Response to the Editor and Reviewers

Josip Brajkovic

July 2026

Dear Editor,

Thank you for giving us the opportunity to revise our manuscript. We would also like to thank the reviewers for their constructive comments, which have greatly improved the quality of the paper.

We have made every effort to address all of the reviewers' comments in the revised manuscript. In particular, we have:

- expanded the literature review to better motivate the research question;
- redesigned the precipitation association methodology and revised the corresponding figures accordingly;
- substantially reorganized the *Material and Methods* and *Discussion* sections;
- improved the manuscript throughout and clarified the points raised by the reviewers.

The following document contains our detailed point-by-point responses to all reviewer comments. For clarity, our responses are shown in blue, while all modifications made to the revised manuscript are highlighted in red.

We hope that the revisions satisfactorily address all concerns and that the revised manuscript is now suitable for publication in *Weather and Climate Dynamics*.

Kind regards,
Josip Brajkovic, on behalf of all co-authors

Reviewer #1:

The article by Brajkovic et. al. aims to understand the precipitation associated with cut-off lows (COLs) over Europe in the reanalysis era and attempts to demonstrate that extreme cut-off low precipitation is of particular importance in a future climate. Notably and importantly, the study highlights the failure of climate model projections to capture cut-off frequency correctly. Given their importance to precipitation extremes, this is a critical finding. However, in my view, the current study has a number of limitations and issues that need to be resolved before publication should be considered. These include the methodology of COL detection and precipitation attribution to them, the physical mechanism analysis, the discussion surrounding the findings and comparison of their results to the vast range of established literature. Below are a series of general and more specific comments for consideration of the authors and editor.

We would like to thank the reviewer for the thorough review of our paper. We truly appreciate the amount of work that has been achieved in order to revise our paper.

Below, we provide a point-by-point response to all comments. Our responses are shown in blue, while modifications made to the revised manuscript are highlighted in red.

General comments

1) Literature review

The current literature review seems to be very lacking in reviewing the pertinent literature on both precipitation over Europe and COLs. In essence the literature review is about 4 paragraphs long. There is a range of COL literature available tracking (Nieto et. al., 2005; Reboita et. al., 2010 etc.), where the authors simply cite the review of Munoz et al., 2020. Precipitation variability over Europe and its relationship to COLs also seems only marginally covered. As a non-European reader, I found it difficult to navigate the problem without this being more clearly laid out and discussed. It also makes the gap in the literature that authors are trying to solve more difficult to understand without. Similarly lacking literature includes previous work on projections of precipitation over Europe, past studies on extreme precipitation and its drivers, the role of bias correction in precipitation projections over Europe and many other related topics.

We thank the reviewer for this constructive comment. Following this suggestion, the Introduction has been substantially revised and expanded to provide a broader overview of the literature on both European extreme precipitation and cut-off lows (COLs). In particular, we have:

- expanded the review of previous COL detection methodologies, including approaches based on geopotential height, potential vorticity, and conceptual definitions of COLs;
- added a broader discussion of the observed and projected evolution of extreme precipitation over Europe, together with its main thermodynamic and dynamical drivers;
- introduced recent studies investigating future changes in COL occurrence and COL-associated precipitation;
- clarified the scientific gap addressed by this study and reformulated the research objectives accordingly.

In addition, following the reviewer's suggestion regarding precipitation projections, we expanded the discussion of the bias-adjustment procedure applied to ESM precipitation. The revised manuscript now discusses the potential influence of empirical quantile mapping on future projections and explicitly acknowledges this as a limitation of the study.

These modifications are incorporated throughout the Introduction (lines 28–70) and the Discussion (lines 393–400), resulting in a substantially more comprehensive review of the existing literature and a clearer motivation for the proposed methodology.

2) COL detection methodology

The methodology presented to detect COLs makes an interesting advance. In essence the closed contour methodology is a common one, however the authors method to limit the contour search using their cubic spline methodology is an interesting advance, which I think is worth reporting on. However, given the methods only detect closed contours at 500hPa, its entirely possible that the authors detect a range of weather systems including "medicanes" and deep extratropical cyclones that can have closed contours at 500hPa . It remains unclear to me whether the authors filter these out using their method or whether these other circulations are caught up in the overall methods.

This is particularly true given that a large proportion of the COL-related precipitation is found in the Mediterranean. In addition, the authors mention that on average they detect a COL every day by this algorithm (average of 360 per year). This number seems very high to me. The authors should attempt to convince the reader that their methodology sensibly detects the features they are trying to detect and better defend the number of COLs they detect.

We thank the reviewer for this constructive comment and for recognizing the originality of the proposed histogram-based methodology.

Following this suggestion, we substantially strengthened the methodological validation of the detection algorithm. A new manual validation based on 100 randomly selected daily synoptic situations (50 with at least one detected COL and 50 without detected COLs) has been added to the manuscript. The complete validation database together with the corresponding diagnostic figures illustrating the successive steps of the algorithm is now provided as supplementary material. This validation demonstrates that the algorithm reliably identifies synoptic-scale COLs while also explicitly documenting its principal limitations (lines 161–179).

We also clarified the scope of the proposed methodology. Because the algorithm identifies upper-level cut-off circulations solely from the z_{500} field, it does not attempt to distinguish between the different types of surface cyclones that may be associated with these circulations. Consequently, medicanes associated with a well-defined upper-level cut-off circulation may also be detected. This behaviour is consistent with the objective of the algorithm, which is to identify upper-level cut-off circulations rather than classify the associated surface weather system. This point is now explicitly discussed in the revised manuscript (lines 174–180). Moreover, based on the algorithm evaluation, we now explicitly acknowledge in the Discussion that, in a few isolated cases, other types of extratropical cyclones may also be identified as COLs (lines 388–392).

Finally, following the reviewer's concern regarding the relatively large number of detected COLs, we expanded the discussion of the counting methodology. We now clarify that COLs are intentionally counted on a daily basis because the objective is to associate precipitation with active COLs on each day. To further address this point, we additionally performed a spatio-temporal declustering analysis in which each persistent COL is counted only once. This reduces the mean annual number of detected systems from approximately 360 to 160 without altering the temporal evolution or the statistical significance of the reported trends (Results, lines 252–257; Supplementary Fig. A2). This additional analysis demonstrates that the principal conclusions of the study are robust to the counting strategy adopted.

