We would like to thank the referees for their thorough reviews of the manuscript. They have raised many important issues with the manuscript, covering many different aspects, and which we hope to have solved satisfactorily.

Before answering the reviewer's individual comments, we summarise here the large changes to the manuscript that are the results of one or several major points raised by a reviewer or several.

Structure:

All reviewers mentioned problems with the structure of the manuscript. The revised manuscript has a new structure where results also include discussion (instead of discussion being a separate section), and the sections are more thematic instead of grouped by methods. As an example, section 3.4 of the revised manuscript is the result and discussion for the drivers of STJ persistence, and discusses both real-space composites, jet-centred composites (fewer than in the submitted manuscript), and the breakdown of individual events. A summary and discussion of these results is contained in this same section next to the figures and references them a lot more to make the article hopefully flow a lot better. The conclusion is leaner and references figures and questions more.

Introduction

The introduction was reworked to include a more in-depth literature review of atmospheric persistence, as per a comment by reviewer 1.

Questions were rephrased and their numbers reduced to 3, as per a comment by reviewer 3.

Methods

Reviewer 1 suggested reducing the number of variables considered. In the revised version of the manuscript, we only discuss 2m temperature, precipitation, potential temperature and EKE for the STJ, and these variables plus Rossby wave breaking frequency for the EDJ.

Reviewer 1 pointed out the inconsistent aggregation periods in different analyses. This has been fixed, now all aggregations during the episode take the full episode length into account.

Reviewers 2 and 3 mentioned a lack of justification of certain choices we made in the methods. We agree with these and have made several changes to that section but also the methods themselves:

Jet tracking is now explicit rather than implicit. It is easier to justify, is closer to existing approaches such as Limbach et al. 2012, and leads to similar results as implicit tracking. Every step of the jet tracking is carefully explained and justified in the revised version of the manuscript.

The wording was also modified, where the inverse of the jet-to-jet distance is now called "slowness" and barely plays a role in the analysis, and "persistence" is the sum of the "slowness" over the duration of a jet "lifecycle".

In the list of 30 most persistent lifecycles of the STJ, we artificially removed those that correspond to the wintertime position of the STJ. This has two benefits. We can choose a more usual time period (1 June to 30 September) that requires less justification. We can also focus on the (arguably, to us) more interesting drivers of STJ persistence, hot continent and associated anticyclones separating the jet more and lessening the EDJ's influence on the STJ, as well as enhanced tropical driving, the two other "mechanisms" described in the original manuscript. We believe them to be more interesting because they are specific to summer or rather are special cases of summer lifecycles that anomalously behave more like winter than usual.

Instead of mentioning that a bias exists in the appendix, we apply a bias correction to the jet-centred composites and explain the procedure in the new Appendix A.

Because we actively work on the code on which the analyses of the manuscript are based, several bug fixes and improvements were made since the initial submission. The most notable ones are listed here, and the history of code commits is visible on the github page for the code for an exhaustive, if difficult to parse, list of changes.
- Episodes were mistakenly sub-sampled to a daily resolution before creating the jet-centred composites. This led to a much harder statistical testing and was fixed to properly use the six-hourly episodes. See the cell containing "def do_one (...)" in the notebook containing the code used to produce the figures of the original manuscript, compared to the cleaner re-implementation "create_all_relative_plots" in the package, used in the new notebook.
- Rossby wave breaking on the 320K isentrope, created using the “wavebreaking” package, had an unrealistically low climatological frequency in the northern latitudes due to an unknown bug in this package. A re-implementation of the algorithm from the description of Barnes and Hartmann 2012 using more modern tools leads to results more comparable to the results of that article.

Results and discussion

We have added lon-lat composites with relevant variables during persistent episodes, and reduced the number of variables composited in jet-centred space, to ease the readability and anchor our results with a more familiar presentation, when more adapted than jet-centred.

All reviewers mentioned the information overload and the confusing description of figures 8 and 9, the individual episodes figures. The figure was reworked. The main change is that the complicated mix of absolute mean values and anomalies was replaced with binary values for each box. Each binary value corresponds to a yes or no question, answered by comparing the mean of the row's quantity over the duration persistent

lifecycle of that column to the distribution of lifecycle-means of this quantity over all lifecycles of this same jet (STJ or EDJ). An example question is "waviness, above?". We have access to the distribution of lifecycle-mean waviness values for all summer lifecycles of the EDJ, from which we can compute a mean (mu_waviness) and a standard deviation (sigma_waviness). The question has "yes" for answer for episode 2 of the EDJ if the mean waviness over the duration of EDJ episode 2 exceeds mu_waviness by half of sigma_waviness. The figures also contain a text box explaining the groups, and a bar plot summing the number of episodes answering yes to a given question.

Reviewer 1

Review of 'Persistent episodes of the Euro-Atlantic upper-level jets in summer: precursors, maintainers and impacts.' Submitted to Weather and Climate Dynamics.
This study applies a novel, rather detailed identification method of different jet types to identify persistent (>5 day) episodes of the subtropical and eddy-driven jets STJ and EDJ, respectively. Many synoptic features such as Rossby wavebreaking, waviness, EKE, and 2-meter temperature and precipitation are considered relative to the first three days of persistent episode onset. There are some notable shifts in temperature and precipitation, and indeed hot, wet, and dry spells, associated with persistent jet episodes. However, many of the results are interpreted, though statistically insignificant. Some figures examine shifts during the first three days of persistent episode onset (Figure 4, 6, 8, 9), while Figure 5 and Table 1 examine 3-day hot/wet/dry spells during the entire episode. These two views do not always provide consistent results. Some shifts are considered geographically while others are jet-centered, which seems to provide different takeaways regarding shifts in temperature and precipitation. Statistical significance seems to less common than one would hope. I suggest reducing the number of metrics considered and focusing on one perspective – shifts during onset or shifts of spells throughout their duration.
Thank you for your time reviewing our article. We have made large changes to the manuscript thanks to your insightful comments which we believe have made it a lot stronger.
In the new version of the manuscript, the interpretation of the results has been checked again, and all mentions of insignificant results have been removed. As mentioned at the start of the document, all analyses relate to the full length of the episode and not the first three or five days as in the initial submission. We explain more clearly why certain signals must be interpreted in geographical space and others in jet-centred space, such as the precipitation signal around the EDJ, which does not appear in geographical composites because of a lack of spatial coherence, and highlight their agreement.

While the analysis presented is reliably interpreted in the text, and the discussion section is well-written, it is difficult to follow a clear picture of how the conclusions are drawn from the figures, which seem to mainly emphasize high event-to-event variability over consistent synoptic shifts. It almost leads the reader to draw the conclusion there there isn't much that is consistent across the events captured, so why are they grouped together? Are the jet events causing the associated temperature and precipitation impacts, or are they merely coincident? Though this study acknowledges both options

are possible, one does wonder why these jet events are important if there isn't a systematic lead-lag relationship with temperature / precipitation demonstrated. Do persistent jet episodes shift the impacts of focus if more than just the first three days are considered? It seems Table 1 and Figure 4 show these shifts most clearly, while Figure 5 suggests these shifts might be occurring prior to the beginning of a persistent episode. Given the need to tighten up the perspectives taken on jet-relevant shifts in weather extremes, this paper can only be accepted pending major revisions.

We have indeed tightened the discussion of the impacts section significantly thanks to your comments. The figure you are mentioning here was replaced with a simplified picture of the distribution of regional temperature and precipitation anomalies before and during persistent episodes, using box plot. It allows for a simpler discussion without the noise of intra-event evolution which is beyond the scope of the article as you rightly suggest in your first major comment. It clearly shows the lead/lag relationship of persistence with these variables, as discussed in the revised version of the manuscript, which is supported by the updated figures 7 and 10 (previously 8 and 9) that have a temporal element to them to.

