Review

A cyclone phase space dedicated to extratropical cyclones

Besson et al.

Dear Authors,

I thoroughly enjoyed reading this manuscript, that develops a bespoke phase space to analyse the evolution and structure of extratropical cyclones, particularly in terms of core temperature, thermal asymmetry and baroclinic conversion. While my review contains a fairly large number of comments, most of them are minor and even the key ones should not constitute a substantial barrier for the manuscript to progress through peer review and eventually be accepted. I hope you see this review as constructive and I am happy to be contacted should you have questions on any of the comments.

Main comments

1. <u>"It is not clear yet what the proportion of cold-core and warm-core cyclones in midlatitudes is,"</u> [Lines 3-4]

   This statement is made in the abstract and is repeated in the introduction, then listed as one of the key questions of the study and thus addressed in the conclusions. Whilst I agree with the statement in general, I should point out that a recently published study contains a brief analysis of the proportion of cyclones displaying a warm seclusion at their centre, and therefore being warm-core, at their time of maximum intensity (Gray et al., 2024, see Section 4.1 and Figure 4). The results of that analysis show that the proportion of warm-seclusion cyclones increases more than linearly with cyclone intensity and that for the top 10% intensity cyclones, around three quarters of them have a warm seclusion. This means that intense cyclones are much more likely to be warm-core at maximum intensity than their less intense counterparts. That result does not make your analysis less novel or important, given that the methodologies used are not identical, the scope of your study is markedly different and your analysis is much more detailed than the single section in Gray et al. (2024). Nonetheless, I think you should acknowledge that these results exist and re-frame your discussion as starting from that evidence rather than from an absolute lack of in this particular topic. I should disclose that (as you surely know!) I am an author of that study, but I genuinely believe that including it would improve your discussion, and provide further robustness to your results, regardless of who the authors are.

2. “This study uses the ERA5 reanalysis dataset of the European Center for Medium-range Weather Forecast (ECMWF) from 1979 to 2014. [L88-89]

   Why did you stop at 2014 and did not consider the most recent decade? It seems a fairly odd choice to make, also considering your point on reliability of reanalysis data and particularly as the two examples considered in this manuscript are from 2023. I would suggest considering integrating your analysis with data up to 2024 or 2025, unless a satisfactory explanation for not doing so is provided.

3. “Core temperature” [L120-142]

   In Volonté and Riboldi (2024), already cited in the manuscript, we identify Storm Ciarán as a warm-seclusion cyclone, something that is also done in this manuscript. Looking at Figure 3e-f in that paper, it can be seen that the local maximum in wet-bulb potential temperature characterising the warm seclusion is only present between the surface and 800 hPa, while between 700 hPa and 800 hPa wet-bulb potential temperature above the cyclone centre is actually lower than the immediate surroundings (although probably still higher than 500 km away and therefore not fully conflicting with your results). Starting from this example, I would like to ask a few questions:
   - Could you explain why you decided to look at (dry bulb) temperature instead of wet-bulb potential temperature (as also done in Gray et al., 2024) or equivalent potential temperature? These latter quantities are conserved by the flow and thus represent appropriate markers of different airmasses, meaning that they can be used to identify whether the air at the core of the cyclone originates from the warm sector (i.e. the seclusion at the cyclone centre is warm) or not.
   - Are your results dependent on the pressure level chosen for the computation of DTL, as the example above might suggest? It would be really helpful if you could verify this, even on a subset of your data. See also my minor comment on Figure 8.
   - Did you test radii smaller than 5° (again, see my example above). If not, could you please do so?

Gray, S. L., Volonté, A., Martínez-Alvarado, O., and Harvey, B. J.: A global climatology of sting-jet extratropical cyclones, Weather Clim. Dynam., 5, 1523–1544, https://doi.org/10.5194/wcd-5-1523-2024, 2024.

Minor comments, line-by-line:

Introduction:

- L27: “Additionally, as extratropical cyclones have an impact on climate”
- L28-29: “or even with other Earth system’s components. Finally, another”
- L30-31: Please rewrite in a less colloquial way.
- L32-33: “Most ~~of~~ extratropical cyclones differ from tropical cyclones, which are”. I will stop now highlighting minor grammar/language issues. Please make sure they are addressed throughout the manuscript.
- L39-41: Could you include a citation to show that the concept of cyclone core temperature provided here is commonly used and not something that you are defining for the first time. Related to this, could you provide references in section 2.2.2 to justify your choice of a 500 km radius to represent the cyclone core?
- L51-52: This would be a good place to mention the results in Gray et al. (2024). The first and last sentences of this paragraph should also be modified, according to key comment #1.
- L61: “Dominant”, rather than “dominated”.
- L61-62: “Core temperature…”. Again, this sentence should be modified.