3) Precipitation attribution

The attribution methodology attributes all precipitation over Europe on that day to COLs if the 95th percentile in the scene is within the COL area. To me, this is problematic for several reasons. First, COLs in my experience have a rainfall distribution on its eastern flank and therefore, at times, some of the heaviest falls perhaps fall outside of the area of the detected COL. Of course, this would be included if at least a single gridpoint falls within the COL, but I do wonder whether there are occasions where this does not happen. Second, the attribution of rainfall to the COL is dependent on the rainfall accumulations on that day. In other words, if there is severe precipitation in other parts of Europe but moderate precipitation from a COL, then presumably this would be considered a "dry COL". Yet, the COLs related rain rates could be large. This provides an inconsistent attribution of rainfall to the weather systems under investigation. Third, the precipitation over Europe is all considered to be related to the COL if any COL related precipitation is detected. Surely this misattributes (at least in part) some of the precipitation to COLs that had nothing to do with this circulation? The authors need to convince the reader that this is not the case. Finally, the authors make no mention of rainfall or extreme rainfall trends not considered to be COLs. How do these change? And what are they?

** We sincerely thank the reviewer for this important comment. We agree that the original precipitation attribution strategy relied on methodological choices that could lead to ambiguous associations in some situations. Following this suggestion, we substantially revised the attribution methodology.

First, the attribution procedure has been completely reformulated. Rather than associating precipitation based on the location of the most extreme precipitation pixels alone, the revised methodology first identifies spatially continuous precipitation fronts. A precipitation front is retained if it contains at least one grid cell exceeding the daily 99.9th spatial precipitation percentile over the European domain. A detected COL is then considered precipitation-associated only if this extreme precipitation front intersects the closed geopotential contour defining the COL (lines 186–189).

This revised methodology no longer assumes any preferred spatial relationship between the COL and the associated precipitation, allowing precipitation to occur anywhere around the circulation provided that the precipitation front remains physically connected to the detected COL. Likewise, it no longer attributes all precipitation occurring over Europe on a given day to the COL, but considers only the precipitation front containing the extreme precipitation event.

Following the reviewer's suggestion, the methodology figure has also been redesigned to explicitly illustrate two contrasting situations: one in which an extreme precipitation front intersects a detected COL and is therefore associated with it, and one in which no association is made because the precipitation front remains spatially disconnected (revised Fig. 1, see Material 1 below).

Finally, we expanded the discussion to clarify that COLs represent only one of several mechanisms responsible for European extreme precipitation. Other important processes, including atmospheric rivers and localized convective events, are now explicitly discussed as phenomena outside the scope of the proposed attribution framework (Discussion, lines 463–468). We also added a comparison with the evolution of all extreme precipitation events irrespective of COL association (Appendix Fig. A7), providing additional context for the reported COL-associated trends.

We believe these revisions substantially strengthen the physical consistency of the precipitation attribution methodology and directly address the concerns raised by the reviewer.

4) Discussion of trends and results

The authors make a few claims from their results based on the trend analysis of the results in both ERA5 and ESM data that seem to not be majorly substantiated. The authors highlight the result that future changes in rainfall are related to thermodynamic changes rather than dynamical changes. However, in the same analysis, the authors also show that the ESM projections fail to capture the dynamical process of interest (COLs), at least not adequately. To me, therefore, these assertions seem to be a stretch at most. The authors also discuss the trends found in projections and make statements about future change. However, given that the results show a lack of fidelity for the process under investigation, do the authors think their statements of future change are perhaps too strong? The authors justify their choice for example of SSP5-8.5 as this choice as this scenario gets closest to that which is observed. Does this choice make scientific sense?

We thank the reviewer for this important comment. We agree that our original discussion did not sufficiently acknowledge the limitations associated with the ESM representation of COLs and that some statements regarding future changes were stronger than warranted. Accordingly, we substantially revised and expanded the Discussion to provide a more balanced interpretation of both the historical and projected trends.

First, the discussion of the historical evolution of COL occurrence has been expanded. We now compare our results in greater detail with those of Muñoz et al. (2020), discuss the influence of the selected analysis period (including the post-satellite era), and highlight the dominant role of

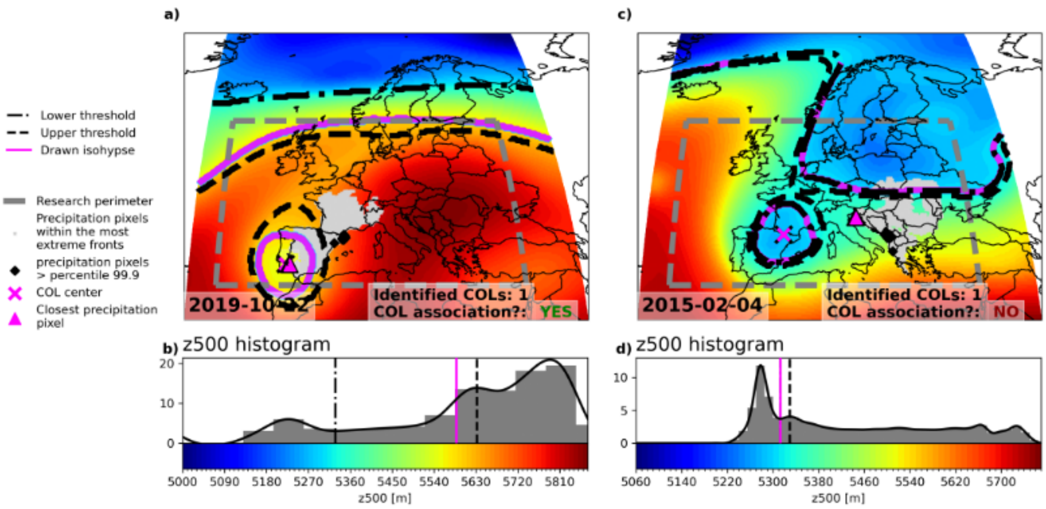

Figure 2. Illustration of the cut-off low (COL) detection and precipitation association methodology. (a) Example of a COL detected in the 500 hPa geopotential height (z_{500}) field on 22 October 2019. The magenta contour represents the closed isohypse retained by the iterative detection algorithm and the cross marks the centre of the identified COL. The most extreme precipitation front is associated with the detected COL because it intersects the COL area, resulting in a positive COL–precipitation association. (b) Histogram of z_{500} values showing the dynamically derived lower and upper thresholds used to guide the identification of closed contours. (c,d) Same as (a,b), but for 4 February 2015. Although a COL is successfully detected, the most extreme precipitation front does not intersect the detected COL. Consequently, no COL–precipitation association is assigned.

