REFEREE COMMENTS #1 (ANONYMOUS REFEREE):

This is a very nice addition to the literature on extratropical cyclone classification and development. It presents a new method of determining whether a cyclone is warm or cold core and makes use of the phase space ideas of Hart (2003) to group cyclones. The paper presents two case studies and then climatological analysis of the groups for winter and summer. The paper is well presented and nicely written and I have mostly minor comments, suggestions, and queries.

Thank you very much for having taken the time to read our work. This is greatly appreciated and your comments are very relevant and useful to improve our paper. We have tried our best to take every remark into account and to provide answers to your questions.

Major comment

1. Given some of the findings about the cold-core versus warm-core cyclone intensities, I find myself wondering if the comparisons between the subgroups when it comes to baroclinic conversion are entirely fair. Would it be fairer to compare this aspect for a subset of each group with the same intensities? This might also be addressed using a case study analysis as I think it would be very useful to show a case of a cold-core cyclone. So, for two cyclones (or two small subsets) with similar intensities, does the baroclinic conversion show up as being much higher for the warm-core group despite the same intensity?

   That is an interesting remark. Let us here distinguish between the two seasons:

   - In JJA, the comparison between warm and cold cores is fair to that viewpoint because they are two subsets with a similar intensity distribution, as can be seen in Fig. 7a (see transparent color curves). In that case, the differences in baroclinic conversion exist but are quite minor (Fig. 10c).
   - In DJF though, the intensity distribution is indeed different. As can be seen on Fig. 10c, the baroclinic conversion is much higher for warm cores and this is surely related to their higher intensity. To make that clearer in the text, we added the following sentence: 'This is consistent with a higher ETE rate of change and by extension a higher deepening of the DJF warm-core cyclones subset.' We do not go further than that in the article. However, following your question, we did the composites of Fig. 10 again with two new subsets: DJF warm cores with intensity between 980 and 1,000 hPa and DJF cold cores with intensity between 980 and 1,000 hPa. It can be seen on Fig. R1. Taking closer intensity subsets leads to closer localizations (Figs. R1b and R1c) and closer BC as well, but warm-core cyclones have still a 25% larger BC than cold-core cyclones (Fig. R1e), which is similar to the summer findings.

   At the end of the paper, the focus is made on JJA specifically because warm- and cold-core cyclones have the same intensity. The main conclusion is that, at first order, their mean baroclinic conversion is similar, explaining a priori well the intensity similarities. However, it is interesting to see that, despite these overall similar intensities, baroclinic conversions are spatially very different between the two structures.

Minor comments

- Line 66 – since this question about occluded cyclones is not really fully answered in the paper, I wonder if the emphasis on these should be removed.

  You are right we do not fully answer this question, we only study the case of Domingos. However, we decided to highlight this question because occlusion and seclusion are two classic models for extratropical cyclones, well known by the community. At some point, there was hope that the diagnostic could be of use to easily split cyclones of these two categories but Domingos is a counter-example of that. Moreover, we find it interesting to highlight this result because Hart was suggesting in his paper that occluded cyclones are cold-core cyclones which does not seem to be true, at least not all the time. Despite this highlight, we clearly acknowledge in the conclusion that a more systematic study would be needed.

- Line 89 – why is the end date of 2014 chosen when ERA5 data are available to the end of 2025?

At first, we stopped in 2014 because we had in mind to compare with model outputs only available until 2014. It turned out that this comparison is not included in the study after all. There is thus no more good reason to stop in 2014. We have extended the analysis to 2024.

- Line 138 – When you say "largely different" do you mean that they vary widely?

  We mean the values of DTL and $-VTL$ computed for a same snapshot can be very different. We rephrased the sentence as: 'As we will see in the next section, the difference between the two parameters $-VTL$ and DTL can be large for extratropical cyclones.', hoping it is clearer.

- Line 240-241 – The sentence is confusing. It is hard to know whether you are comparing the storms or the CPS diagrams here.

  We are comparing the evolution of the CPS parameters associated to the two storms in the two different CPS diagrams. To clarify that, we rephrased it as: 'As shown above, the evolutions of B and B_K in the two CPS diagrams are similar for the two storms but the ones of DTL and $-VTL$ are dramatically different, particularly during the early stage of their life cycle, to the point that they have opposite signs in their respective CPS diagrams.'.

- Line 257 – make clear this is just the Northern Hemisphere.

  Done.

- Line 259 – rather than the differences being opposite in sign, I think you mean the differences are sometimes negative.

  Indeed. It has been rephrased.

- Line 266 – Add "whereas DTL is representative of the difference between the center and the average around a 5 degree radius circle".

  Done.

- Section 3.2.1 Make it clear at the start that Figure 3 and the description is purely for illustrative purposes and is not any new information.

  The aim of these maps is indeed more to validate the tracking method than to bring new results. We added the following sentences at the beginning of 3.2.1: 'To validate the tracking algorithm calibration and our methodology, this subsection displays seasonal track density maps of midlatitude cyclones and compares them with the existing literature.', and at the end: 'The consistency with existing literature confirms that the tracking methodology is accurate.'.

- Figure 3 – I wonder why you chose to plot the 200 hPa zonal wind. This is much more indicative of the subtropical jet, rather than the eddy driven jet that is strongly related to the stormtracks.

  That is a very fair remark. The eddy-driven jet would be more characterized by the 850 hPa zonal wind. However, we wanted to show here the vertical shear rather than the jet itself, that is why we plotted the upper level zonal wind. 200 hPa is a rather high level though, we plotted instead the 300 hPa zonal wind to be closer to the tropopause.

- Figure 4 – Can you explain why you have chosen to look at the maximum intensity in this figure?

  There are several ways to use ETC-CPS. Here, we chose to plot only the points corresponding to the time of maximum intensity because the categories of warm- and cold-core cyclones are after defined at this specific time. The visualization in Fig. 4 is thus a direct representation of the proportion of warm- and cold-core cyclones according to our definition of these subsets. Then, the question of choosing maximum intensity as the instant to define the warm- and cold-core subsets can be raised too. Alternative plots were tried and led to similar findings. An example in shown in Fig. R2, plotted at the time of maximum growth rate. Percentages of warm- and cold-core cyclones are very close.

- Line 345-350 – To what figure does this paragraph refer?

  It still refers to Fig. 8. It was not clearly mentioned so we added precise references to the figures. It was maybe also unclear because of the mention of eddy kinetic energy: this variable is not shown directly but rather roughly estimated from the geopotential heights horizontal gradients following geostrophic balance. It is now more explicitly stated.

- Line 356 – Is this shown? Please refer to a figure.

  We were referring to the rectangular panels of Fig. 8, it is now clearer.

- Figure 9 – Can you explain why you have chosen to plot BC versus BK instead of versus DTL?