We can re-write the details here if interest: Specifically, we see warm and dry Western and Northern Europe as well as wet and cold Southern Europe during persistent STJ episodes, where the dry Northern Europe signal is already visible prior to the onset, suggesting a precursor block. We also see wet and cold Eastern Europe and a cold Northern Europe during EDJ episodes, as well as a dry Southern Europe, with only the cold Eastern Europe signal being visible prior to the episode.

Other major comments

While the refined approach taken to study persistent jet episodes and their link to Euro-Atlantic summer extremes is appreciated, the introduction is often terse and lacking reference to prior literature studying persistent extratropical patterns, such as the seminal paper by Dole and Gordon 1983, among others. Since some of the introduction addresses concepts such as persistence broadly, instead of just for Euro-Atlantic summertime, the literature referenced should be much more complete and just as broad, and not limited to more recent studies.

https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2024GL109788

https://journals.ametsoc.org/view/journals/atsc/60/16/1520-0469_2003_060_1941_ptdowr_2.0.co_2.xml

https://www.science.org/doi/10.1126/sciadv.aat3272

Thank you for these suggestions. As per your and the other reviewers' comments we have thoroughly reworked the introduction to define and discuss persistence more broadly and included these references.

While the analysis examines several perspectives on the composite shifts associated with persistent STJ and EDJ episodes, revealing some interesting features, the approach would greatly benefit from some streamlining. Figures 4 and 6 consider the first three days after a persistent episode begins, which, given the emphasis on persistence (>5 days), seems a bit odd and appears to miss the impacts on hot/dry/wet spells (Table 1). Why were only the first three days considered? Why is this approach so different from the focus on spells during the entire persistent episode as discuss in Figure 5 and Table 1?

The description of Figure 6 is rather confusing as most shifts are not statistically significant, and I think there's a typo in the caption about significance which made verifying the text a bit confusing also.
We have streamlined the methods, such that all figures reflect the properties of persistent lifecycles throughout their lengths, without the (difficult to justify and ultimately useless) sub-selection to the first three or five days.
The caption and the code to create the figure was fixed, thank you for catching this problem.

Section 3.5 covering the individual event shifts is rather confusing and hard to follow, given the many partitions and jet-relative areas that are described without any spatial visuals. While I support considering event-specific shifts and the general spirit of quantifying how many cases have similar features, it's very hard to find the main message of these results and figures 8-9, particularly regarding the 'mechanism' groupings introduced.
Thanks to your and the other reviewers' comments, we have greatly simplified these figures and their discussions. For more details, please refer to the start of this response document.

Specific Comments
Line 10 'persistent episode temporal distribution' unclear what is meant, please rephrase.
We meant when persistent episodes happen throughout summer, mainly referring to the preference of STJ episodes for the start and end of our study period. This was rephrased, thank you.

Line 14 'we a significant increase' typo in abstract.
Fixed, thank you.

Line 26 'Persistence is not uniquely defined and the term is used in different contexts in large scale dynamics and extreme events research Tuel and Martius (2023)' could write (Tuel and Martius 2023) I think? At first I thought research was a verb without the () around the reference (maybe just a personal problem).
A: No, not personal, very much a typo thanks for pointing it out

Line 70 Q3 – somewhat awkwardly phrased.
A: As per your and another referee's comments the questions were rephrased.

Line 76 'We focus our analysis on the Euro-Atlantic sector and the northern hemisphere extended summer.' – since this regionality and seasonality is not mentioned in the list of research questions, I suggest moving this statement to be before the research questions so it is implicit.
A: Good point, the focus on summer is made clear earlier.

Line 78 'yet the bulk of the research on jet dynamics focuses on winter, when the jets are stronger and more separated from each other.' – it may be helpful to mention this earlier because otherwise the literature review is rather sparse. Arguably the wealth of literature

on ‘persistent flow anomalies’ going back to at least Dole and Gordon 1983 applies as persistent episodes of the jet even if the episodes are identified with geopotential height anomalies. However, as stated, past research on summer Euro-Atlantic jet episodes is less extensive.
A: It is sparse, we agree with you on this point. A general intro on persistence in any season was the aim and as you pointed out is missing a lot of important articles. It was updated to be more complete.

Line 84 – why June 15?
A: To avoid too many uninteresting early- or late-summer STJ episodes. In the revised version of the manuscript, we have chosen to use a longer period (from June 1) that is easier to justify and instead focus on the STJ episodes that have a latitude above 30°N to make sure they correspond to the summer position of the STJ.

Line 98 ‘the four weather regimes’ – these haven’t been mentioned before, a description in the introduction explaining what these are is needed.
A: This is unintended and the result of an unproperly done change in the methods. Hopefully the methods section is clear in the updated version of the manuscript.

Line 101 – the z500’ field is defined using a 15-day running mean, these fields us 10 day filter. Why are the methods different when they are all indicated with the same notation? Is there any concern regarding the mismatch between the 15-day and 10-day fields?
A: Good catch, these are two different things with the same sub-notation. The notation is wrong as you point out because Z500' is produced very differently from u' and v'. The same notation refers to an anomaly to a 15-day smoothed daily climatology, and to 10-days high-pass-filtered data, respectively. 15 days climatology smoothing is the typical smoothing used in the weather regimes literature and 10 days high-pass filter is the typical value used in eddy-mean flow interaction literature. In their respective literature, both are marked with an apostrophe and commonly referred to as "prime".

In the case of Z500' the results are largely insensitive to the choice of smoothing window, in the case of u' the choice of frequency cutoff will alter the magnitude of the response but typically not its qualitative nature (internal testing between 5 and 30 days for the results involving EMF convergence and EKE). u' and v' are now u^e and v^e, where 'e' stands for 'eddy term' to distinguish them from anomalies.

Line 109 - since this method is described in detail in the 2025 paper, I suggest mentioning that fact earlier than line 114 so that the reader knows this is a quick summary of prior published methods.
A: This section was reworked for the better taking this and another referee’s comment into account.

Line 136 “The persistence metric shows a similar distribution for both jets, with a slight shift to higher persistence values for the STJ. The EDJ has more longer episodes then the STJ although the longest episode, lasting 56 days, is of the STJ.” If EDJ has longer episodes how does that not correspond to a higher persistence metric? Some interpretation of what the persistence metric means, given this counterintuitive comparison, is needed.

A: A sentence was added to this paragraph to hopefully clear the confusion. As an example of how this result might appear, slowness might be high on average (small distance between jets) but very variable. Then, short periods (< 5 days) of high slowness might occur often but longer ones will occur more rarely since a spike in distance (a dip in slowness) will break a persistent episode.

Line 171 – by 7 August not 4 August right?
A: Probably a typo. This whole section was re-written.

Line 184 – heating… would add this is reflected by the heavy precipitation located on the anticyclonic shear side of the entrance region since that's the only evidence in the figure.
A: That is correct.

Line 187 'Selecting periods only by jet persistence has highlighted two interesting situations where jet dynamics and extreme events were connected' – well at this point in the results, there isn't any evidence it was specifically jet dynamics that produced the extremes in these two cases, there's simply a coincidence of features. I suggest removing this sentence. A robust link between jet dynamics and extremes is the hypothesis that has yet to be tested.
A: It was removed. Instead of extensively interpreting the results of the case study we use it as an example in the later sections when discussing the more robust signals from many episodes. Thank you for your comment

Line 191 'slightly more tilted' – I see mainly indistinguishable tilt differences at almost all lags. For the EDJ events there are more meaningful shifts it seems.
Discussion of tilt was not interesting and was removed.

Line 196 'persistent in average windspeed' – well, the shift isn't even outside the 90th percentile confidence range, so there's limited evidence for this being significant.
A: The figure was maybe not super clear but the signal was far outside the confidence range (the pink line and its confidence range compared to the pink dashed line in figure 3f in the first submission). The figure was updated and is now hopefully easier to read.

