advances in Geosciences*, *2*, 217-220.

Emanuel, K., Alberti, T., Bourdin, S., Camargo, S. J., Faranda, D., Flaounas, E., ... & Romero, R. (2025). CYCLOPs: A unified framework for surface flux-driven cyclones outside the tropics. *Weather and Climate Dynamics*, *6*(3), 901-926.

Gray, W. M. (1968). Global view of the origin of tropical disturbances and storms. *Monthly Weather Review*, *96*(10), 669-700.

Gutiérrez-Fernández, J., Miglietta, M. M., González-Alemán, J. J., & Gaertner, M. A. (2024). A new refinement of Mediterranean tropical-like cyclones characteristics. Geophysical Research Letters, 51(8), e2023GL106429.

Kouroutzoglou, J., Samos, I., Flocas, H. A., Hatzaki, M., Lamaris, C., Mamara, A., & Emmannouil, A. (2021). Analysis of the Transition of an Explosive Cyclone to a Mediterranean Tropical-like Cyclone. Atmosphere, 12(11), 1438. https://doi.org/10.3390/atmos12111438

Mazza, E., Ulbrich, U., & Klein, R. (2017). The tropical transition of the October 1996 medicane in the western Mediterranean Sea: A warm seclusion event. *Monthly Weather Review*, *145*(7), 2575-2595.

McTaggart-Cowan, R., Galarneau Jr, T. J., Bosart, L. F., Moore, R. W., & Martius, O. (2013). A global climatology of baroclinically influenced tropical cyclogenesis. *Monthly Weather Review*, *141*(6), 1963-1989.

McTaggart-Cowan, R., Deane, G. D., Bosart, L. F., Davis, C. A., & Galarneau Jr, T. J. (2008). Climatology of tropical cyclogenesis in the North Atlantic (1948–2004). *Monthly Weather Review*, *136*(4), 1284-1304.

Miglietta MM, Rotunno R. Development mechanisms for Mediterranean tropical-like cyclones (medicanes). *Q J R Meteorol Soc* 2019; 145: 1444–1460. https://doi.org/10.1002/qj.3503

Tous, M., & Romero, R. (2013). Meteorological environments associated with medicane development. *International Journal of Climatology*, *33*(1), 1-14.

Zhang W, Villarini G, Scoccimarro E, Napolitano F. Examining the precipitation associated with medicanes in the high-resolution ERA-5 reanalysis data. Int J Climatol. 2021; 41 (Suppl. 1): E126–E132. https://doi.org/10.1002/joc.6669

Reviewer 3:

We thank this additional reviewer for taking the time to read our manuscript and for bringing constructive criticism to improve the clarity of our work.

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.

We thank the reviewer for this suggestion. The manuscript now explicitly mentions that the DWCC class contains well-known medicanes, warm-seclusions, and cyclops (lines 146-150; 358-361). Also, we removed any term medicane / MTLC referring to the analysis that we performed.

Compared to the work of Dafis *et al.* (2020) and Gutiérrez *et al*. (2024), 9 cyclones out of 13 well-known Medicanes are present in our DWCC dataset (Callisto, Celeno, Cornelia, Rolf, Quendresa, Trixi, Zorbas, Ianos). One is not present in the Flaounas *et al.* (2023) dataset because it lasted less than 24 hours (Maria, 26/09/2006). Three cyclones (Leucosia, 1982; Zeo, 2005; Ilona, 2014) are present in the Flaounas *et al.* (2003) dataset, but they never develop a deep warm core. They only have negative values for lower thermal values (-Vl>0 but -Vu<0). Those info are now included in the manuscript (lines 173-178).

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. Issues 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).

We thank the reviewer for this suggestion. We now explicitly state, both in the Results and Discussion section of the manuscript (lines 146-150; 353-371), that our DWCC dataset contains different types of cyclones: Medicanes, warm-seclusion, and cyclops; and why our results are still relevant nonetheless.
We also note that the Flaounas et al dataset already selects quite symmetric cyclones, thus the inclusion of the criterion B<10m/s does not significantly modify the selection (see lines 144-147 in the manuscript). However, we acknowledge this potential bias in the manuscript (lines 406-409).

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.