  Thank you for your question because our choice was here rather subjective. When plotting DTL vs. BC with the mean life stage in shading (Fig. R3), we do see that BC is close to 0 and DTL near 0 or slightly negative at the latest life stages. It also shows that, at the early life stages, BC and DTL are both mainly positive, which is consistent with the usual westward tilt with height of geopotential isolines during baroclinic interaction. Indeed, such a tilt characterizes an increase with height of the geopotential anomaly above the cyclone center, which corresponds to a warm temperature anomaly at low levels following hydrostatic balance. Looking at this alternative ETC-CPS, we decided that it is a better visualization and changed it in the manuscript.

- Line 457 – Is this a fair comparison due to the sample size and overall higher intensity of warm core cyclones?

  The idea of these sentences is to contrast the difference between cold-core and warm-core cyclones during the two seasons. In winter, we do see large differences in intensities and baroclinic conversions between the two types of cyclones which can be largely explained by large differences in baroclinicity. In contrast, in summer, the two categories of cyclones have similar intensities, similar baroclinic conversions and similar baroclinicities. The explanation given states that tracking in regions with higher environmental baroclinicity results in higher baroclinic conversion and thus higher intensities. This seems to work at first order and to be consistent with summer findings. It can also be seen on Figs. R1b and R1c that the two winter subsets of the same intensity track approximately in the same regions.

- Line 470 – What about winter?

  In winter, the differences between the two categories of cyclones are so large that it does not seem of particular interest to have a detailed energy budget to compare them. We do see very large differences in vertical velocities between them (Fig. 8e,f) and we expect large differences in diabatic generation term too. What would be more interesting is to have a detailed energy budget for the two subsets of wintertime cold-core and warm-core cyclones having the same intensities.

Typographical comments

- Line 98 – consist in -> consist of.

  Done.

- Line 98 - times -> time.

  Done.

- Line 116 – lies -> lie.

  Done.

- Line 117 – originates -> originate.

  Done.

- Line 127 (and elsewhere) – please avoid using the brackets with (resp. cold) etc. It is much clearer to write out the full sentence.

  Done.

– Line 133 – circle -> radius.

Done.

– Line 322 – smaller -> lower.

Done.

– Line 331 – higher -> larger.

Done.

– Line 355 – reference to positive values of omega representing ascent and negative values representing descent should be the other way round.

Right. Done.

REFEREE COMMENTS #2 (AMBROGIO VOLONTE):

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.
Thank you very much for having taken the time to read our work. This is greatly appreciated and your comments are very relevant and useful to improve our paper. We have tried our best to take every remark into account and to provide answers to your questions.

Main comments

1. "It is not clear yet what the proportion of cold-core and warm-core cyclones in midlatitudes is," [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 reframe 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.

   We fully agree with your comment and this is the reason why, in the introduction, we detailed the findings of Gray et al. (2024) and highlighted how contradictory the results of Gray et al. (2024) are with those of Hart (2003) (see lines 50 to 56 of the paper). At the end, our study fits well with Gray et al (2024)'s findings. The following changes were made in the article:

   – Abstract: 'Few studies have addressed what the proportion of cold-core and warm-core cyclones in midlatitudes is but it is not clear yet if occluded cyclones are cold-core or warm-core cyclones and how different the processes leading to cold-core and warm-core cyclones are.'

– Introduction: in the revised introduction, we detail the findings of Gray et al. (2024) and highlight how contradictory are the results of Gray et al. (2024) with those of Hart (2003): ‘However, Gray et al. (2024) studied warm seclusions in the midlatitudes and observed that the proportion of warm seclusions, thus of warm-core cyclones, varies between 40 and 80% depending on the intensity percentile. They concluded that a ’warm core is typical of midlatitude cyclones’ and that the most intense midlatitude cyclones are mainly warm cores.’

– Conclusion: ‘Another important result is that the deepest cyclones are warm-core cyclones and their warm core is more pronounced at the early stage of their life. This is in agreement with the findings of Gray et al. (2024) but not with Hart (2003), whose diagram is showing a majority of cold-core cyclones, but the diagnostic used in that paper, denoted to as $-VTL$, is not appropriate to detect the core temperature for baroclinic non-axisymmetric cyclones.’

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.

   At first, we stopped in 2014 because we had in mind to compare with model outputs only available until 2014. It turned out that this comparison is not included in the study after all. There is thus no more good reason to stop in 2014. We have extended the analysis to 2024.

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.

     That could indeed be another possibility. The choice of basic temperature was guided by simplicity. For an equivalent potential temperature, the specific humidity needs to be extracted as well for example to compute a diagnostic. However, we have tried to do the statistical ETC-CPS again with a DTL based not on T but on θ_e, the equivalent potential temperature. Figure R4 shows that the differences are minor. DTL_e values tend to be a little bit higher because taking into account the humidity strengthens the gradients but the percentages of warm- and cold-core cyclones as well as the general pattern of the ETC-CPS do not change a lot.

   – 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.

     Some results depend on the pressure level chosen like the proportion of cold-core and warm-core cyclones. We plotted an additional figure (Fig. R5a) to show the sensitivity of the proportion of cold-core cyclones to the pressure level. Our initial objective was to compare with Hart(2003)’s results where the core temperature diagnostic was made within a layer between 600 hPa and 900 hPa. This is the reason why we chose the middle of that layer, that is the 750 hPa level, for our analysis. Our purpose was also to look at the core temperature at lower levels and not in the middle of the troposphere or above as in Croad et al. (2023) because we were interested in the lower-level cyclones. While the proportion does not change much in summer below 600 hPa (transparent blue in Fig. R5a) it does change in winter with less and less cold-core cyclones when considering lower levels.

- Did you test radii smaller than 5° (again, see my example above). If not, could you please do so?

  Similarly as for the pressure level sensitivity, we plotted an additional figure (Fig. R5b) to show the sensitivity to the radius chosen. The radius sensitivity is quite low but it is still interesting to note that the higher the radius chosen, the higher the proportion of cold-core cyclones.

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"

  Done.

- L28-29: "or even with other Earth system's components. Finally, another"

  Done.

- L30-31: Please rewrite in a less colloquial way.

  We rephrased it as: 'Finally, as extratropical cyclones are associated to strong winds and precipitations, their impacts on populations, infrastructures or ecosystems can be heavy. Risk assessment for these phenomena is crucial, especially as their structure and intensity are likely to be modified by climate change (Priestley and Catto, 2022)'.

- 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.

  Done. We have tried to correct other small mistakes too.

- 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?

  There has not been many studies dealing with the concept of a unique core temperature for an extratropical cyclone. Most of the time, an extratropical cyclone is defined with both a warm sector and a cold sector. It cannot be said that our definition is brand new but it is rather a logical intuition of what a core temperature would be. It is equivalent to the definition used by Croad et al. (2023) for their R_{O_T} parameter and this study is mentioned at the end of 2.2.2 Core temperature section. As regards to the choice of 500 km, we chose it to be the same as the two main studies about CPS (Hart, 2003 and Croad et al., 2023) and then conducted sensitivity tests. A sentence was added in 2.2.2 to explicit that.