Figure 5 – it doesn't seem that the European heat wave regions necessarily align with the significant shifts associated with the persistent jet events. Particularly the application to precipitation which is only significant in the UK for EDJ events and at no other times. If kept in the results, the statistical significance needs to be tested for the temperature and precipitation shifts. I realize that makes the figure less interpretable if all 5 regions are compared, though again, considering shifts in all of these five regions seems minimally justified based on Figure 4.
A: The updated version of figure 5 is simplified to only two temporal points (before and during) and includes significance testing.
A set of regions created as in Pappert et al. 2025 but for precipitation is completely doable but leads to a lot more regions for the same amount of inter-region coherence (internal testing and hinted at by Tuel and Martius 2022).
In the updated figure, keeping the same regions, there is a similar number of significant shifts in precipitation as in temperature before and during persistent jet episodes.

Furthermore, the events we are looking at should be larger-scale than average because driven by synoptic-scale atmospheric persistence, so the rather large heat wave regions seem adapted.

Table 1 – this considers spells of hot/wet/dry extremes *during* persistent jet episodes, while Figure 5 considers a [weakly justified] lead-lag relationship. I suggest focusing on the Table 1 results which seem more salient than the lead/lag evolution in Figure 5. As Figure 4 only considers the figure three days after a persistent episode begins, there seems to be a discrepancy between it and Table 1 that would benefit from better alignment (example: no precip signal in west/east regions in Figure 4d but one suggested from Table 1). Can a spatial representation of the hot/wet/dry spell results in Table 1 be presented?
A: We have simplified figure 5 such that it is more interesting to discuss and give it about as much importance as table 1. We could not find a good and digestible way to represent the results of table 1 spatially, however.

Figure 6 – grey hatching is *insignificant* in this plot? With 90 or 95% confidence (both have been used in prior plots). I would've though the hatching showed significant points, not insignificant? The text about AWB and CWB is confusing, as both look either significant or insignificant depending on if the caption has a typo.
A: Caption is a typo, also there was an error in the code there.

Line 276 'On the cold flank…' – Are these statistically significant? The offset between the positive/negative PV anomalies and the jet core is really confusing – I appreciate this is acknowledged but it's pretty glaring here.
This type of discrepancy in jet-centred composites came from two places. One of them was a proper technical bias that is extensively discussed in appendix A2 and systematically fixed. The other one, a misalignment between the levels, is expected and still present. Bias correction and a more judicious choice of vertical levels have removed these confusing artifacts.

289 – 297: it's hard to follow how the choices here reflect the prior results or are motivated specifically by jet/extreme linkages. Can they be linked to what we see in prior figures?
In the updated version of the manuscripts, the choices are all motivated by literature and a geographical composite that eases the reader in the jet-centred composites.

Figures 8-9 It is quite difficult to digest the information in these figures, for me. But as a reference for those interested in particular cases, they may be of use (assuming there are statistically significant shifts..). Were the weather regimes shown in the top row ever defined in the text? From Figure 9 caption " Orange: lower-lower, magenta: higher-lower, green: lower-higher, pink: higher-higher." This is very confusing.
A: As described in details at the start of this document, we have greatly improved this figure thanks to your comments and those of other referees.

Line 365 – I'm not convinced that it's a mechanism to just be starting out southward shifted at the beginning of the summer period. A mechanism to me is something like

maintenance due to nonlinear PV flux convergence due to a certain PV configuration, for instance a series of positively-tilted waves north of the jet axis.
A: We agree, we removed these episodes artificially and justify our choice in the relevant methods section and at the start of the document.

Discussion – which figures are used to support which statements would greatly help the reader follow the arguments presented.
A: You are correct, this is now done.

Reviewer 2:

The authors introduce an analysis of persistent jet periods for both the eddy driven and subtropical jet over the European region. They illustrate the environmental conditions leading to as well as during these episodes. The authors make inferences about potential dynamical reasons for these persistent events. While the results are interesting, the presentation is not easily accessible for the reader, as the text is somewhat repetitive due to the chosen structure and because the authors sometimes speculate without further substantiating their claims. The manuscript might be publishable after major revisions are taken into account.
We thank the reviewer for their comments which have helped make the manuscript a lot stronger and easier to read. The manuscript was re-structured, weaving discussions into the results section, which was also reordered, by theme rather than method. Conclusions we are sure of and speculations are better separated in the discussions and the conclusions.

The authors should provide a more detailed motivation for the presented analysis. What is the relevance of these persistent jet periods? What is the purpose of identifying these jet periods? What motivates to look at these periods from a jet perspective? Where does this question emerge with respect to the jet and not with respect to surface weather or other previously used features in the context of persistent or extreme weather conditions. It also needs to be clarified if the jet is just a feature that is part of a larger scale persistent weather pattern or if the jet is regarded as a driver. In the context of the latter, the authors often argue about mechanisms, though their analysis does not go beyond a synoptic composite of these persistent jet periods. Hence, it is difficult to see how the analysis provides any conclusive findings with respect to the proposed mechanisms. The authors should more clearly distinguish between hypotheses, speculation, synoptic composites, and a dynamic analysis of actual mechanisms.
The introduction was reworked to, among others, better justify the use of our methods. A large body of literature relates extreme events of many different types to the maintenance of the jet in an anomalous configuration. This naturally leads to the study of jet persistence in both its position and its shape. So far, people have only looked at jet position persistence and not shape, partly (we think) because it is much easier to diagnose. Thanks to algorithms such as Spensberger et al. 2017, we have access to the instantaneous shape of the jet. We can therefore now holistically study jet persistence and see if the links between jet persistence and extreme events, so far diagnosed only with jet location persistence, still hold up with shape persistence also considered.

The discussion was heavily reworked to better justify our interpretations of the results. We systematically look at all the signals before and during the episode to get a sense of causality, as also suggested by reviewer 3, and more carefully link what we see with the literature.
The rest of this response focuses on this specific point: "It also needs to be clarified if jet is just a feature that is part of a larger scale persistent weather pattern or if the jet is regarded as a driver".
It's a very difficult question in general, because all aspects of the circulation obviously interact with each other. A blocking event has a persistent anticyclone, cyclones around, and a jet weaving around those, but the jet has advected this anticyclone to where it now stalls, and feeds it negative PV, such that this whole system is one big feedback loop.
Because of the large body of literature linking jet to extremes, we choose to put on the lens of jet persistence. With this lens, we end up finding a wide variety of event types, from blocks and very wavy jets to zonal jets driven by double wave breaking, that are two very different persistent weather patterns in which the jet is a central feature. The strength of our approach is that we didn't have to limit ourselves to a finite set of weather patterns using, for example, clustering. Our persistent events sometimes resemble known weather patterns and oftentimes not.
In summary, the jet is always embedded in a larger pattern, it is the salient feature that allows us to find all these different persistent patterns, and the jet maintains the pattern and/or is the cause of the impacts because it advects smaller weather systems and moisture.

The structure of the entire manuscript would benefit from weaving the discussion more directly into the results section, because there are many facts, hypotheses, and speculations put forward in the results section, though the reader must memorize all of them until the discussion section. Given the number of facts, hypotheses, and speculations put forward, this is a rather impossible task, which makes the manuscript a difficult read and often leaves the reader confused. Just to give one example: RWB is introduced at some stage, then a hypothesis is put forward in one section, in the next section a negative result is presented with respect to this hypothesis (not much of a signal and not significant), but it is not discussed further or put in context with the original hypothesis. Then, RWB is discussed in the discussion section again, here in an affirmative way with respect to the hypothesis, followed by a concluding remark in the summary. This split of presentation and discussion makes it a bit cumbersome to parse the manuscript and to ensure that one takes the essential messages from it.
We thank the reviewer for this comment which has made the manuscript a lot better. The discussions are now weaved into the results and the whole manuscript has been thoroughly reworked to flow better.