Material 1: Figure 1 of the revised manuscript.

inter-decadal variability in the historical evolution of COL occurrence (lines 422–426).

Second, we substantially revised the interpretation of the projected changes in COL-associated precipitation. Rather than relying solely on our analyses, we now place our findings within the broader context of recent studies investigating the respective roles of thermodynamic and dynamical processes in future precipitation changes (e.g., Pfahl et al. (2017), Crawford et al. (2025), and Schoofs et al. (2025)). Importantly, we now explicitly acknowledge that the selected ESMs systematically underestimate COL occurrence during the historical period and therefore that projections of future COL-associated precipitation should be interpreted with appropriate caution. The discussion and conclusions have been revised accordingly (lines 525–546 and 580–582).

Finally, we clarified the reason for focusing on the SSP5-8.5 scenario (lines 125–127). The objective was not to identify the most likely future emissions pathway, but rather to maximise the climate-change signal and facilitate comparisons between the selected ESMs. To demonstrate that our main conclusions are not specific to this scenario, we additionally present results obtained under the SSP1-2.6 scenario in the supplementary material and discuss them in the revised manuscript (lines 541–546).

We believe these revisions provide a more balanced interpretation of the results while explicitly acknowledging the limitations of the climate model projections.

0.1 Specific comments

L3: “polar regions” - do you mean “mid-latitudes”?

We thank the reviewer for this comment. We agree that the wording was ambiguous and have clarified it in the revised manuscript (lines 1–3):

Extreme precipitation events are projected to become more frequent and intense in Europe under climate change. In recent decades, cut-off lows (COLs), i.e., cold-core, upper-level low-pressure systems originating from polar/mid-latitude

L75-75: Why are PV and thermal gradients not “dynamically consistent and temporally homogeneous” in ERA5, while geopotentially heights are? This argument did not make sense to me.

We thank the reviewer for this comment. We agree that this statement was not sufficiently justified. It has therefore been removed from the revised manuscript.

L78: “polar or subpolar” - most COL studies agree that a detachment from the “midlatitudes” is what makes a COL. What do you mean by polar or subpolar? We thank the reviewer for this comment. We agree that the wording was ambiguous and have clarified it in the revised manuscript without specifically mentioning the geographical origin (lines 38–40). The revised text now reads:

A COL is a cold-core upper-level depression that becomes isolated from the main westerly flow as a result of large-amplitude jet-stream meanders (Palmén, 1949; Palmén and Newton, 1969; Nieto et al., 2005; Eumetsat, 2024)

L82-104: There are many studies which utilise a closed contour definition on isobaric surfaces and some on 500 hPa specifically, almost none of which are reviewed and referenced here. The difference of this algorithm seems to be the limiting of the contours that are candidates to be closed. In general I think there needs to be more discussion of this algorithm to be limited and could be expanded somewhat to help the reader along.

We thank the reviewer for this comment. In the revised manuscript, we have expanded both the literature review of previous closed-contour detection methodologies and the description of the proposed algorithm.

First, additional references to previous COL detection methods based on closed contours, potential vorticity and thermal structures have been added in the Introduction (lines 45–60). Second, we expanded the methodological section to better explain the philosophy behind the proposed histogram decomposition and how it relates the detected closed contours to the large-scale circulation (lines 152–155):

Furthermore, because the histogram decomposition is performed on the entire z500 field, the threshold values are derived from the large-scale synoptic configuration rather than from local properties of individual COLs. This provides a consistent statistical framework for identifying isolated upper-level circulations while preserving their relationship with the surrounding synoptic-scale flow.

L106-110: Are all the daily rain points that are 0 included in this calculation? Given rainfall occurs on the eastern edge of the COL, are there occasions where the heaviest rainy points may occur outside of the COL object, making the COL by their definition dry? All rainfall in the scene (all of Europe) is related to the COL if it appears on that day by this methodology. In my view, there would be a significant number of days on which there would be heavy rainfall elsewhere in the region which is unconnected to the COL. How do the authors account for these situations? The rainfall attribution methodology is biased towards other non-event related rainfall in the domain. Consider the following two scenarios. In both scenarios the max rainfall related to the COL is 5mm. However in one scenario there is severe convection of another part of Europe (unrelated and unconnected to the COL) and in the other scenario the remainder of Europe remains dry. This would potentially make q_{rain},95»5 in one scenario and q_{rain},95<5 in another and would result in one COL scene being a "dry COL" and the other a "wet COL". Yet they are both producing a similar rain rate in the vicinity of the COL. Does this hold true by their methodology and how do the authors account for this in their analysis?

We thank the reviewer for raising these important questions regarding the precipitation attribution methodology.

First, Fig. 2 has been extended to include the September 2024 (Boris) event. As for the July 2021 (Bernd) event, this example illustrates that the precipitation associated with impactful COLs is not necessarily confined to the eastern flank of the circulation. Consequently, our methodology does not assume any preferred spatial relationship between the COL centre and the associated precipitation. Instead, a COL is associated with an extreme precipitation event whenever the identified extreme precipitation front intersects the detected COL. This constitutes one of the main differences compared with the methodology proposed by Muñoz et al. (2020). This point is now discussed in the revised manuscript (lines 405–409):

In particular, the detection algorithm of Muñoz et al. (2020) requires the presence of an eastward-propagating thermal front associated with the COL, whereas the methodology proposed here identifies upper-level cut-off circulations solely from the z_{500} field, independently of the position of the associated frontal system. This distinction is important because the precipitation associated with impactful COLs is not systematically confined to the eastern flank of the circulation, as illustrated by the July 2021 (Bernd) and September 2024 (Boris) events (Figs. 3a and b).

Second, the reviewer correctly points out that the previous attribution strategy could depend on precipitation occurring elsewhere over Europe. Following this comment, we therefore revised the attribution methodology (see response to the previous comment**). The revised approach is now independent of the daily q_{rain} threshold and no longer attributes all precipitation occurring over Europe on a given day to the detected COL. Instead, only the spatially continuous extreme precipitation front containing the most extreme precipitation pixels is considered when determining whether a COL is associated with an extreme precipitation event. Consequently, the two hypothetical situations described by the reviewer would now be treated consistently, since unrelated precipitation elsewhere in Europe no longer influences the attribution.

L124-126: This paragraph is about input data and appears to be in the wrong section? Additionally, what timestep are you using for your COL detection?

We thank the reviewer for this comment. We agree that this paragraph was misplaced and largely repeated information already provided in the manuscript. It has therefore been removed from the revised version.