- 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.

  Done.

- L61: "Dominant", rather than "dominated".

  Done.

- L61-62: "Core temperature…". Again, this sentence should be modified.

  Done.

Data and methods:

– L92: “fields analysed” (I think).

Done.

– 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.

Done.

– L106: This section would benefit from saying why you chose to use SLP as the field for the detection step.

Done.

– L107-108: Is the merging done to exclude temporary secondary cyclones? Please explain in the text.

Yes. It is done to avoid the detection of smaller kernels inside a big cyclone. The value of merging was calibrated with the RALI-THINICE example.

– 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.

Indeed, the first calibration was made with only 4 cases. However, several months of work consisted in: 1) gathering as much bibliography as possible on different tracking algorithms used in the community, 2) identifying ranges of variations of the parameters, 3) testing sensitivity of the number of tracks detected and on some track density maps, 4) eventually adapting the values of the initial tracking calibration. We believe there is no need to include all that work in the current paper but the track density maps presented in Fig. 3 play the role of a check that the tracking used reproduces the main extratropical cyclone characteristics identified in the literature. This figure brings indeed nothing new but allows us to check the tracking is consistent. We added a few sentences at the beginning and the end of 3.2.1 to clarify that. It is on the whole quite hard to compare with existing literature as nobody is using the same parameters and some information can be missing in the description of the tracking steps... Neu et al. (2013)'s work is the only one trying to compare several algorithms on a common basis. We have tried our best to gather information and to start from that but we think the best check can then only be made with some manual eye-tracking or with the reproduction of characteristics highlighted in the literature and commonly shared in the community.

– 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?

We did not do a particular tracking of medicanes. However, there are 1 or 2 per year only which is very little compared to the total amount of midlatitude cyclones (reference 1 below). Moreover, medicanes may not be well represented and may even be underestimated in ERA5 (reference 2 below). We did not try to follow medicanes specifically but we have tried to remove a subset of cyclones that have their genesis in the Mediterranean area (Fig. R6). We have done again the distributions of Fig. 7 (Figs. R7a and R7b) and the composites of Fig. 8 (Figs. R7b-n) without these Mediterranean cyclones and the results are similar. Thus the few exceptional tropical-like cases do not affect the statistical results. It would however for sure be of interest to study them more in depth with this new CPS tool but it was not the aim of the paper.

[1] Cavicchia, L., von Storch, H., & Gualdi, S. (2014). *A long-term climatology of medicanes.* Climate dynamics, 43(5), 1183-1195.

[2] Pantillon, F., Davolio, S., Avolio, E., Calvo-Sancho, C., Carrió, D. S., Dafis, S., ... & Flaounas, E. (2024). *The crucial representation of deep convection for the cyclogenesis of Medicane Ianos.* Weather and Climate Dynamics, 5(3), 1187-1205.

– 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?

We again took this decision for simplicity reasons. We believe this can be of importance for particular case studies but it does not affect the statistics. We show on Fig. R8 again the ETC-CPS with the norm of $\nabla\overline{\theta}$. The gradient will necessarily take the maximum asymmetry. These CPS are similar to those obtained with B_K.

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?

This could be but it is not necessarily the case as Domingos, which does not result from Diabatic Rossby Wave, has more or less the same DTL values at the beginning of its lifetime. So the strong heating induced by the DRW surely contributes to high DTL values but it is however not the only phenomenon that can explain this kind of values.

– 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.

We plotted Fig. R9 and added it in the supplementary material as well. One sees on Fig. R9a that a scaled B in K and B_K are proportional with a very good coefficient of linear regression. Figure R9b highlights the fact that Hart (2003)'s $-VTL$ diagnostic mainly commits mistakes for warm-core cyclones ($DTL > 0$) which are categorized as cold-core cyclones ($-VTL < 0$) and these mistakes particularly happen at the beginning of the life cycle (red shading of the dots). Finally, Fig. R9c shows that absolute differences between $-VTL$ and DTL are proportional to B_K's absolute value.

– Figure 3a: Does wind at 200hPa exceed 20m/s everywhere in the map domain?

No, indeed. We actually did not plot every contour below the bold one, it was rather unclear, it is now specified in the caption.

– 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.

We plotted an additional figure (Fig. R10) to show that. Fig. R10c is designed as the one in Gray et al. (2024). The finding is similar: the more the cyclone is intense, the more likely it has a warm core. The percentages are higher because we take here all cyclones and not only the warm seclusions. We also made the link with Gray et al. (2024) in the conclusion.

– Figure 5: I find intriguing that warm-core cyclones do an almost-perfect phase space loop. Any ideas as to why they display this behaviour?

The fact that they loop is notable but we do not know what could explain that. However, one needs to remember that these are composites of several different tracks so it might also just be by chance that the mean of all these tracks loop. For instance, there is no loop in the case studies of Ciarán and Domingos.

– 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.

We already answered a bit before in your key comment #3. Our main objective was to focus on a level similar to the Hart's diagnostic to make the comparison easier. Figure 8 shows some vertical variations that are worth highlighting: (i) the small warm-core area in DJF cold-core cyclones at lower levels (setting thus a lower bound of DTL diagnostic to 900 hPa), and (ii) the quite shallow warm-core in JJA warm-core cyclones (setting thus a higher bound of DTL diagnostic to 600 hPa). Finally, we think 750 hPa is a good compromise to look at low levels without being focused on boundary-layer effects. As regards the vertical scale, it is logarithmic in pressure and approximately linear with height.

– 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).

  To have a better idea of PV in low levels, we plot in Fig. R11 the 1.5 and 1 PVU PV contours. There is indeed a slightly stronger lower-level potential vorticity in warm-core cyclones than in cold-core ones, both in DJF and JJA, probably indicating stronger diabatic heating in the middle of the troposphere. This is consistent with Fig. S5 that shows that diabatic heating is larger in warm-core cyclones in summer. In cold-core cyclones, PV is increasing with altitude in the whole column whereas, in warm-core cyclones, PV in increasing with altitude in the lower levels and then decreasing with altitude.

– Line 345: Where are you showing EKE in Figure 8? Please clarify.

  EKE is not shown directly but rather qualitatively estimated from the geopotential heights horizontal gradients, we clarified it by saying explicitly it is suggested.

– 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 Figure 10b 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.

  Following your suggestion, we tried that (Fig. R12) and the results are similar. We preferred to keep the non-dimensional life stage in order to keep the same weights for all 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.