The very form of separating results and discussion also leads to quite some repetition, which also reduces the reading efficiency and pleasure of the manuscript. Furthermore, the discussion reads more like a summary, not like a discussion. In addition, the discussion contains several speculations that are not further substantiated, such as local convection potentially feeding the momentum of the regional STJ, even though no link to the MJO has been found. There have been recent publications addressing the asymmetry of the Hadley cell, which could be put in context here (e.g., publications by

Schwendike and Reeder). The third mechanism and its link to the first mechanism also seems rather speculative.

We have Removed all mentions of MJO as we could not find any interesting signal about it, and it is not clear how valid it is for the region and the season. As shown in the article that you mention (I hope; Schwendike et al. 2021), no phase of the MJO is associated with significant anomalies in wind speed in the region of the African STJ in summer. But MJO is only the main (two) mode(s) of variability of tropical precipitation, such that no link to it does not necessarily mean no link between tropical precipitation and STJ speed, since Atlantic and African precipitation are far from the largest signals in MJO, and they are probably the most important regions for our local STJ. We now diagnose precipitation with lon-lat boxes and in a jet-centred box and find a solid link between it and STJ persistence for half of the episodes, and a weaker on in the mean.

Recognising that the momentum of the STJ is not sourced locally (Martius 2014), we added a discussion of tropical precipitation in the other basins as well but only found small patches of significantly positive precipitation anomalies there. We have therefore reworked our conclusions with regards to STJ persistence. The main driver of STJ persistence is a more winter-like Hadley cell. The Hadley cell is bounded by eddy activity (Walker and Schneider 2006, Schneider and Bordoni 2008) and because in the summer it is shifted poleward closer to the baroclinic eddies/the storm track than in winter, the eddies are able to tap more momentum from it. In most persistent STJ episodes, the storm track is poleward shifted so the Hadley cell is stronger and can expand further poleward compared to its normal summertime extent. Therefore, more momentum feeds the STJ. On top of that the eddies are less able to deform the STJ, which also helps the persistence. Additional tropical precipitation is another factor that explains a stronger Hadley cell but is only measured in half the episodes.

If the authors decide to split the manuscript into these specific sections (results, discussion, summary), the content should adhere to the titles of the respective section. Alternatively, which would also aid the readability of the manuscript, the discussion could be moved directly to where the respective results are discussed. Furthermore, the discussion is repetitive, as the three mechanisms are iterated each at least twice. This also unnecessarily confuses the reader. Please try to structure your discussion more linearly or integrate it entirely into the results while keeping the summary on point.

Sections 3.5.1 and 3.5.2 read like enumerations with many disjoint facts. It would be helpful for the reader if the authors put the different findings into better context with each other, otherwise these sections leave the reader rather confused. The suggested inclusion of discussions with the results could be a way to alleviate these shortcomings. Several of the variables used for the analysis are inter-dependent, such as theta and PV, but also temperature anomaly and position deviation, where a south/north shift translates into a temperature anomaly. Hence, it is not clear how the choice of these variables and the choice of the method at hand help to disentangle the role of the jet versus everything being part of the same weather pattern. The authors need to more carefully discuss these inter-dependencies and their implications for their analysis. After taking into account these potential inter-dependencies, what would be a net signal and what could be said about its potential origins and causes?

Thanks to a re organisation of the article prompted by your comments, the discussion of the individual events flows better with the rest of the manuscripts. More specifically, every row is justified and interpreted in the broader scope of the discussion on this jet's persistence drivers.
With a reduction in the number of variables shown in the figures overall, we have reduced the number of inter-dependencies to focus on more independent signals.
We have reframed a lot of the discussion as feedback loops, trying to find from what "end" of the loop persistence typically starts. CWB <-> equatorward shift for the EDJ starts more often with CWB, while for this same jet the loop warm flank precip <-> acceleration starts more often with acceleration. The rest of the EDJ episodes are part of blocks which pre-date the event or driven by double wave breaking also pre-dating the event. For the STJ, the favourable EDJ conditions almost always pre-date the onset of persistent STJ episodes, and often so do blocks. Tropical precip is intense before and during ~halfof the STJ episodes.

Specific Comments:
Line numbers refer to the ones on the manuscript.
L14: Grammar off in sentence beginning "We…"
Fixed
L17-21: First sentence difficult to parse. Consider splitting, as it contains two main clauses.
Reworked the abstract
L27: Check style of citation.
Fixed
L36-54: The paragraph is difficult to parse and includes several topics. Please rephrase and split into at least two paragraphs with logical units to follow the arguments more easily. Also, please further clarify the different strands or arguments related to the momentum fluxes. Momentum flux convergence, baroclinic lifecycles, and Rossby wave breaking are all mentioned, though it remains difficult to understand the implied linkages by the authors.
The introduction was entirely re-written and spends more time explaining the role of all of these concepts and their relationships.
L55-64: Persistence of different phenomena seems to be addressed in this paragraph, which leaves the reader a bit confused. The connection to the overarching research topic of persistent jets needs to be more clearly developed.
The introduction was entirely re-written and more clearly defines and discusses persistence as a whole before focusing on jet feature persistence.
L65-74: The research questions are all interesting, but they need to be more clearly motivated by the previous paragraphs. In a way, there should be a paragraph motivating each of these research questions. This link needs to be developed more clearly for the reader to be able to follow the origin of these research questions and their relevance.
The number of questions was reduced and they now more clearly link to the discussion points of the introduction.
L84-85: What is the motivation for the chosen time of year, i.e., 15 June until 31 September. This has not been motivated anywhere in the introduction, nor is this choice further clarified in the data section.

This choice was made to avoid too many uninteresting early- or late-summer STJ episodes, which ultimately failed in the submitted version of the manuscript since the majority of STJ episodes were still winter types. In the revised version of the manuscript, we have chosen to use a longer period (from June 1) that is easier to justify and instead focus on the STJ episodes that have a latitude above 30°N to make sure they correspond to the summer position of the STJ, highlighting more of the interesting dynamics such as jet-jet interactions.

L97: Clarify how the momentum flux is used to assign the type of RWB. If this is based on previous work, it should be cited.

In the revised version of the manuscript, we use the definition of Barnes and Hartmann 2012 as previously, and their assignment of RWB type based on the geometry of the overturning, comparing the latitudes of the westmost and eastmost points of the overturning.

L105-108: The method of detecting the jet core as a line appears rather similar to Spensberger et al. (2017). In general, the authors should put their detection in context of previously introduced jet detections and clarify why their specific choice is superior to already existing and more commonly used jet detection schemes. In other words, why does the world need yet another jet detection scheme?

We now more clearly state that our algorithm is really a rewrite of Spensberger et al. 2017 with minor modifications, not a whole new detection scheme, which was clearly stated in the 2025 article presenting it.

L119-122: The explanation of the methodology for connecting jets over time is insufficient. The authors need to provide more details about the method and provide a justification for your choices. Just being “technically simpler” is not a valid justification and it needs to be clarified what the authors mean by “more stable”.

As stated at the start of the document, thanks to your comments the methods were updated to be more solid and justifiable. The methods section you are mentioning was therefore updated to present the new version of the methods (explicit tracking instead of implicit) and justify each step.

L126: Is this really a persistence metric or rather just a time-weighted distance metric? Persistence would imply a coherence for several time steps, not the distance of two objects for one weighted time step.

You are correct, naming this metric “persistence” was a bad choice. It is really just the inverse of a distance. We have now renamed this “slowness”, not a great name but fortunately it does not play a large role, and the sum of this "slowness" over an entire jet lifecycle is what we now call “persistence”, which better fits intuitive definitions of these terms.