The COL detection is performed using daily z_{500} fields, as now clearly stated in the methodology (lines 108–111).

L135-140: Are there potential dangers to bias correction from a quantile mapping approach? For example, you may be elevating "weaker COLs" that are produced by the coarse resolution model to "stronger, rainier COLs" using a statistical approach that is unphysical. These kinds of limitations or disadvantages should be addressed and discussed fully. We thank the reviewer for raising this important question regarding the bias-adjustment procedure. We agree that no bias-adjustment technique is free of limitations and that empirical quantile mapping may influence the representation of the most extreme events. We have therefore added a dedicated paragraph in the Discussion (lines 393–400) to explicitly acknowledge these limitations and to soften the interpretation of the future projections:

The association of future precipitation events with detected COLs may also be influenced by the applied bias-adjustment procedure. In this study, precipitation is corrected using a classical empirical quantile mapping approach. Although conceptually simple, univariate quantile mapping has been shown to perform competitively with more sophisticated multivariate bias-adjustment techniques (Van de Velde et al., 2022). Nevertheless, for events located in the extreme tails of the distribution, the correction factors derived from the historical period may not fully represent the biases affecting the most exceptional future events (Pierce et al., 2015). Despite this limitation, the systematic underestimation of COL counts during the historical period suggests that the low number of projected COL-associated events is likely to be influenced, at least in part, by an underrepresentation of COL occurrence in the driving ESMs rather than solely by the bias-adjustment procedure.

Figure 2: I wondered whether his figure was absolutely necessary. Of course, the topography comes into your argument, but I wondered whether this could be incorporated into another figure, say Figure 1.

We thank the reviewer for this suggestion. We carefully considered merging this figure with Fig. 1. However, we decided to retain it as a separate figure for two reasons.

First, it provides useful geographical context for readers who may not be familiar with the European regions discussed throughout the manuscript (e.g., the Ionian Sea, the Cévennes Massif, or the Carpathian Mountains). Second, Fig. 1 is already dedicated to explaining the methodology and contains a substantial amount of information. Incorporating the topographic map into that figure would considerably reduce its readability. We therefore believe that keeping this figure separate improves the overall clarity of the manuscript.

L185-191: This paragraph's wording confused me. I think what you are trying to convey is that on average there are 360 COLs per year with a maximum of over 400. Could you clarify either way?

We thank the reviewer for this comment. We agree that the original wording was ambiguous and have clarified it in the revised manuscript (lines 247–250):

In particular, the 1970s stand out as a phase of high activity, with annual counts exceeding 400 detected COLs in the daily geopotential height fields. This period is preceded and followed by alternating phases of higher and lower COL occurrence, with annual values generally fluctuating around the average value of 360 COLs per year.

L190-191: I agree with this sentence that the number of COLs per year (essentially 1 per day!) seems high. In fact, for me this is too high! Are there any other studies that get a similar frequency of COLs? Please reference these if they exist. The caveat here that some COLs occur simultaneously to me was not enough to convince me that this result was physically possible. How many COL days are there?

We thank the reviewer for raising this important point. We agree that the reported annual counts appear high compared with previous climatologies and therefore required additional justification. In the revised manuscript, we now compare our results with previous COL climatologies, discuss the methodological differences that explain these discrepancies (lines 401–412), and clarify the effect of counting persistent COLs on a daily basis rather than after spatio-temporal declustering (lines 381–387):

Over the historical period, the proposed algorithm identifies on average approximately 360 COLs yr^{-1} over the European domain. At first glance, this value may appear relatively large compared with previous climatologies. These reported counts correspond to daily detections and are therefore not spatio-temporally declustered. As shown by the additional declustering analysis (Appendix Fig. A8), counting each persistent COL only once substantially reduces the annual number of detected systems while preserving the temporal evolution and the main conclusions of the study. Together with the manual validation presented in Section 2.2.2, these analyses provide confidence that the reported frequencies primarily reflect methodological differences rather than detection errors.

L192-196: There is a large maximum over Italy and the Mediterranean Sea. Are any of these Medicanes or other kinds of cyclones?

We thank the reviewer for this important question. The proposed algorithm identifies upper-level COLs solely from the z_{500} field and therefore does not distinguish between the different types of surface cyclones associated with these upper-level circulations. Following the reviewer's suggestion, we now explicitly discuss this limitation and clarify that some detected COLs may indeed be associated with medicanes. We also note that the role of upper-level COLs in Medicane development is well documented in the literature. The following paragraph has therefore been added to the manuscript (lines 173–180):

Overall, this manual validation indicates that the proposed methodology reliably identifies synoptic-scale COLs at various stages of their lifecycle while providing a transparent assessment of its principal limitations. The proposed methodology identifies upper-level COLs solely from the z_{500} field and does not explicitly distinguish between the different types of surface cyclones that may be associated with them. Consequently, some detected COLs may also correspond to medicanes provided that these systems are associated with a well-defined upper-level cut-off circulation. This behaviour is consistent with the objective of the algorithm, which aims to identify upper-level cut-off circulations rather than classify the associated surface weather system. Moreover, medicanes are commonly associated with upper-level COLs (Eumetsat, 2024; Saraceni et al., 2023).

Figure 4a: The highest variability in ERA5 seems to be outside of the satellite era (1940-50s) and 1970s. Other than this the variability seems relatively consistent. Are there possibly data issues in the analysis record that contribute to this? And if they were removed, how would this affect the trend that you have found (ie. from 1980 onwards only)?

We thank the reviewer for raising this important point. We agree that changes in the observing system and data assimilation may influence long-term reanalysis products. Although we do not explicitly quantify this contribution, we have investigated the sensitivity of our results to the analysis period by repeating the trend analysis over the post-satellite era (1980–2024). The following discussion has been added to the revised manuscript together with the corresponding supplementary figure (lines 422–426):

Furthermore, when restricting the analysis to the post-satellite era (1980–2024), no statistically significant yearly trend in COL occurrence is detected (Fig. A5). Depending on the season, the estimated trends can even become negative, as observed during MAM. We believe that this sensitivity to the selected analysis period further highlights the dominant role of inter-decadal variability in the historical evolution of COL occurrence.

L205-209: Given the number grid points, would the 99.9th percentile not just be the maximum value?

We thank the reviewer for this question. No, because the analysed ERA5 domain contains 8,469 grid cells. Consequently, the 99.9th spatial percentile corresponds to approximately the nine wettest grid cells rather than to the single maximum precipitation pixel. This choice therefore allows the attribution criterion to rely on a small cluster of the most intense precipitation pixels instead of a single potentially noisy grid cell.