Data and methods:

- L92: “fields analysed” (I think).
- L102: You should provide a reference for each of the two fields, here or after . For relative vorticity you could use Hodges (1995), see details below.
- L106: This section would benefit from saying why you chose to use SLP as the field for the detection step.
- L107-108: Is the merging done to exclude temporary secondary cyclones? Please explain in the text.
- L112-114: My understanding is that only a handful of cyclone tracks are available from the THINICE field campaign. This seems a very small number to base your calibration on. Did you verify that the tracking calibration is sensible using a larger set of cyclones? How do your threshold compare with other works? Surely, this is not the only study tracking extratropical cyclones in ERA5 using SLP.
- L116: A southern limit of 30N is set to avoid detecting tropical cyclones. However, tropical-like cyclones such as medicanes commonly have genesis near or poleward of 30N. Did you check how many are present? What are the implications of including them in your study?
- L147-148: “…to the right and left sides of the cyclone motion.” Croad et al. 2023a,b made sure that uncertainties in the movement of the cyclone (sometimes high for Arctic cyclones with unusual tracks) wouldn’t affect the

calculation of thermal asymmetry by calculating it over a number of bearings and selecting the highest values. If you decided that this was not necessary for your study, could you explain why?

Hodges, K. I.: Feature Tracking on the Unit Sphere, Mon. Weather Rev., 123, 3458–3465, https://doi.org/10.1175/1520-0493(1995)123<3458:FTOTUS>2.0.CO;2, 1995.

Results:

- L217: DTL increases to beyond 7.5K during the initial development of Ciarán. Could this be related to it being a DRW and therefore characterised by strong low-level heating near the centre?
- L256-257: Could you consider including your systematic comparison as supplementary material, or even (maybe only partially) in the main body of the paper? It feels quite important to me and I don’t think the manuscript is too long as it is.
- Figure 3a: Does wind at 200hPa exceed 20m/s everywhere in the map domain?
- L294: This is consistent with the result in Gray et al. (2024) mentioned in key comment #1. Could you be more quantitative here and calculate the proportion of warm-core vs cold-core cyclones (or cyclones exceeding a certain DTL threshold if too many of them are warm-core according to your method) at different intensity percentiles? Without discounting the differences in the methodologies, doing this would be a useful comparison exercise and would provide additional robustness to your results.
- Figure 5: I find intriguing that warm-core cyclone do an almost-perfect phase space loop. Any ideas as to why they display this behaviour?
- Figure 8: looking at the vertical sections in panels i-l (and considering the point in made in key comment #3), would you say that 750 hPa is the best pressure level to be used to characterise cyclones as warm-core vs cold-core? Also in these panels, is the vertical scale linear, logarithmic or something else? The separation between values looks a bit odd, but it might just be my eyes being funny.
- Line 339: You should also mention that PV just about reaches 2 PVU at low levels in the DJF warm-core composite, indicating strong diabatic processes in that set of cyclones (again, there are links to what is shown in Gray et al., 2024 and Volonté and Riboldi, 2024, but I would leave it to you to decide whether you want to mention them again).
- Line 345: Where are you showing EKE in Figure 8? Please clarify.
- Figure 9: With the high variability in the duration of cyclone lifecycle shown in this manuscript (see e.g., the heavy tails in Figure 7b), I would suggest colouring the regions according to the number of hours from the time of maximum intensity. This would better highlight the evolution of BC and give a clearer indication of

where the time of maximum intensity is. It would also make the figure more directly comparable with the timeseries in Figure10b and justify the L369 statement ("the baroclinic conversion peaks before the cyclone maximum intensity"). If we use Storm Ciaran as an example, we see that its deepening lasts only a couple of days while the mature and decaying stages take almost six days, but the proportion between these two periods might be very different for other cyclones.

- L369: "The main pathway is however from high and positive values of BC to lower ones…". This is not very is to see at the moment, particularly in panel a.
- L370: "core temperature anomaly"

Conclusions:

- L465-470: The contribution of diabatic generation and its relative importance compared to baroclinic conversion is only mentioned briefly here, with a single figure relegated to the supplementary material. While I appreciate that a substantially longer and more detailed analysis would go beyond the scope of this manuscript, I think that this topic would deserve to be expanded a bit more, even if as an example of potential future work. In particular, it would be nice if you could compare your results against those in Christ et al. (2025) which, in several aspects (such as the tendency of diabatically driven cyclones to intensify faster and tend more towards a Shapiro-Keyser structure) are consistent with yours.

Christ, S., Quinting, J. & Pinto, J.G. (2025) Characteristics of diabatically influenced cyclones with high wind damage potential in Europe. Quarterly Journal of the Royal Meteorological Society, e70083. Available from: https://doi.org/10.1002/qj.70083

Best wishes
Ambrogio Volonté