127: What were these “several alternative distance definitions” that were tested? Please provide more details and reasoning for your choice of metric as well as a discussion of its advantageous and caveats.

Part of the rewrite now includes a more thorough justification of the choice of distance metric, comparing it against another well known distance metric between curves, the Hausdorff distance. Internally we tested other “home-made” distance metrics such as the area between jet cores, but since these are not standard or documented we chose not to mention them.

L131-133: Please justify your choices in more detail and discuss the sensitivities of the results to your choices.

Part of the rewrite now includes a discussion of the choice of thresholds.

L135-136: What are the implications of this statement about STJ and EDJ?

No specific implications for the EDJ, and for the STJ it was thoroughly discussed later in the article. It was a bad place for these results and the discussion for the new version of this same figure now interprets these seasonal inhomogeneities.

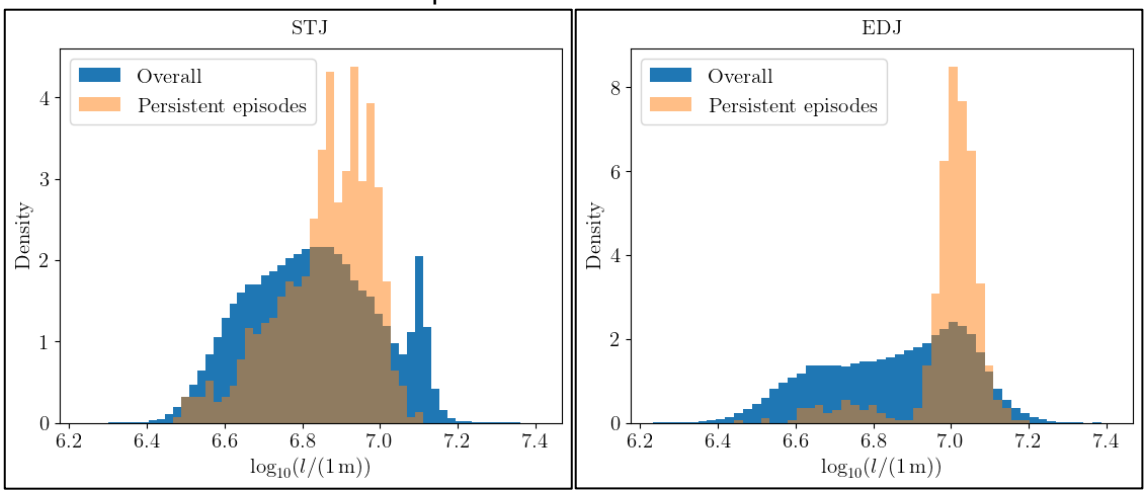
L136-138: This seems to be a discussion of results, though no figure reference or reference to other literature is provided to back up the quantitative statements made.

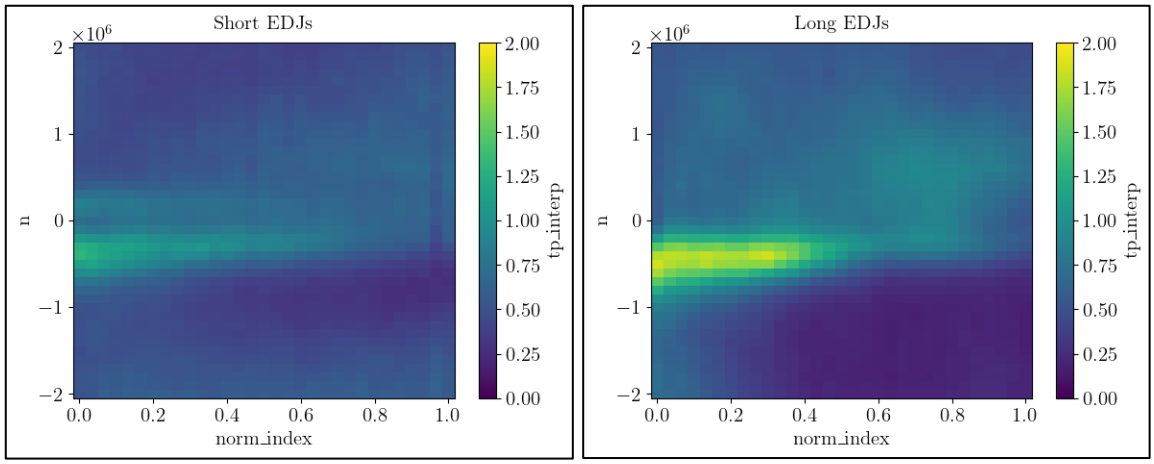
This is correct, we have now dedicated a results section to the updated version of this figure, comparing our findings to the literature when applicable.

L150: The authors need to discuss the sensitivity of their choice to project all jet lengths onto a [0,1] length. One wonders about the sensitivity of their findings to jet length, i.e., what is the variability in their analysis and how does it project on different jet lengths.

This is a though point to fully explore, and we try to answer some of it here. Firstly, the lengths of the jets vary by an order of magnitude in our dataset, but the distribution during persistent summer episodes is narrower, especially for the EDJ, as shown on the two histogram plots here. The most important results with jet-centred composites, the anomalies during persistent episodes, are therefore less affected by length variability.

To get a quantitative idea of the effect of jet length on the jet-centred results overall, we compute the summer mean climatology of EDJ-centred precipitation for the 20% shortest EDJs and the 20% longest EDJs (just above 7400 jets each). The results are shown in the next two plots, showing the same qualitative patterns of precipitation but with a factor ~2 in maximum intensity. There is therefore an effect in jet length; the shortest jets probably do not have their entrance regions over the gulf stream region as often as the long ones which most often span the whole domain. We mention this in the revised version of the manuscript.





L168: “the height of the”

Fixed, thank you

Figure 3: The figure caption does not explain the difference between the purple and pink lines and shading. It is thus not clear what is shown in this figure. One can assume that the darker color corresponds to the EDJ and the lighter to the STJ, but this needs to be clarified.

Fixed, thank you

L207: What is the definition of “sensible weather”? A literature search could also not clarify the origin of this term. Please consider rephrasing.

Reworded to “weather”

L208: Statement needs backing by references.

Added a few, we wanted to avoid repeating citations from the intro which, especially in the revised version of the manuscript, makes this link clear from a large body of literature.

L221: “precipitation”

Fixed, thank you

L397-399: These statements read rather speculative, see also general comment.

L404-409: These sentences are rather speculative, and it is difficult for the reader to understand what to take away from this discussion. Please stay closer to your own results and discuss those for the benefit to help the reader to interpret your findings. The amount of speculations makes it difficult to disentangle what was actually discovered in this manuscript and what the key messages are.

Two the last two points: the whole discussion section was reworked and now clearly justifies claims from significant results in the figures and / or literature. Speculation is rare and clearly signalled.

L446: The statement that the authors “investigate the mechanisms” is not quite correct. The authors provide a synoptic presentation of various variables around persistent jet conditions. “Mechanisms” implies that the authors provided a causal analysis of what lead and maintained these persistent jet regimes, though such an analysis has not be provided.

We have reworded the discussion to be more conservative about our statements and included more time-lag analyses to make a further step towards causality.

L462: What are “event odds”?

Odds of extreme events. Reworded, thank you.

L468: “when it is persistent”

Reworded, thank you.
L482-483: The statement in this sentence needs to be backed up by literature.
We now repeat the citations to Shutts et al. 1983 and to Yamazaki and Itoh 2013.

Reviewer 3:

The underlying ideas regarding Euro-Atlantic jet persistence are interesting, but the manuscript needs substantial restructuring. In its current form, the study is weakened by imprecise language, motivations that are not always clear, and somewhat overstated goals, as several claims appear broader than the analysis can support. The figures are often visually dense and read as an extensive compilation of results rather than a curated selection intended to support a focused narrative. The reader is not kept in mind enough throughout the study, which would be substantially stronger if it were more concise.