L215-219: Again your estimates of COL rain days seems high. On average you are saying that 180 days are rainy COLs. That is half the year on average! This finding needs to be justified and contextualised with other literature and defended. For example, in Figure 5c, you show that COLs affect 2 / 3 months of winter on average !

We thank the reviewer for this comment. We agree that the proportion of COL-associated precipitation days appears relatively high and therefore deserves additional discussion (lines 507–514) :

We find that a large fraction of extreme precipitation days over Europe are associated with COLs (Fig. 5a). A similarly dominant role of persistent synoptic-scale systems has also been reported over Australia by Barnes et al. (2023), although direct quantitative comparisons remain difficult because different weather systems and precipitation thresholds were considered. During summer, the proportion of COL-associated extreme precipitation days can reach up to 68% (Fig. 5i). Although the numbers of COL-associated days may appear relatively high, they should be interpreted in the context of the large European domain considered in this study and the relatively permissive precipitation threshold used to associate the event with a COL. These findings indicate that accurately representing COL occurrence is essential for future assessments of hydroclimatic risk over Europe.

Figure 8: It remains unclear to me why only projections are shown here? Why not show the full timeseries for the projections? In addition, there are many instances where the projections have the opposite trend or no trend compared to reanalysis. This has not been explained or discussed in detail. Additionally, it is clear that the projections completely underestimate the frequency of COLs. What does this say about our confidence in the projections regarding both dynamics and rainfall variability? Discuss this in detail. We thank the reviewer for this comment. Actually, Fig. 8 already shows the complete ESM simulations, including both the historical period (1981–2014) and the future SSP5-8.5 scenario (2015–2100). As this was not explicitly stated, we have clarified it in the figure caption:

Black curves correspond to ERA5 over the historical period, whereas coloured curves correspond to continuous ESM simulations covering the historical period (1981–2014) followed by the SSP5-8.5 scenario (2015–2100).

Regarding the discrepancies between the ESMs and ERA5, as well as the systematic underestimation of COL-associated precipitation days, the Discussion has been substantially expanded. We now discuss the large inter-model spread, the dominant role of internal climate variability, the limitations associated with the representation of COL dynamics in current-generation ESMs, the potential influence of the bias-adjustment procedure, and the implications of these limitations for confidence in future projections. We also softened the conclusions regarding future changes accordingly (lines 536–546).

The underrepresentation of COLs is also reflected in projections of COL-associated extreme precipitation. Among the selected models, EC-Earth3-Veg exhibits trends most consistent with those derived from ERA5. Nevertheless, no ESM is able to reproduce the recent observed levels of COL-associated extreme precipitation. In most cases, present-day ERA5 values are only reached—or even exceeded—by the end of the 21st century in EC-Earth3-Veg under the SSP5-8.5 scenario. This finding motivated our focus on this high-emissions scenario, as even under such forcing, simulated COL-related extremes remain lower than those currently observed. Moreover, the SSP5-8.5 scenario is particularly useful because it maximizes the climate change signal and therefore facilitates the comparison between the selected ESMs. Nevertheless, the conclusions of this study are not specific to this scenario. Simulations performed under the SSP1-2.6 scenario exhibit a similar behaviour, characterized by strong inter-decadal variability in COL-associated precipitation and by EC-Earth3-Veg providing the closest agreement with the ERA5-based attribution. These additional results are presented in Appendix Fig. A7. This underrepresentation implies that current climate projections may underestimate the contribution of COLs to future European precipitation extremes.

Section 4.1: To me this section is not a discussion section but a part of the methodology section. Testing the sensitivity of the algorithm (all factors including area sizes, q_{rain} thresholds etc. Should all be done much earlier and cohesively in one section.

We thank the reviewer for this helpful suggestion. Following this recommendation, the manuscript has been substantially reorganized. Most methodological choices, including the validation procedure and the precipitation attribution methodology have been moved to or discussed directly within the Methodology section to provide a more coherent presentation of the framework.

The former Section 4.1 has been completely rewritten and is now entitled *"Methodological limitations and comparison with previous studies"*. We nevertheless retain the discussion of the sensitivity to the A_{crit} parameter in the Discussion section because its interpretation relies on the results presented in the paper. In particular, the impact of A_{crit} on the detected COL climatology and long-term trends can only be meaningfully assessed after the corresponding results have been introduced.

Figure 9 and discussion: It still remains unclear to me whether this methodology is capturing what it needs to capture and the sensitivity test does little to convince me of that. See comments on this above.

We thank the reviewer for this comment. Figure 9 evaluated the sensitivity of the COL-associated precipitation trends to the q_{rain} parameter. Following the reviewer's comments, the precipitation attribution methodology has been fundamentally revised and is now independent of the q_{rain} parameter (see our response to the previous comments). Consequently, this sensitivity analysis is no longer relevant and Figure 9 has therefore been removed from the revised manuscript.

L350: "slowing of mid-latitude dynamics" - what dynamics? What do you mean here? Be specific.

We thank the reviewer for this comment. We agree that this wording required clarification. The point made by Mishra et al. (2025) is that polar amplification may weaken the mid-latitude jet stream, thereby reducing the propagation speed of upper-level circulation patterns and favouring longer-lasting COLs. The sentence has therefore been reformulated as follows (lines 55–60):

Some recent studies have suggested that climate change may also influence the large-scale atmospheric circulation through Arctic amplification, i.e. the faster warming of the Arctic relative to lower latitudes, which reduces the meridional temperature gradient (Preece et al., 2023). Such changes may affect the characteristics of the mid-latitude jet stream and, consequently, the occurrence and persistence of cut-off lows. For example, Mishra et al. (2025), based on analyses of both the fifth-generation ECMWF atmospheric reanalysis (ERA5; Hersbach et al., 2020) and Earth System Model (ESM) projections, suggested that a weakening of the upper-level westerlies could favour longer-lasting COL systems.

L362 - L364: The trends are different to that found in the Munoz et al. (2020) period. What does this say about the fidelity of the trends you have found? This should be discussed more fully.