  There are two things to see here. The fact that the main pathway is from high values of BC to low values is visible with the colours: high values of BC are found at early lifestages (red) and close-to-zero ones are found later in the life cycle (blue). This is stated a few lines before. Then, the fact that the pathway with positive BC values is the main one is seen with the black contours that are shifted toward the positive half of BC. It is more visible in a DTL vs. BC ETC-CPS as in Fig. R3 so we replaced the manuscript figure by this one.

– L370: "core temperature anomaly"

  Done.

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.

Indeed, the study of the diabatic generation rate would require a study in itself if one is to make a complete computation of all terms, that is why we decided not to expand ourselves on that topic. At this stage, it is difficult to make a link with Christ et al. (2025)'s work, because the authors separated winter storms depending on the WCB activity and diabatic heating and they show their two categories have similar SLP minima. In contrast, in our case, the wintertime cold-core and warm-core cyclones have very different SLP minima.

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

Here is a study of two subsets with similar intensity in DJF. Cyclones that have a SLP min between 980 and 1,000 hPa were selected among warm-core and cold-core cyclones to build these two subsets.

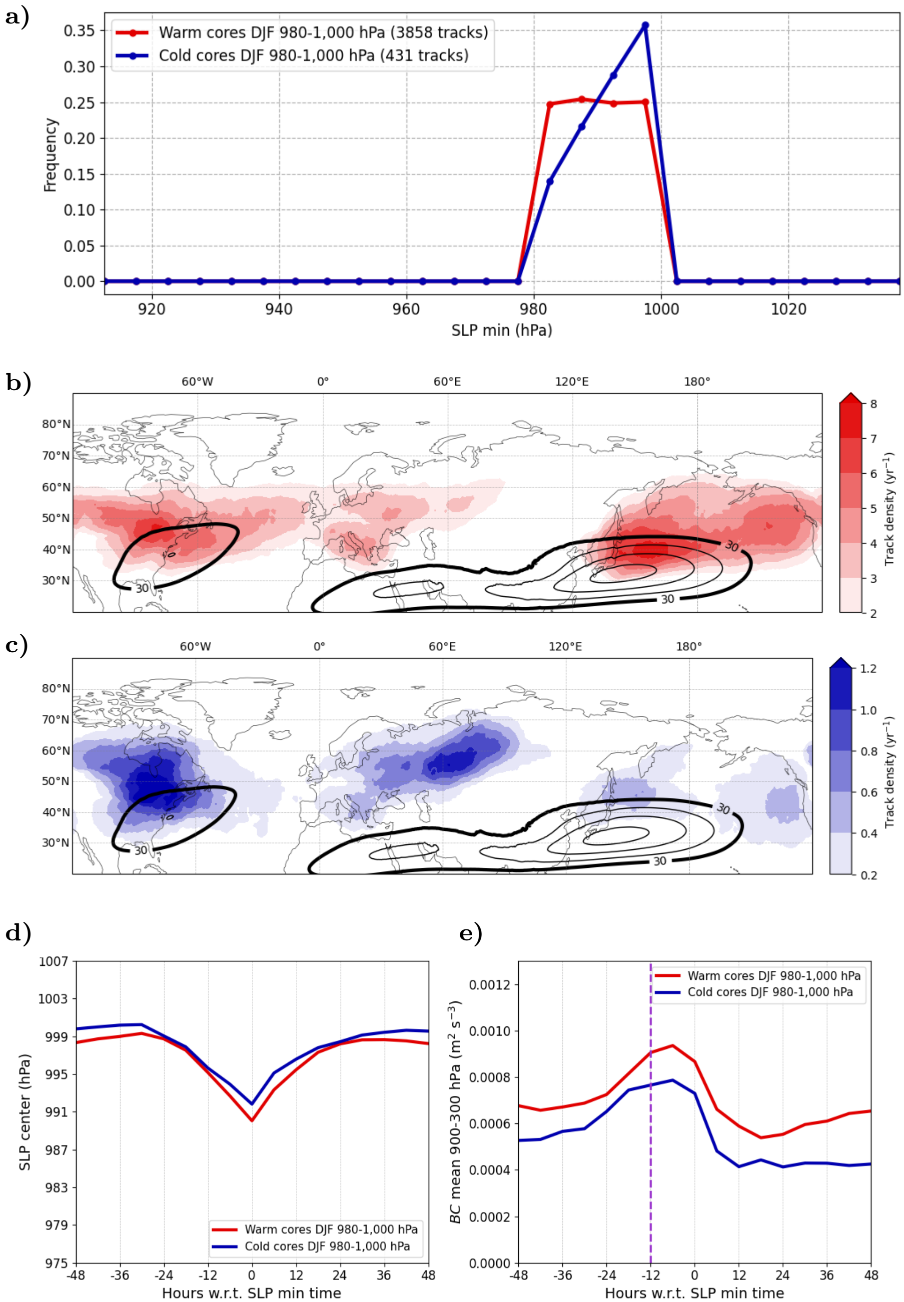

Figure R1. **(a)** Probability density function of SLP minimum for the two DJF subsets. Track density of the **(b)** warm-core and **(c)** cold-core DJF subsets of cyclones. Lagged composites of **(d)** SLP of the center and **(e)** mean BC on a 10°-radius circle and on the layers between 900 and 300 hPa. Red color corresponds to the warm-core subset, blue to the cold-core one.

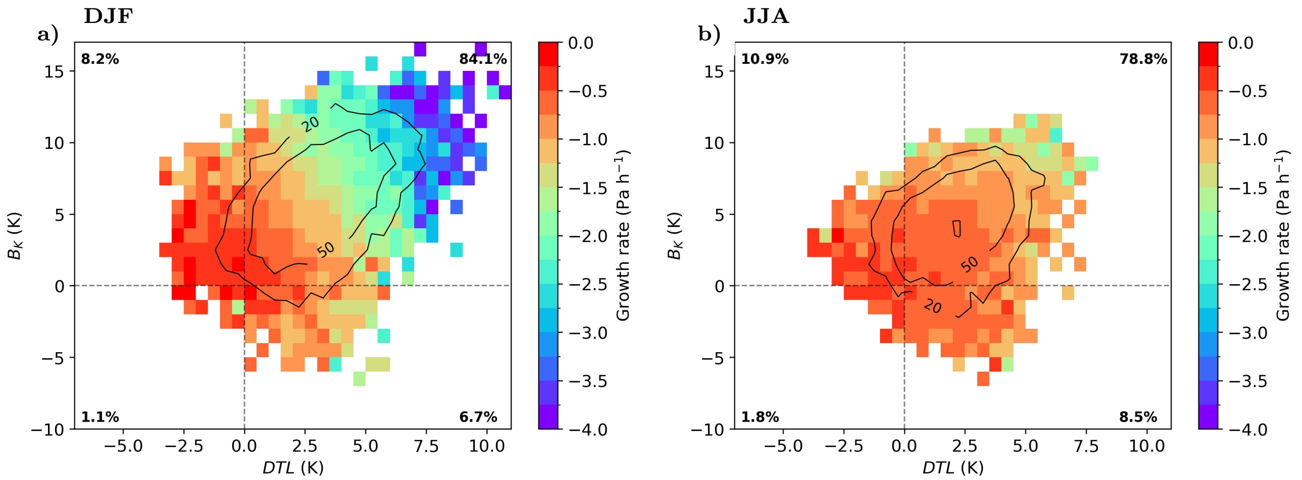

Figure R2. DTL vs. B_K diagrams for **(a)** DJF and **(b)** JJA. The diagrams are made with track points at the time of maximum growth rate only. Shading indicates the mean value of growth rate in each small rectangle (Pa h^{-1}). Black contours indicate the density of points. Four regions are built with gray lines of value 0. The percentages of data points in each region are indicated in the corners. Rectangles with less than 3 data points were removed for visualization but not in the percentages.