Given the substantial comments I provide below, a resubmission may serve the manuscript better than a major revision

We thank the reviewer for their work and their comments. As stated at the start of this document, the manuscript was extensively reworked to take into account the comments of all three reviewers. Addressing the many insightful points raised here have made the manuscript a lot stronger and for that we are grateful to the reviewers.

Major comments:
- Statistical aggregates are construed as physical mechanisms. The manuscript sets out to elucidate the mechanisms responsible for jet persistence, and while the compositing results provide context to understand environmental conditions associated with some jet configurations' persistence, I do not think that this context alone provides a generalized mechanistic understanding. For the STJ persistence, the first mechanism (see L365) consists of an initial condition for the STJ that is weakly linked to references but not to the results in the analysis. It remains to be understood why the STJ would be at its winter position at the onset of a summertime persistent spell. The second mechanism (L371) and third mechanism (L377) are similarly based on co-occurrence of conditions from a small sample at the extreme end of the persistence metric distribution defined in the study, and they do not demonstrate causality. Case in point, only weak links were found between organized tropical convection and jet persistence. The same points apply to the EDJ persistence mechanisms, which are in fact formulated as hypotheses in places. It would be informative to composite events not solely based on persistence, but also based on the composited favorable environmental conditions, to establish whether there is a two-way correspondence with persistence. In light of these comments, the title of the manuscript does not faithfully reflect the analysis and should be revised accordingly.
I hope I am not missing anything if I isolate two main criticisms in this point. The main one is that composites during persistent episodes are not enough to get mechanistic / causal understanding, and the second one is that we mix substantiated interpretations of statistically significant results backed by literature with hypotheses.

The second point was raised by both other reviewers and we agree. We have entirely re-written the discussion section in order to clearly justify our claims based on evidence and clearly signal when a statement is a hypothesis.
There are two ways we have furthered our discussion of causal / mechanistic understanding in the revised version of the manuscript, thanks to your comments.
The first one, already mentioned in a response to a point by reviewer 2, is the expansion of lead/lag relationships. In particular, the new version of figures 8 and 9 include indicators before the onset of the persistent episodes, such that we can see if the conditions were already favourable before the onset. I invite you to read the response to that point of reviewer 2 for more details on this change.
The second one is the inclusion of an appendix that gives an alternative summary of our results for all jet lifecycles, not only the tail end of the persistence distribution, which shows, for example, the gradual decrease in mean latitude for more and more persistent EDJ lifecycles.
Where this appendix helps responding to your present comment is with its second figure, where we bin lifecycles by the quantile of an indicator of interest (tropical precipitation, for example) averaged over its length and average the persistence values of episodes falling into that bin. This helps close the loop as you suggest, looking at jet persistence filtering on favourable conditions. It helps confirm or infirm some of our hypotheses.

These analyses are too crude to be part of the main text. While they add a layer of proof to certain hypotheses, they do not bring anything new, so we believe their place is in an appendix rather than in the main text.

The abstract needs a thorough revision: As is, motivations for the study are not clear enough (What knowledge gap is being addressed? Which jet configurations persist? What makes the new toolbox the best tool for this study?), the word count is not used particularly judiciously (method details L6-8 are not needed at this point, stating that different jets/composites show different behavior L10-12 is somewhat insubstantial and arguably does not belong in the abstract), and the take home message L14-15 could be stated more clearly and with added detail about the implications
We substantially reworked the abstract based on your comments. Unnecessary details were removed and the results are more precise.

The objectives are spliced into too many subcomponents (Q1-Q6) (L67-74): The underlying ideas here are interesting, and it's good to clearly state the question(s) you are addressing. That being said, presenting six distinct research questions feels overly broad and ambitious for this format, and leads to structural and conceptual redundancy. Referring to six different questions across the rest of the paper makes the result feel more like a catalog or a research proposal, rather than a focused contribution. I recommend consolidating into 2 or 3 more precise questions. This would give the reader a clearer roadmap of the core objectives
Thank you for your feedback, yes you are correct we have rephrased Q1 as a working hypothesis that we thoroughly justify and rephrased the five others as three more precise questions.

- Q1 is difficult to parse out: what does “objective” mean here? Observer-independent? Climate-independent? What does “represent” mean? Is a “scalar time series” just a metric?

Q1 is rephrased as a hypothesis and heavily reworded. It is justified by an entire paragraph before the questions, supported by the rest of the introduction.

- Q2: previous studies have laid out their own “specific characteristics”, so the question seems not to be about specificity but rather about portability or objectivity. Q2 essentially seems to be a pre-requisite question to answer before asking Q1

I am not sure what you mean here. We want to know if, as stated in the literature, persistent EDJs are shifted equatorward, for example. This is the question answered by figure 2 of the original manuscript.

- Q3 is a special case of Q2.

Yes, we just removed Q3. I would argue it falls more as a special case of Q5 because the anomalous characteristics of the EDJ during STJ episodes tell a lot about the processes involved in STJ persistence.

- Q5 also seems like a question to ask before Q1: defining persistence in an objective manner seems sensible only if one knows how said persistence arises in the first place.

I don’t agree this specific point. Persistence is an intrinsic property of dynamical systems, things sometimes move slower for a while compared to some other times when things are very transient, and there are many ways to quantify this. We choose to focus on one way to quantify this, tracking an important feature (the jet), finding times when the feature does not move a lot over a few days, and looking at how that situation arises.

- Q6 is a special case of Q5.

Yes.

The choice of methods needs better justification and contextualization. The use of the Banderier et al. (2025) jet identification scheme is not adequately justified. There are other methods to choose from and compare to, formulated for a wide range of applications. As it stands, the primary rationale for choosing this specific method appears to be author familiarity. The particular strengths of this method for the present application should be clearly stated and put in context of the existing literature. In addition, labeling the method as “objective” (L448) is a bit misleading; an algorithm can be mathematically objective while still relying on arbitrary heuristics. The limitations of the methods should be discussed earlier than L484. For instance, if jet features meant to represent the eddy-driven jet are extracted from filtered wind fields, are the transient eddy momentum fluxes that drive the jet actually captured? If not, what do these features physically represent? If they are the waves themselves, how are they related to the waveguide? What other aspect(s) of the EDJ are/aren’t captured in this perspective? Similar questions apply to STJ-related jet features, which could capture transient fluctuations in the broader thermally direct jet stream itself. The statement that “There does not seem to be an easy way to separate eddies from instantaneous jet axes” (L496) may likely be read as the dismissal of an important methodological limitation. The manuscript could instead refer to the dynamical systems literature, where the perspective on jets as Lagrangian coherent structures or transport barriers does provide a synoptic description of jets that is distinct from a collection of eddies.

As stated at the start of this document, the methods section was rewritten almost entirely with more justifications, mainly prompted by your comment.

The choice of this jet detection method is motivated by its ability to capture EDJs and STJs in the same wind speed field and measure their properties separately. Tracking this type of jet features allows to measure persistence in the shape of the jet as well as in its position, which is both new and important according to the extensive literature we cite that links extreme events to anomalous jet configurations.

“Objective” was a wrong choice of word you are right. We know more carefully qualify our method.

The rest of your point relates to the interesting discrepancy between the mean flow in eddy-mean flow interaction theory and the instantaneous jets, or between the time-mean jet of climate and the instantaneous jet of synoptic weather. This is a topic of large interest for the authors of this paper, and we didn’t want to explore it too much here as it is a side point.

What we capture is the line at the core of the instantaneous jet, no filtering involved. Our jet cores have both high- and low-frequency components, if you will. In climate literature, another thing bears the name “jet”: an elongated band of high zonal wind in the time- or zonal-mean wind speed field. This seems to be your interpretation of the word jet, and this is not the perspective we use in this article. Where there is a problem and we completely agree with you, is in the link to “eddies”.