We thank the reviewer for this comment. Following your suggestion, we substantially expanded the discussion comparing our results with those of Muñoz et al. (2020) and Mishra et al. (2025). In particular, we now discuss the influence of the selected analysis period, the strong role of interdecadal variability, and the methodological differences between the two detection approaches (lines 401–418):

Beyond these methodological considerations, it is also useful to compare the proposed framework with existing COL climatologies. Among similar previous studies, Mishra et al. (2025) reported approximately 50 COLs yr^{-1} over a broadly similar European domain for the 1979–2004 period using ERA5 reanalysis data, while Muñoz et al. (2020) obtained comparable frequencies using NCEP/NCAR reanalyses over 1960–2014. These differences are likely explained by methodological choices. In particular, the detection algorithm of Muñoz et al. (2020) requires the presence of an eastward-propagating thermal front associated with the COL, whereas the methodology proposed here identifies upper-level cut-off circulations solely from the z_{500} field, independently of the position of the associated frontal system. This distinction is important because the precipitation associated with impactful COLs is not systematically confined to the eastern flank of the circulation, as illustrated by the July 2021 (Bernd) and September 2024 (Boris) events (Figs. 3a and b). Likewise, Mishra et al. (2025) identify COLs using potential vorticity anomalies rather than closed geopotential structures. Consequently, the proposed methodology detects a broader range of mature upper-level cut-off circulations, including systems in a decaying stage that still exhibit well-defined closed z_{500} contours.

In their study, Mishra et al. (2025) also suggest that the frequency of particularly long-lasting systems might increase in the future. Their conclusions are derived from projected COL frequency anomalies computed for the 2071–2100 period. Similarly, Muñoz et al. (2020) reported slight long-term trends in COL frequency over Europe for the period 1960–2017. Using the detection algorithm developed here, we find no evidence for a systematic long-term increase in the number of detected COLs on a yearly basis. Instead, the historical record is dominated by pronounced inter-decadal variability, with periods such as the 1970s exhibiting enhanced COL activity and persistence.

L375-377: I do not understand this point that leads to a conclusion about the thermodynamic environment? There is a trend in the frequency of COLs in reanalysis and there is a trend in extreme precipitation and the ESMs fail to capture the dynamics?

We thank the reviewer for this comment. We agree that this point was not sufficiently explained in the original manuscript. We have therefore reformulated this part of the Discussion to clarify that our conclusions regarding the dominant role of thermodynamic processes are primarily based on the historical ERA5 analysis and are consistent with previous studies rather than being inferred solely from the ESM projections. We now write (lines 575–590):

Overall, this study suggests that the observed increase in European COL-associated extreme precipitation is not necessarily driven by an increase in COL frequency itself, but is instead consistent with a growing influence of thermodynamic factors. This interpretation is supported by the absence of a systematic increase in observed COL occurrence together with the documented increase in extreme precipitation intensity reported in previous studies. At the same time, the strong role of natural variability implies that periods of clustered high-impact COL events may continue to occur, even in the absence of clear long-term dynamical trends.

Furthermore, we now explicitly discuss the limitations arising from the underestimation of COLs in current-generation ESMs and have softened our conclusions regarding future changes accordingly.

L402-405: How do you know this? In my view, your evapotranspiration does not show this to me. The Baltic Sea and North Sea are not major moisture sources are they?

We thank the reviewer for this comment. We agree that our original wording was too strong and could suggest a causal relationship that is not directly demonstrated by our analysis. We have therefore reformulated this paragraph as follows (lines 491–495):

In Central Europe, for example, total monthly evapotranspiration shows significant long-term increases in April, August, September and October over the 1940–2023 period. Other regions across Europe, such as the Baltic Sea and the North Sea, also exhibit significant positive evapotranspiration trends, particularly during April, May, and June (Fig. 10). These trends suggest an increasing availability of moisture in some regions, which may contribute to a more humid lower troposphere during the most active COL seasons.

Moreover, Fig. 10 has been modified to display only statistically significant positive evapotranspiration trends, making the main message clearer. The related caption now writes :

Monthly anomalies of total evapotranspiration (%) over selected European regions based on ERA5 data, expressed relative to the 1981–2010 climatological mean. Linear trends are shown only where they are positive and statistically significant (p-value < 0.05), as determined using a Mann–Kendall test. A 15-year running mean is applied.

L413-416: In my view, there is not enough evidence in the current analysis to make this statement. We thank the reviewer for this comment. We agree that the original statement was too strong and could not be fully supported by the present analysis. We have therefore softened the conclusion as follows (lines 503–505):

Taken together, COLs may act primarily as the synoptic-scale trigger, while the ultimate severity of extreme precipitation events may depend on a complex interplay between atmospheric circulation, land–atmosphere feedbacks, and thermodynamic conditions. These interactions remain insufficiently constrained and represent an important avenue for future research.

L429-430: This sentence and the reasoning behind the choice of SSP5-8.5 does not make sense to me. One cannot simply choose this scenario because it fits better? What is the physical basis for this choice?

We thank the reviewer for this comment. We agree that the reason for selecting the SSP5-8.5 scenario required clarification. The objective was not to select the scenario that best reproduces the observations, but rather to maximize the climate-change signal in order to facilitate the comparison of the responses among the selected ESMs. In the revised manuscript, we now write (lines 541–546):

Moreover, the SSP5-8.5 scenario is particularly useful because it maximizes the climate change signal and therefore facilitates the comparison between the selected ESMs. Nevertheless, the conclusions of this study are not specific to this scenario. Simulations performed under the SSP1-2.6 scenario exhibit a similar behaviour, characterized by strong inter-decadal variability in COL-associated precipitation and by EC-Earth3-Veg providing the closest agreement with the ERA5-based attribution. These additional results are presented in Appendix Fig. A8.

L460-462: This is a really nice highlight of this work. It should be elevated as a finding. If COLs are underestimated in projections, this has large implications on the adaptation and climate risk community who use these data.

We thank the reviewer for this positive comment. We agree that this is one of the main findings of the study. It was already emphasized in the Conclusions, and we have now also highlighted it in the Abstract (lines 18–20):

Our results suggest that current-generation ESMs struggle to represent both the dynamical characteristics of COLs and their associated precipitation impacts, which has important implications for the interpretation of future projections of extreme precipitation over Europe.

L463-465: From the evidence provided in this study, I do not understand how this assertion can be made. How do we know that the "thermodynamic environment" contributes to most rainfall change when it has been clearly demonstrated by the authors that COLs are completely mis-represented by their definition?

We thank the reviewer for this comment. We agree that the original wording was too strong given the limitations of the ESMs in representing COL occurrence. We have therefore reformulated this statement to focus on the observed historical changes rather than on future projections (lines 575–578):

Overall, this study suggests that the observed increase in European COL-associated extreme precipitation is not necessarily driven by an increase in COL frequency itself, but is instead consistent with a growing influence of thermodynamic factors. This interpretation is supported by the absence of a systematic increase in observed COL occurrence together with the documented increase in extreme precipitation intensity reported in previous studies.