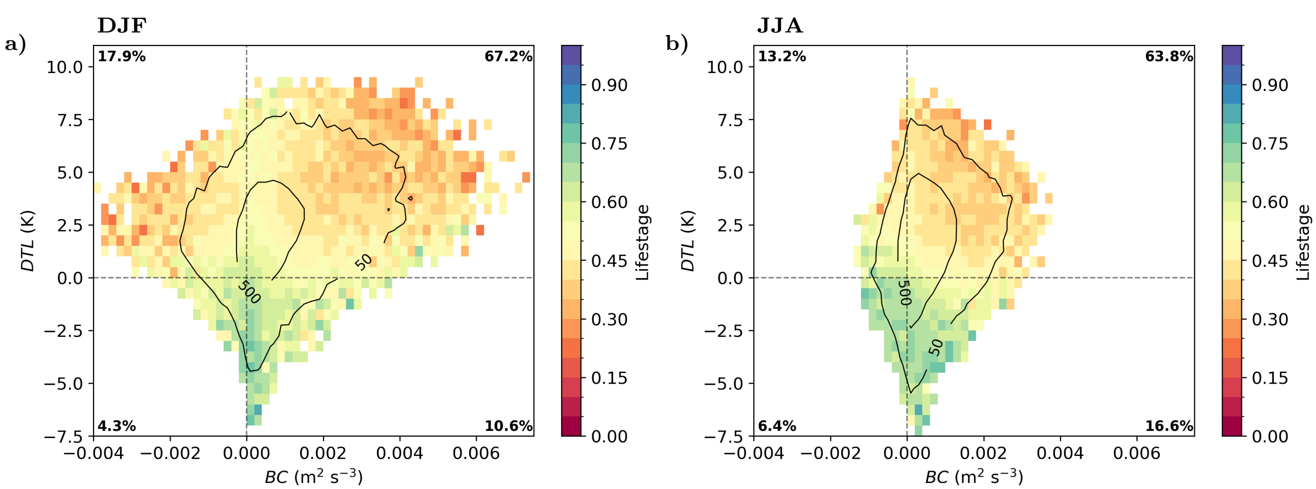

Figure R3. DTL vs. BC diagrams for **(a)** DJF and **(b)** JJA. The diagrams are made with all track points. Shading indicates the mean lifestage in each small square. Black contours indicate the density of points. Four regions are built with gray lines of value 0. The percentages of data points in each region are indicated in the corners. Rectangles with less than 10 data points were removed for visualization but not in the percentages.

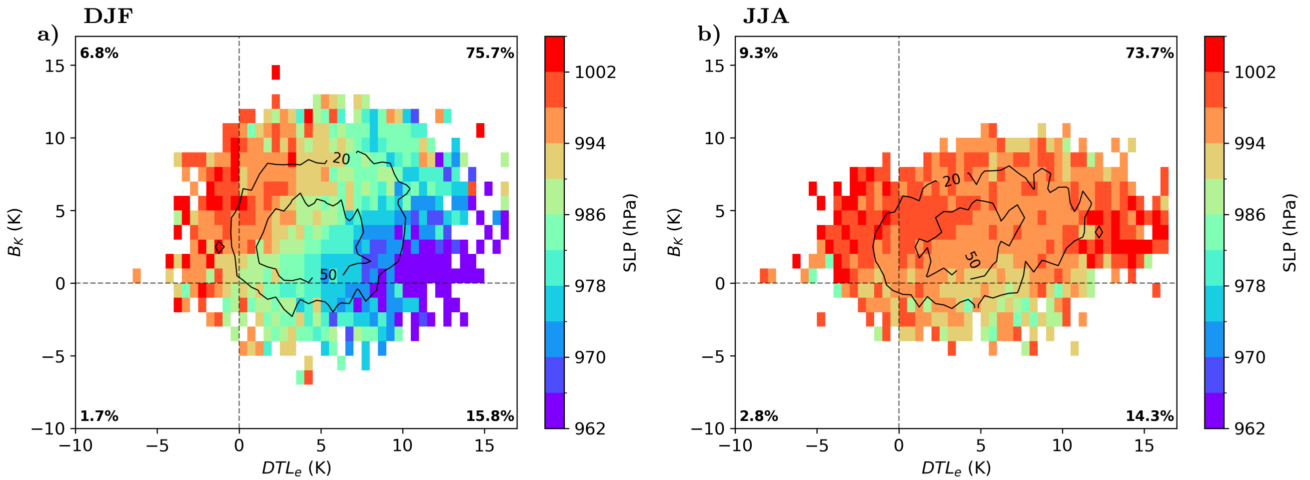

Figure R4. B_K vs. DTL_e diagrams for **(a)** DJF and **(b)** JJA made with track points at the time of SLP minimum only. Shading indicates the mean value of SLP in each small rectangle (hPa). Black contours indicate the density of points. Four regions are built with gray lines of value 0. The percentages of data points in each region are indicated in the corners. Rectangles with less than 3 data points were removed for visualization but not in the percentages.

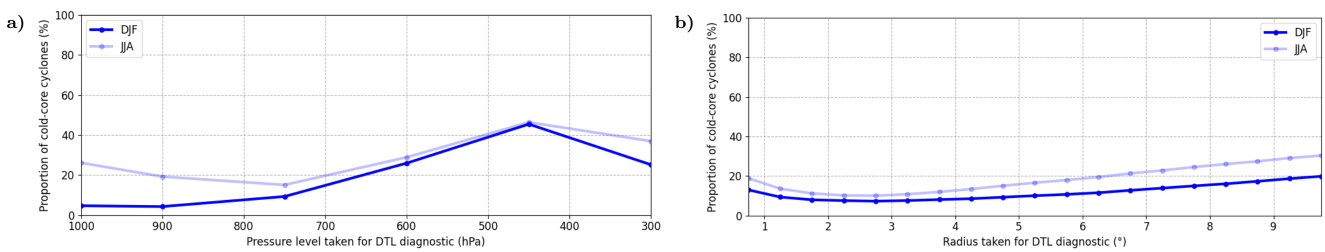

Figure R5. Percentage of cold-core cyclones identified for **(a)** different pressure levels and **(b)** different radii in the DTL definition.