Mathematically, eddy velocity fields arise from linearisation, where one defines a “large” mean flow and “small” fluctuations around it. This separation can be done, in practice, using zonal mean (Palm and Eliassen 1961), temporal filtering (Hoskins et al. 1983) or PV zonalisation (Allen and Nakamura 2003), where the small fluctuations are defined as the difference between the full field and the mean flow. In the atmosphere, these two components of the full field have been identified as the mean flow, sometimes as the low-frequency jet, and the eddies.

The theoretical framework relates to a background flow that is not the same thing as our synoptic, instantaneous, 2D jet cores. There therefore no reason to believe the equations would be entirely valid. Furthermore, and more simply, momentum cannot cleanly be separated between eddies and instantaneous jets. Therefore, EKE contains jet momentum, which makes its interpretation less interesting. EKE could be reduced because there is genuinely less eddy activity, however that is best defined, or because the instantaneous jet is weaker or less wavy. We find a correlation of 0.5 between the jet core’s mean meridional component |v| and the EKE immediately around the jet, defined using u^e and v^e defined using 5-day high-pass filters.

Because of this discrepancy, we give less importance to the EKE signals in our discussion in the revised version of the manuscript and mention this problem in more details.

Using Lagrangian coherent structures to study the jet seems promising, and we really want to work with this tool for future studies exploring eddy-mean flow interaction with instantaneous, 2D jets, but are not suited to our study either. We are aware of the approach of Rivoire et al. 2026, building on a Lagrangian transport metric M from earlier papers, but the code to compute the transport metric is not open source, so we cannot recompute the Lagrangian jets from Rivière et al. 2026 so easily, and the way jets were extracted from M does not satisfy our needs. That problem could be solved by simply asking the authors for a M dataset, but there is more fundamental issue in the context of our study. The Lagrangian jets are not aligned with instantaneous wind speed maxima. That means the potential drivers we found such as precipitation or temperature gradients are not guaranteed to be spatially coherent with these jets. They would however be extremely useful as objects playing the role of a background flow in an eddy-mean flow interaction theory functioning outside of the time- or zonal-mean.

Methodological details are glossed over. I do not think that a long data section would be particularly pleasant to read, but several questions come up: L85: can you discuss the possible implications of stitching together variables across an ~5km altitude span? L87:"smoothed daily climatology" smoothed how? L89: 330K is arguably high for the EDJ— please discuss. L92-96: it is difficult to understand how the RWB algorithm works with this description. I would suggest removing the technical details to the benefit of a higher-level explanation of the concept behind the detection algorithm. Some level of detail about the method from Banderier el al. (2025) should similarly be provided. L111: what is the tilt of a jet core? What about the width? How is waviness calculated? L113-114: it sounds like the weighted mean of properties across different and possibly unrelated jet features is being calculated regardless of its physical meaning? L120-122: I am not convinced that "implicitly track[ing] the jets" this way is reasonable for the purpose, and the statement "more stable under complicated situations" demands justification. L124: the Fréchet distance should be briefly explained (perhaps using the dog walking analogy or something along the lines of "minimum peak separation required to map all points along one continuous path to another."), and it should also be stated that it was chosen because it preserves the sequential ordering of the jet cores, unlike other measures of curve-to-curve distance like the Hausdorff distance. You could also explain why this distance is in the persistence metric's denominator. L132: are you changing the quantile summer-to-summer to force the extraction of 30 events per summer? Are there several quantile thresholds? Regarding along/across-jet composites: double interpolation may smooth the data, which becomes a relevant point of discussion since you mention the "across-jet gradient" of the composites.

As stated at the start of this document, we have thoroughly reworked the methods and their explanations such that every non-standard choice is clearly justified. We have given a two-sentence minimal explanation of the Fréchet distance as you suggested, and a justification close to what you are suggesting was added, thank you.

Yes, the way the episodes were created was not very elegant and was greatly simplified in the revised version thanks to your comments. Our all dataset is now split into jet lifecycles based on the tracking a distance threshold that we justify and compare against the literature. All lifecycles have a persistence value (the sum of the inverse distances

over their lifetime, hence the distance at the denominator), and we simply take the 30 most persistent lifecycles of each jet. Much cleaner.

Double interpolation probably smooths the data but the results show spatially coherent results (blobs centred a maximum, gradients,...) spanning a lot of "pixels", such that smoothing would not obscure any small-scale feature. The second interpolation is only in the direction along the jet, so it should not affect the sharpness of the cross-jet gradients.

- Substantial text edits are needed. I include a list below for the first portion of the manuscript, to illustrate the degree to which I believe the manuscript should be edited for concision and clarity.

Minor comments up to L138 and for some of the figures:
- Title: "maintainers" feels awkward to me. Could consider "drivers" or perhaps "maintenance mechanisms"?
In the manuscript re-write and the title, the word "maintainer" does not appear anymore, in favour of drivers or maintenance mechanisms
- L43-45 "Therefore [...] RWB": I am not fully following the logic of the argument about jets' anomalous position/persistence and "additional momentum" because I could conceive momentum acting to disturb these jets rather than reinforce them in place. Could you clarify how momentum comes into this picture?
Because stronger jets are more persistent according to Woollings et al. 2018, anything that makes the jet stronger, i.e. extra zonal momentum, is a good candidate for a persistence driver. We have rephrased this whole section anyways per your and the other reviewers' comments.

- Definition of "jet" and "persistence": Given the paper's stated goal to discuss what "jet" means in the first place, it would be good upon the first few mentions of jet-relevant processes (L49 "equatorward flank of the jet", L61 "persistent conditions for the jet", etc) to clarify what "jet" means in that instance. It is a bit hard to read an introduction about jets, EDJs, and STJs, knowing that their very definition may be in question. The first explicit definition of jet only comes at L105. In that same vein, the paragraph L61-64 about jet persistence comes as a surprise given the central goal of the study to determine how this persistence arises in the first place. Currently, some readers may be under the impression that the study is tackling a solved problem.
Thank you for these suggestions. As per your and the other reviewers' comments we have thoroughly reworked the introduction to define what jets are more precisely. We also mention in more details their driving mechanisms when relevant, to refer back to them in the discussion. We also define and discuss persistence more broadly, to clearly place our study in the literature.
- L78 "the bulk of the research on jet dynamics focuses on winter" I think this is a good point and could be expanded upon in light of your particular perspective on jets.
We have expanded upon this point more in the revised version of the abstract and introduction, thank you for your suggestions.

- L110: "about twenty properties" is the kind of language that makes the reader feel like you're just doing things because you can. It's important to be specific about how many quantities are calculated and why. Quantities that are not used should not be included.
We have revised our language and now focus only on the four jet properties that are actually discussed in results section 3.3 in the revised version of the methods, thank you for your suggestions.
- Fig. 1: I would advise using more neutral colors. The x-axis labels should have a title and units. Dates could be labeled like "JUN-15" for clarity. Please split the y-axis between the top and bottom rows. Is there a specific reason other than aesthetics why kernel density estimates are shown instead of raw distributions? This should be stated especially because (i) you are using different KDE parameters seemingly arbitrarily and (ii) KDEs do produce artifacts, as I suspect is visible at the minimum episode length (5 days) in the last column, and especially for small samples such as the one you are using (30 events, if I understood correctly). With the current setup, the KDEs may not be very representative of the actual distribution (for instance, artificial gaps due to small sampling are likely to be emphasized by the KDEs). Why does the single value b refer to several bandwidths? In the caption, "scaled arbitrarily" does not explain what the scaling does.
These colours are chosen in part from personal preference. However, we keep the same colour scheme throughout the manuscript (and also in our 2025 article) such that pink always refers to the STJ and purple always refers to the EDJ. When we wish to represent the location of a EDJ or STJ core on a composite such figure 4 of the original manuscript, the chosen colours work well with the usual shading colour choices for temperature or precipitation anomalies.
KDEs were chosen for aesthetics, yes. The y axes were arbitrary because the relative heights of various points of the distribution are what matters, and not the absolute height. We have reworked this figure to now contain histogram which are a step closer to the "raw" distribution, so that we have fewer choices to justify. The y axes are now all either densities (such that the area of all bars sum up to one) or bin counts.
- L138: the result that the STJ produces more persistent jets seems to be largely a result of the methodological setup; EDJ features are more likely than STJ features not to be tracked from one time step to the next because of the use of a jet core length threshold.
You are correct, but to a degree this is necessary; it would not make sense to study STJ lifecycles when the STJ is extremely weak or short. The detection algorithm has very generous, seasonally varying thresholds to accommodate for the weak summer STJ.
- Fig. 2: colors are unnecessarily saturated, and it is not stated what they represent. If you cut your composites at +5 days you could get rid of the shading at the bottom of the subplots, which currently makes the figure feel busy and is not easily interpretable. Using the same y-axis limits between STJ and EDJ would allow visual comparison.