Reviewer #2:

Review of "Past and future evolution of cut-off low associated extreme precipitation in Europe" By Brajkovic et al

Summary

This study seeks to under historical and future trends of COL pressure systems in Europe using ERA 5 data and six CMIP 6 models. The study shows that COLs vary per season and the lowest number of events are observed in DJF, the boreal winter. The authors show that there is are no significant trends in COLs in ERA 5, clear increasing trends are seen when higher precipitation thresholds are used, showing that the most extreme COLs are actually on the increase. The study future considers data from CMIP6 models under the SSP5-8.5 scenario. Whilst most models do not show a continued increase of the most extreme COLs into the future, at least one model does.

Because of the significance of these systems at the present moment and their role in contributing to weather and climate extreme events, this study warrants consideration by the WCD. However, as a reviewer I feel that the manuscript requires significant or major improvement before it can be accepted for publication. Broadly speaking, also given the focus of WCD, the manuscript lacks dynamical analysis to explain the results shown. So, I invite the authors to consider this. As it may require additional calculations, and perhaps even the use of approaches like composite analysis to draw out some of the dynamics issues, this suggestion constitutes major review.

One other point to make that applies right through the manuscript, is the use of the words "attribution". This gives the impression that this is a climate change attribution study and it does not really seem to be the case. So please consider another way of characterized what you have done in this work; it is not attribution.

We would like to thank the reviewer for the thorough review of our paper. Following your comments, the manuscript has been substantially revised and improved. In particular, we have:

- expanded the literature review on European extreme precipitation, COL detection methodologies, and future projections of COL occurrence and associated precipitation;
- reorganized the methodology section and substantially revised the precipitation association method, making it independent of the q_{rain} parameter. The results have consequently been updated while remaining fully consistent with the main conclusions of the original manuscript;
- strengthened the Discussion by placing our findings in the context of recent studies on the respective roles of thermodynamic and dynamical processes, while adopting a more cautious interpretation of the future projections;
- replaced the term "attribution" with "association" throughout the manuscript, as we agree that the former could be misleading.

Regarding the suggestion to perform additional dynamical analyses, we carefully considered this possibility. However, we believe that such analyses would constitute a substantial extension of the scope of the present study, whose primary objective is to develop and evaluate an objective methodology to associate upper-level cut-off lows identified from the 500 hPa geopotential field with extreme precipitation events.

Below, we provide a point-by-point response to all comments. Our responses are shown in blue, while modifications made to the revised manuscript are highlighted in red.

Introduction

Lines 25 – 70: There are a number of papers on NH COLs that the review has not considered, e.g. Nieto et al 2005 is one, and other climatological studies such as Wernli and Sprenger (2007) and Portmann et al (2021) and others that looked at the same topic from a PV cut-off perspective. Broadly speaking the literature review is rather thin. Novelty of the work is not clearly defined. What is this study contributing what other studies on the natural variability of NH COLs have not done? It is not sufficient to speak of an opportunity using a consistent "attribution" (as noted previously, not appropriate) framework. Also please mention the data used in studies only when it is material to the discussion, such when you want to point out discrepancies between studies and the reanalyses products are likely the source of them.

We thank the reviewer for this comment. We agree that the literature review in the original manuscript was too limited. The Introduction has therefore been substantially expanded to provide a broader overview of previous Northern Hemisphere and European COL climatologies, including studies based on both closed-contour and potential-vorticity detection methodologies (lines 28–69):

In addition, the novelty of the present study has been clarified. We now explicitly emphasize that, beyond proposing a new objective detection algorithm, this study provides a long-term climatology of COL-associated precipitation over Europe together with a framework to systematically associate extreme precipitation events with dynamically detected COLs, and extends this methodology consistently to CMIP6 simulations (lines 66–86).

Finally, following the reviewer's suggestion, references to the datasets used in previous studies are now only included where they are relevant to the discussion, particularly when differences in results may arise from the choice of reanalysis or climate model product.

Line 30: The correct reference here is Palmen (1949) and Palmen and Newton (1949).

We thank the reviewer for this comment. The references to Palmén (1949) and Palmén and Newton (1969) have now been added in the revised manuscript (lines 38–40).

Line 31 to 32: Should the consideration not be temperature advection at the surface, instead of strong temperature gradients? Unless the authors mean to invoke the thermal wind relation to talk about the jet streaks that have been observed during COL evolution.

We thank the reviewer for this comment. We agree that the original wording was imprecise. Our intention was simply to introduce why COLs are often associated with heavy precipitation. The sentence has therefore been reformulated as follows (lines 40–41):

The cold-core nature of these systems can enhance atmospheric instability through the presence of cold air in the upper troposphere, which can enhance upward motion and the development of intense precipitation.

Line 45: If the authors appropriately reference Barnes et al (2023)'s work on slow-moving PV anomalies, then naturally Wernli and Sprenger is also relevant, as well as where these anomalies come from. What causes them?

We thank the reviewer for this comment. We agree that this reference is relevant. We have now extended the literature review in the Introduction and added this reference. We also briefly explain that PV cut-offs generally originate from Rossby-wave breaking and the subsequent isolation of high-PV air from the main westerly flow (lines 45–55):

Most previous studies have used techniques based on geopotential height minima to identify COLs (Fuenzalida et al., 2005; Ndarana and Waugh, 2010; Favre et al., 2012; Pinheiro et al., 2016), while other techniques are based on closed contours of potential vorticity (PV) anomalies identified on isentropic surfaces (Wernli and Sprenger, 2007) or on conceptual models of COLs (Nieto et al., 2005; Reboita et al., 2010; Muñoz et al., 2020). These PV cut-offs generally originate from Rossby-wave breaking, during which high-PV air becomes detached from the main westerly flow. Among these studies, only Muñoz et al. (2020) proposed an analysis of the evolution of annual COL counts over Europe for the period 1960–2017 using National Centers for Environmental Prediction (NCEP) reanalysis data, based on a detection algorithm relying on isolated low-pressure systems associated with eastward-propagating thermal fronts. Their results suggest significant positive trends over Europe.

Line 53: Even though ERA5 is very well what known, please write out the full-name when using syncronyms for the first time. We thank the reviewer for this comment. We now define ERA5 the first time it appears in the manuscript (lines 58–60):

For example, Mishra et al. (2025), based on analyses of both the fifth-generation ECMWF atmospheric reanalysis (ERA5; Hersbach et al., 2020) and Earth System Model (ESM) projections, suggested that a weakening of the upper-level westerlies could favour longer-lasting COL systems.