To see if Mediterranean cyclones in general may have an impact, a new subset is built, excluding cyclones that are born in the Mediterranean area, i.e. between -10 and 44°E and between 29 and 48°N. The excluded cyclones are shown on the map below. Some statistics are done again with the new subset right after.

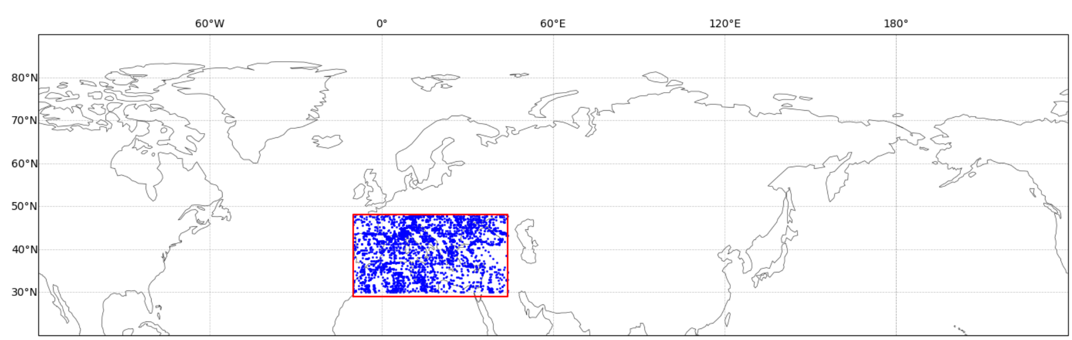

Figure R6. Genesis map of excluded cyclones (2,805 cyclones, representing 8% of the total amount of cyclones).

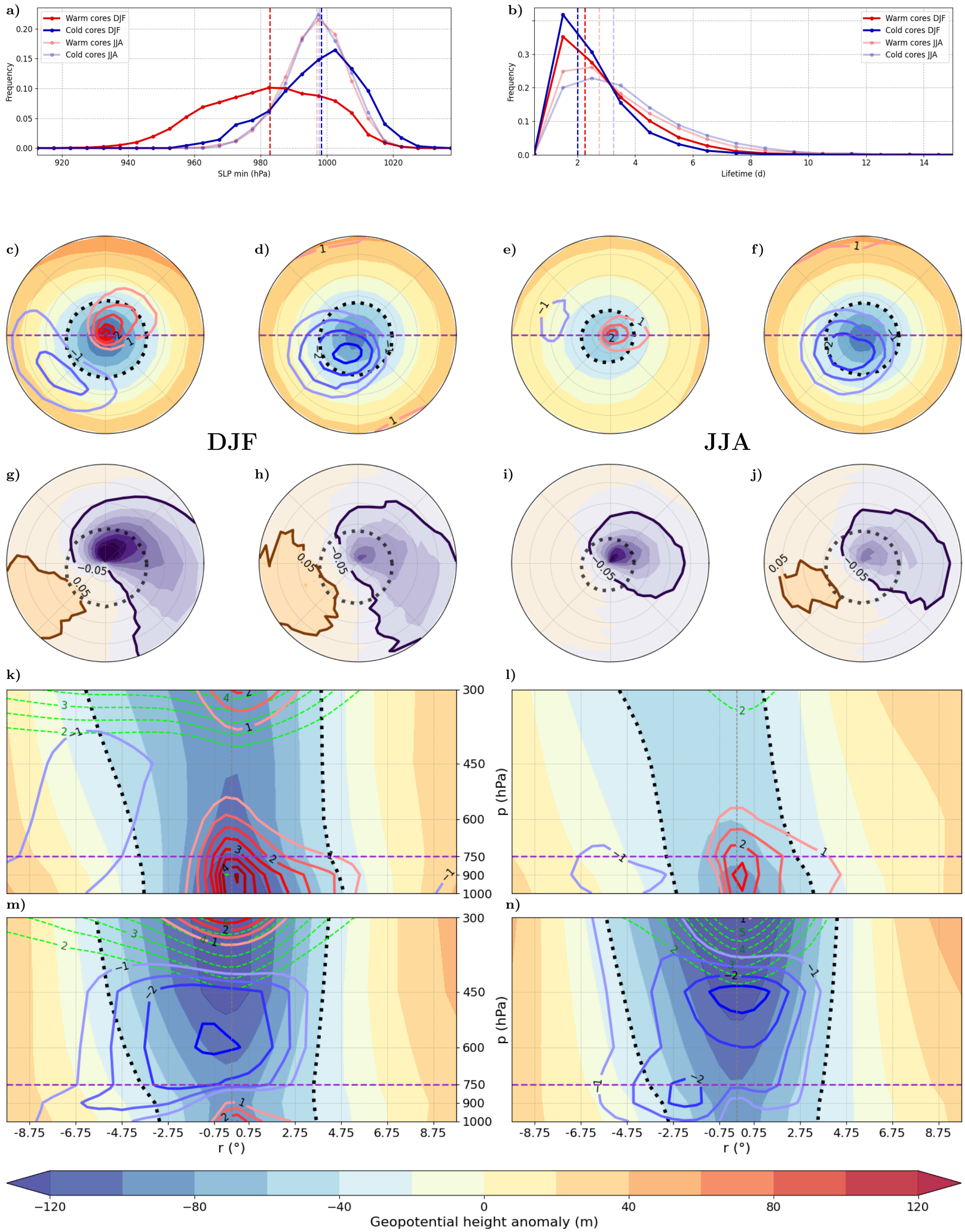

Figure R7. As Fig. 7 and Fig. 8 but for a subset without Mediterranean cyclones.