You are right we removed the bottom shading. I will stick to my colours for preference reasons as I don't think they harm the comprehension of the results. However, it should be clearly stated what they represent and it has been fixed in the revised version of the manuscript. We have used your suggestion for the y-axis in the updated version of the figure.

We have changed this figure to divide each panel into two subpanels, the relative time series for the STJ, with the pink lines only, and the relative time series for the EDJ, with the

purple lines only. This allows to “zoom” on each jet’s properties and get a more precise view of the shifts. We believe the shifts against climatology are more important than having the two jets in the same panel. We have made the two subpanels representing the mean latitude of the STJ, one during STJ episodes and one during EDJ episodes, share y axis limits, thanks to your suggestions. Likewise for all four properties and two jets, of course.

- Fig. 8/9: this is an overwhelming amount of information. The five different color schemes clash, and it is difficult to parse out any patterns.
We have reworked figures 8 and 9 to be much simpler to interpret, thanks to your comments.

Text edit suggestions up to L163:
- Throughout: “mid latitudes” might better be spelled as “mid-latitudes” or “midlatitudes”
Correct, we chose midlatitudes, thank you.
- L10: unclear what “the two jet categories” are in this paragraph L10-11. The EDJ and STJ? Please clarify. Note that EDJ and STJ would have to be defined in the abstract if used (L15).
Indeed STJ and EDJ. We have made sure it is clear what we mean by jet categories in the revised version of the manuscript.
- L10-11 statement “The precursors, [...] commonalities exist” is not substantial and should be advantageously replaced with more specifics about the results.
We have reworked the abstract thanks to your and reviewer 2’s comments. It is now more substantial and precise, and it is shorter.
- L14: “We a significant increase” is missing a verb.
Fixed, thank you.
- L17: earlier references seem more appropriate for this argument
We rephrased and sticked to the 2016 review paper. There are not so many pre-2000 articles specifically linking the jet stream and extreme weather events that we could find.
- L22: “if these” -> whether these?
Yes, much better, thank you.
- L23: “if the” -> whether the?
Yes, much better, thank you.
- L44: is “equatorward shifted” used as a compound adjective like “poleward-shifted” at L50?
Yes, fixed, thank you.
- L44: “Therefore” -> Therefore,
Fixed, thank you
- L46: “cycles” -> cycle? Is it really a cycle though?
“Baroclinic lifecycles” with or without spaces between “life” and “cycle” is the nomenclature in the cited article and the following literature. It is a cycle in the sense that the momentum from wave breaking ultimately feeds into the development of the next wave (very schematically)
- L49: could remove “wave breaking” to avoid repetition.
You are correct, I think I had kept it to be able to define the shorthands, but it’s very ugly.
- L50: “CWBs are” -> CWB is? Since you are referring to the breaking process itself.
Yes, fixed.
- L53-54: “The interaction of” what type of interaction is this referring to?

We detailed this point more in the revised section of the manuscript. WCB outflows bring large negative PV anomalies aloft and this deforms the jet core locally. With an equal size of WCB outflow, Vishnupriya et al. (2025) find that the deformation is greater for a poleward-shifted jet.

- L58 “both jets are linked to” clashes with “A stationary EDJ”

You are correct, this paragraph was rewritten along with the rest of the introduction.

- L63: “Consequently” -> Consequently,

Fixed, thank you

- L67: to me, “episodic” is redundant with “persistence” because in this context persistence already implies a temporary departure from a reference state.

We had the nomenclature of Tuel and Martius 2023 in mind when writing this. You are correct that in this context it is redundant. This question does not exist in the revised version anyways.

- L119: this sentence is only accessible to the reader who is already familiar with Limbach et al. (2012).

This line is not here anymore in the revised version. You’re right, when we refer again to Limbach et al. 2012 in the revised version, we state more clearly what we take from that study.

- L120: what does “successful” mean here?

This line is not here anymore in the revised version. We meant that it demonstrably worked and separated well EDJ features from STJ features in instantaneous data, as shown by both Spensberger et al. 2023 and Banderier et al. 2025, so it could be trusted as a baseline for tracking.

- L123: I do not think the notation for overlap and distance needs to be so complex since it is only used once more at L126 and without the need for the index c nor t. Small “o” is not the best notation.

We now only describe the metric with words without cumbersome notation that is never referred to again. I think I wrote this because for some people a short equation is easier to understand than a long text, but in this case, I do not believe it is useful.

- L126: I would not choose “simple” to describe this metric. Its units alone (s/m) are not simple to me.

We are more careful in how we qualify our metric.

- L133: I do not understand what is meant by “moderate number allows us to study each event individually.” Presumably, you could analyze any individual event regardless of the total sample size?

We meant analyse *all* events individually and present the individual results rather than only presenting statistics over all events. We rephrased this to be clearer.

- L135: I would be more cautious here and add “The STJ episodes defined as such are more likely”

Right, we have rephrased

- L136: your conclusion that “EDJ episodes happen throughout the year” does not match the seasonally specific plots shown in the figure.

Right, we meant throughout summer; we have rephrased.

- L137: “then” -> than. Add comma after “than the STJ”

Fixed, thank you

- L146-7: 2000 km and 100 km would read more easily than the exponent notation. What are “segment points”? I think the explanation of the method is contrived. Stating that 2D

fields are bilinearly interpolated onto the along-jet and across-jet direction could be sufficient, along with a statement that across-jet = orthogonal to the horizontal wind direction (rather than orthogonal to the local jet core direction). Providing a range of min to max jet core lengths would help contextualize what the [0,1] domain captures and how to interpret Fig. 6/7.

We have thoroughly reworked this subsection of the methods thanks to your comments and those of other reviewers. We use km rather than exponent notation throughout, we have simplified the explanation of the jet-centred composites and provided an explanatory figure, and provided a range of jet lengths, as well as discussed the influence of jet length variability on jet-centred composites thanks to a comment by reviewer 2.

- L152: "No special consideration is applied…" why not? What proportion of the data sampled does this apply to?

We removed this statement because it does not apply to more than 0.1% of the data and just confuses readers uselessly. We do not do it to always maintain a warm flank and a cold flank, or alternatively the wind speed in jet-centred space is always pointing to the right.

- L154-5: I do not understand this statement.

We meant to prepare the reader for the use of jet-centred composites also for surface variables although they represent the situation kilometres away from the jet vertically. We rephrased this sentence in the revised version of the manuscript.

L162-3: what p-values are calculated? What is the false discovery rate here?

p-values are always computed using block bootstrapping and significance is assessed using the Benjamini-Hochberg procedure. We do not compute the FDR rate explicitly, however. We make our procedure clearer in the revised version of the manuscript