As suggested by the reviewer, we modified our interpretation of the results in section 3.4 of the manuscript, weakening the causality statement.

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 add 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."

We modified the end of paragraph 3.3 in the following way:

In summary, our results show that, during their early stages, DWCC are characterized by higher potential intensity than ETC and evolve in environments with comparatively weak vertical wind shear. Both factors are conducive to the development of tropical-like features within cyclonic systems that are initially triggered by upper-level potential vorticity intrusions.

Although statistically significant differences in mean SST, PI, and wind shear are found between DWCC and SETC, the corresponding distributions exhibit substantial spread and overlap. Consequently, none of these environmental parameters alone provides a reliable discriminator between the two cyclone types. Rather, they should be interpreted as indicating environmental conditions that increase the likelihood of deep warm-core development.

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.

Potential intensity is a theoretical upper limit of how strong a tropical cyclone can be depending on its (undisturbed) environment. In the tropics, the large-scale environmental conditions preceding tropical cyclone formation and those prevailing at the time of peak intensity in the cyclone environment are often relatively similar. As a consequence, the potential intensity can be meaningfully computed from pre-storm environmental profiles, which are representative of the thermodynamic state available to the cyclone during its subsequent development. In this framework, the warming of the cyclone core caused by the tropical cyclone itself does not affect the PI calculation. In the Mediterranean Sea, instead, the development of deep warm-core cyclones (DWCC) is strongly influenced by upper-level potential vorticity (PV) intrusions. These intrusions substantially modify the thermodynamic structure of the atmosphere during cyclogenesis, in particular by cooling the upper or mid troposphere and thereby increasing the thermodynamic efficiency of the system. If PI is computed using pre-storm conditions, the PV anomaly is not yet present, leading to an underestimation of the potential intensity available at the time of cyclone development. For this reason, PI must be computed at the time the perturbation is already present. However, this poses a problem as the computation of PI from storm-time fields tends to underestimate the available PI, because those fields already include the effects of the cyclone-induced warming of the core, which violates the conceptual definition of PI as an environmental constraint.

This motivated the development of a modified PI formulation that accounts for the impact of the PV intrusion on the large-scale environment, while excluding the thermodynamic modification directly induced by the cyclone itself. The difficulty arises from the fact that the warming of the cyclone core is dynamically linked to the storm evolution and cannot be cleanly separated from other processes affecting the pressure and temperature fields.

The approach proposed by Emanuel *et al*. (2025) seeks to estimate and remove the cyclone-induced warming by relating it to air density changes inferred from the sea-level pressure decrease at the cyclone center. However, in the Mediterranean context, the central pressure fall is not solely attributable to diabatic warming within the cyclone core. A significant fraction of the pressure reduction can instead be attributed to the presence of the upper-level PV intrusion, which induces mass divergence aloft and lowers surface pressure independently of warm-core development. As a result, attributing the entire pressure deficit to core warming can lead to an overcorrection.

In summary, while the standard PI definition at the time of the cyclone underestimates the potential intensity of the cyclone (due to the presence of the warm core), the Emanuel et al. 2025 modified PI definition overestimates it (due to the underlying assumption that the warm core is the sole driver of the sea level pressure deficit). The actual PI relevant for the development of the cyclone therefore lies between the original PI computed from storm-time conditions and the modified PI that corrects for the cyclone-induced warming (using, as discussed, an upper limit for the correction). Considering that there is no clear physical basis for preferring one of these limits over the other, as both are affected by compensating biases associated with the separation of environmental and storm-induced signals, and considering that, as said in Appendix A of Emanuel et al 2025, **“in general, the contribution of the correction (last term in Eq. A10) is modest”** we prefer not to introduce the modified PI. Clearly, further study is needed to investigate all these aspects, but this goes beyond the scope of our work.

While this would open an interesting discussion, we believe our current manuscript is not the right place for it, as it would divert the attention of the readers from the main results of the work. However, we have briefly introduced the topic in the discussion section (lines 472-475).

References:

Dafis, S., Claud, C., Kotroni, V., Lagouvardos, K., & Rysman, J. F. (2020). Insights into the convective evolution of Mediterranean tropical‐like cyclones. *Quarterly Journal of the Royal Meteorological Society*, *146*(733), 4147-4169.