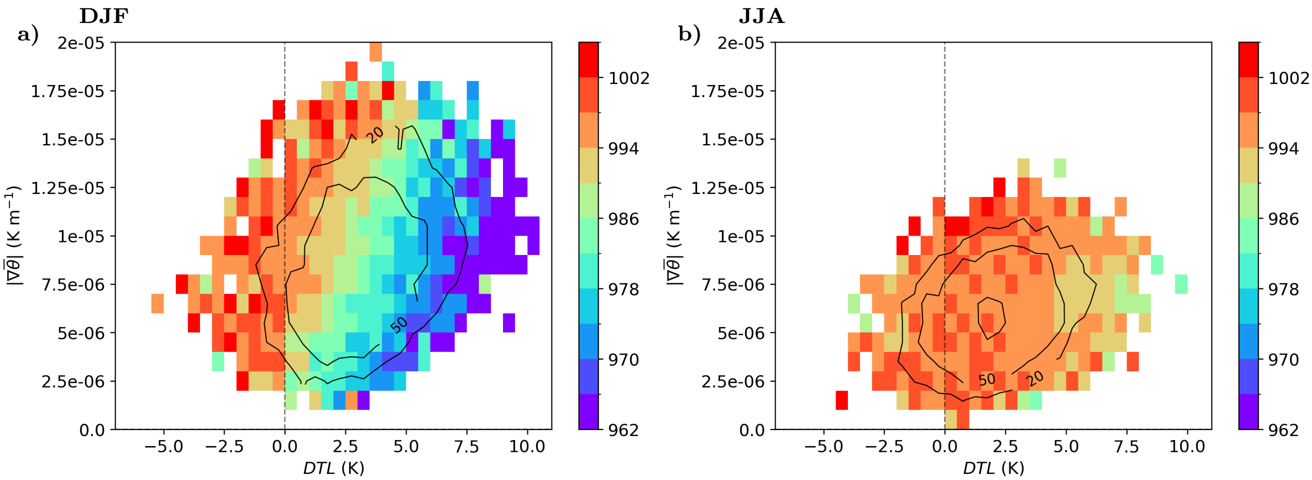

Figure R8. $|\nabla\bar{\theta}|$ vs. DTL diagrams for **(a)** DJF and **(b)** JJA made with track points at the time of SLP minimum only. Shading indicates the mean value of SLP in each small rectangle (hPa). Black contours indicate the density of points. Four regions are built with gray lines of value 0. The percentages of data points in each region are indicated in the corners. Rectangles with less than 3 data points were removed for visualization but not in the percentages.

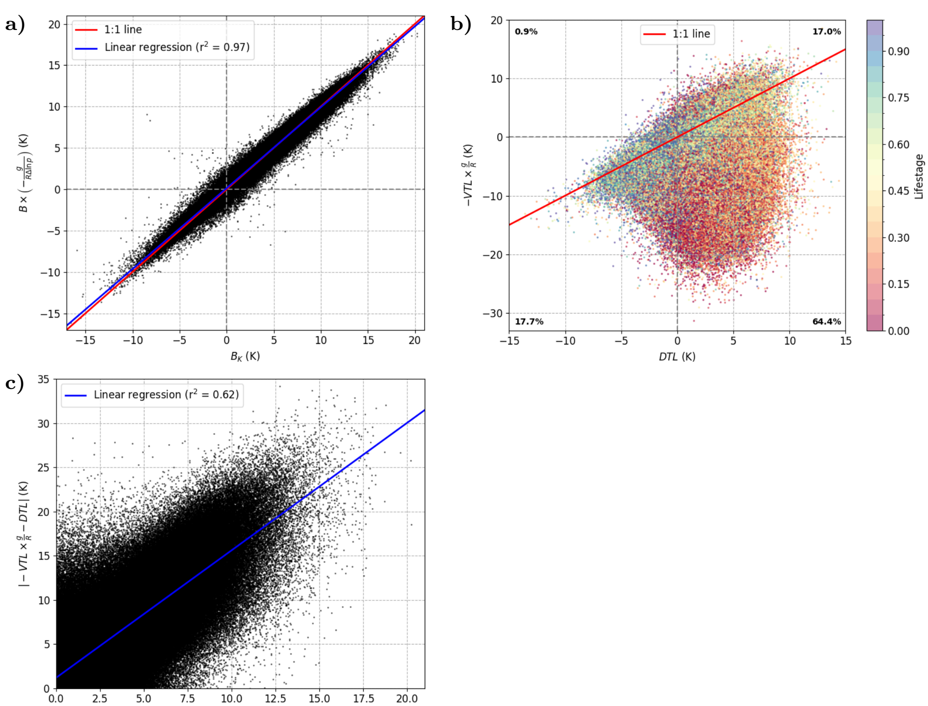

Figure R9. **(a)** Comparison between B_K and a scaled B. The linear regression is plotted in blue and the line $B = B_K$ in red. **(b)** Comparison between DTL and a scaled $-VTL$. The line $-VTL = DTL$ is plotted in red and the shading represents the lifestage. **(c)** Absolute values of differences between scaled $-VTL$ and DTL as a function of absolute values of B_K. The linear regression is plotted in blue.

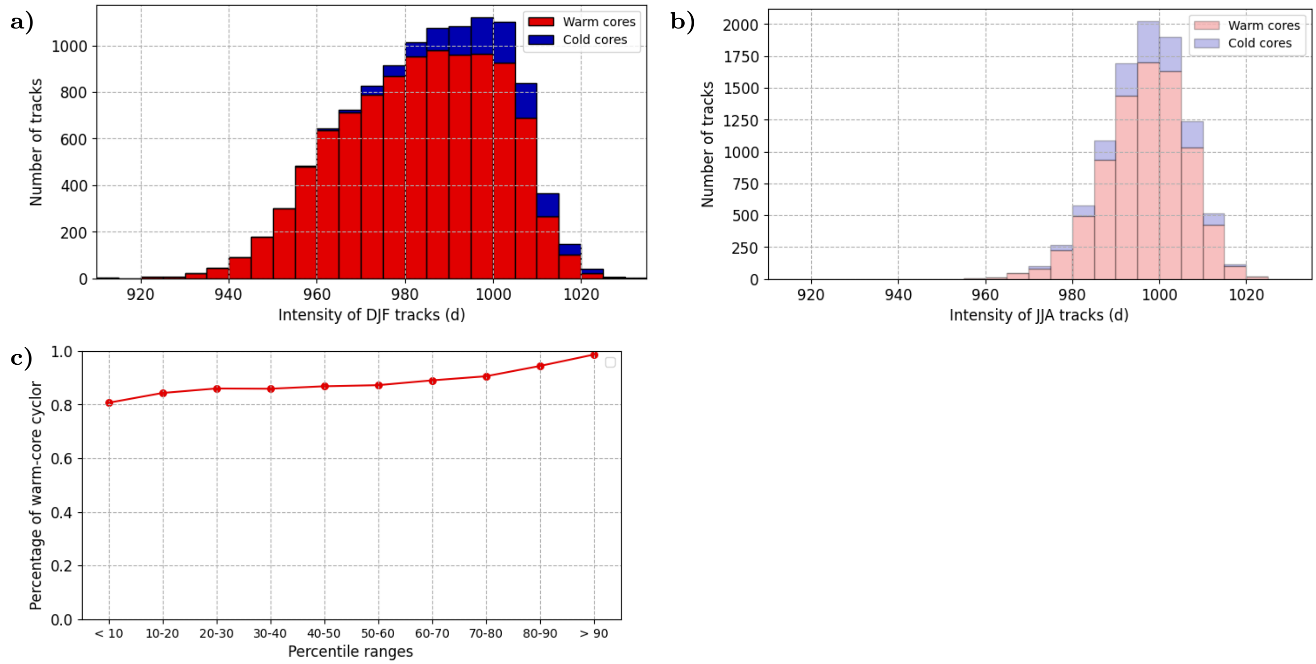

Figure R10. Intensity distributions for **(a)** DJF and **(b)** JJA. The bins are every 5 hPa. **(c)** Percentage of warm-core cyclones for all seasons as a function of the percentile ranges, 100% being the most intense track.

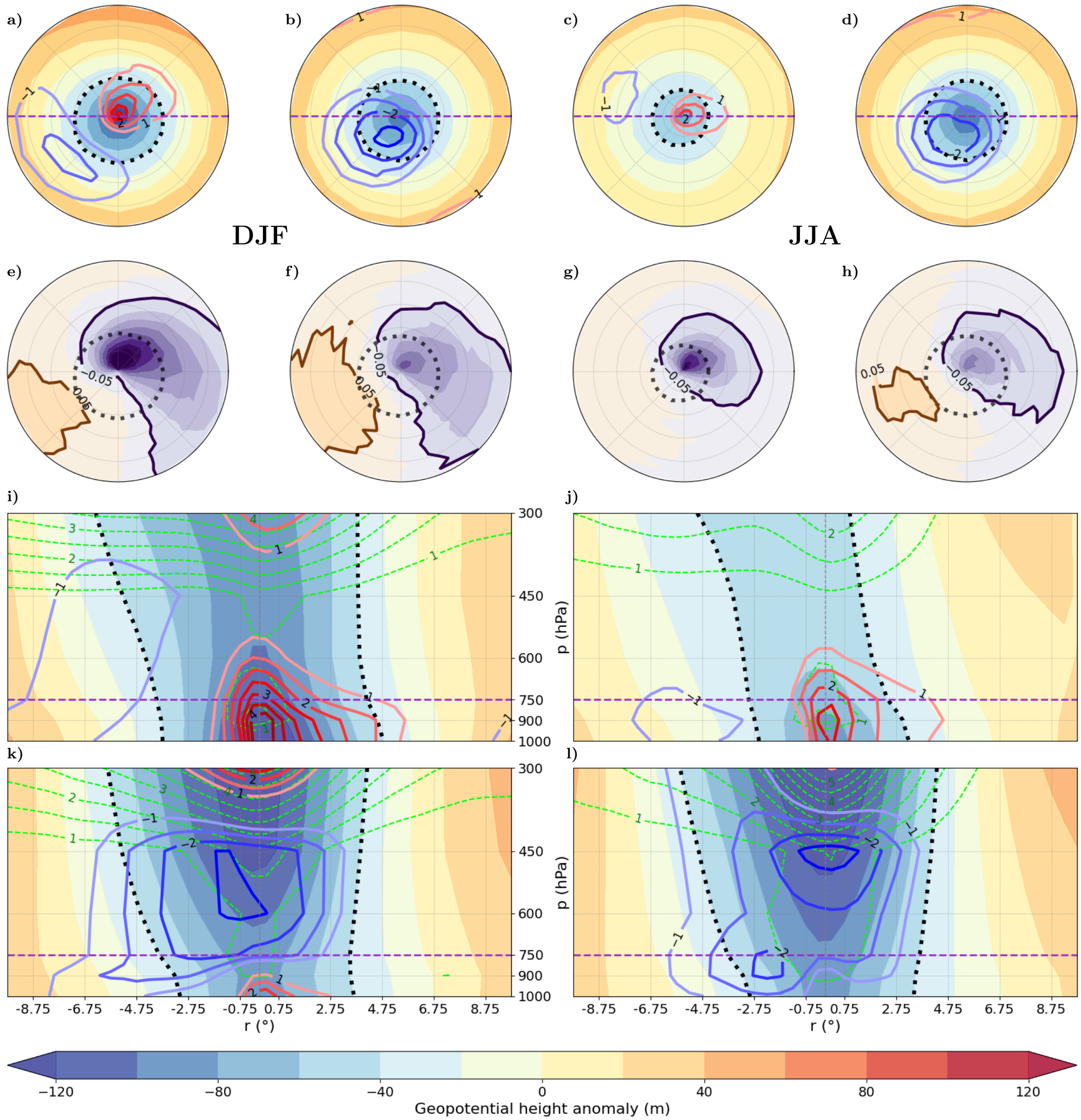

Figure R11. As Fig. 8 but with 1.5 and 1 PVU contours.

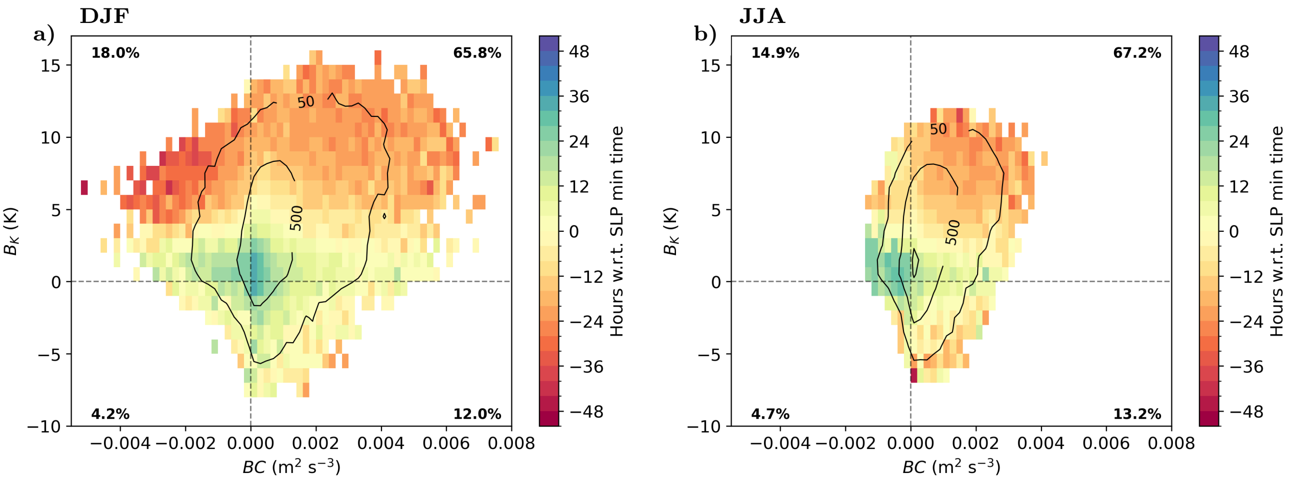

Figure R11. B_K vs. BC diagrams for **(a)** DJF and **(b)** JJA. The diagrams are made with all track points. Shading indicates the mean time to SLP min in hours in each small square. Black contours indicate the density of points. Four regions are built with gray lines of value 0. The percentages of data points in each region are indicated in the corners. Rectangles with less than 10 data points were removed for visualization but not in the percentages.