Methods and materials

Is there any particular reason that the authors started with the methodology. I suggest that you rearranged this section and start with the domain that you discussed, then the data and then the methods. That is the norm and besides who to you refer to the ERA 5 data that you use in line 75 before explaining what it is. At this point of the paper, we don't even know what resolution did you choose and why. On line 73 you talk about 500 hPa GPH, but from which dataset did you extract these. This section requires major revision.

We thank the reviewer for these comments. Following your suggestion, the methodological section has been substantially reorganized. The manuscript now first introduces the study domain and the datasets, followed by the description of the detection methodology. In particular, the ERA5 reanalysis is now introduced before it is first used, together with its spatial resolution and the corresponding references to both the Copernicus Climate Data Store and Hersbach et al. (2020) (lines 107–111):

To ensure that the methodology can be readily applied to both reanalyses and ESM outputs, the detection algorithm relies exclusively on daily 500 hPa geopotential height (z_{500}) fields together with daily precipitation fields. For the historical period (1940–2024), we use the ERA5 reanalysis, with a horizontal resolution of $0.25° \times 0.25°$ (Copernicus Climate Change Service (C3S), 2017; Hersbach et al., 2020).

The numbering of the subsections is inconsistent. Sometimes the authors use numbering in other cases, no numbering. Please correct this cosmetic issue.

We thank the reviewer for this comment. The numbering of the sections and subsections has been made consistent throughout the revised manuscript.

Lines 106 – 106 In my opinion this might exclude important events from the analysis because it is entirely plausible you may find COLs with rainfall occurring outside the closed circulation region. Please test of this is ever the case so that you do not exclude important events. One way to combat this problem is to use vertical motion combined with moisture convergence to identify where rainfall might be occurring for the COLs.

We thank the reviewer for this valuable comment. We agree that precipitation associated with a COL may extend beyond the closed geopotential contour and that a criterion based solely on the location of the most extreme precipitation pixels could therefore exclude relevant events.

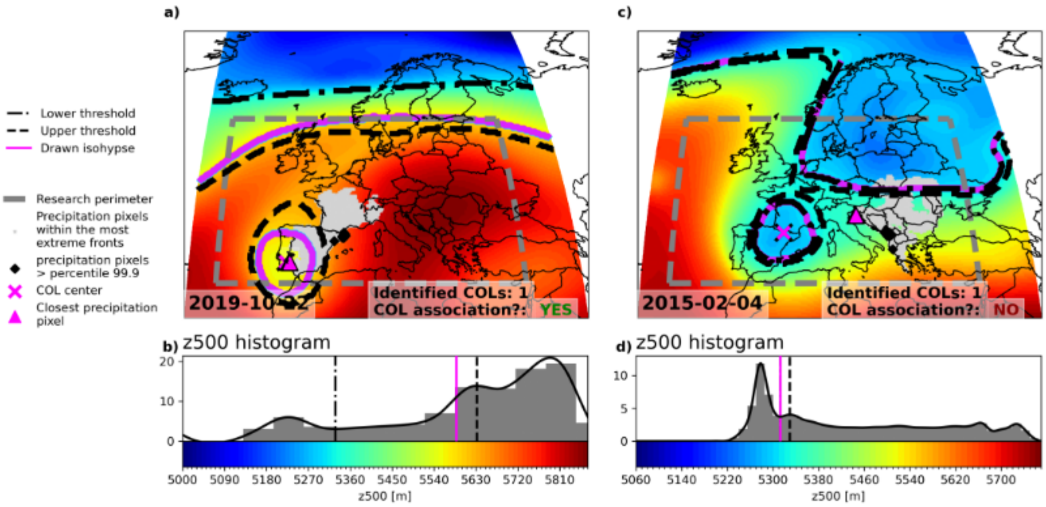

Figure 2. Illustration of the cut-off low (COL) detection and precipitation association methodology. (a) Example of a COL detected in the 500 hPa geopotential height (z_{500}) field on 22 October 2019. The magenta contour represents the closed isohypse retained by the iterative detection algorithm and the cross marks the centre of the identified COL. The most extreme precipitation front is associated with the detected COL because it intersects the COL area, resulting in a positive COL–precipitation association. (b) Histogram of z_{500} values showing the dynamically derived lower and upper thresholds used to guide the identification of closed contours. (c,d) Same as (a,b), but for 4 February 2015. Although a COL is successfully detected, the most extreme precipitation front does not intersect the detected COL. Consequently, no COL–precipitation association is assigned.

Material 2: Figure 1 of the revised manuscript.

Following this suggestion, we revised the precipitation association methodology. Rather than considering only the most extreme precipitation pixels, we now identify spatially continuous precipitation fronts composed of precipitation pixels exceeding 1 mm day^{-1}. A precipitation front is retained if it contains at least one grid cell exceeding the daily 99.9th spatial precipitation percentile over the European domain. A detected COL is then classified as precipitation-associated if at least one precipitation pixel belonging to one of these extreme precipitation fronts lies within the closed geopotential contour defining the COL. The revised text now reads (lines 185–189):

To this end, precipitation fronts are first identified as spatially continuous regions composed of precipitation pixels exceeding 1 mm day^{-1}. A precipitation front is retained if it contains at least one grid cell exceeding the daily 99.9th spatial precipitation percentile over the European domain. A detected COL is then classified as precipitation-associated if at least one precipitation pixel belonging to one of these extreme precipitation fronts lies within the closed geopotential contour defining the COL.

This revised methodology allows the most extreme precipitation to occur outside the closed geopotential contour, provided that it belongs to the same continuous precipitation front intersecting the detected COL. The methodological figure (Fig. 1) has also been redesigned to illustrate both situations: one in which an extreme precipitation front is associated with a detected COL and one in which it is not (see Material 1 below).

Regarding the suggestion of using vertical motion and moisture convergence, we agree that such variables could provide valuable physical insight into precipitation generation. However, we believe that incorporating them would substantially increase the complexity of the methodology and move beyond the primary objective of this study, which is to develop a simple and reproducible framework for associating upper-level COLs with extreme precipitation events.

Caption of Figure 3: Which Isohypse does the closed magenta contour represent? Is it the largest?

We thank the reviewer for pointing this out. No, the magenta contour does not necessarily represent the largest closed isohypse. Rather, it corresponds to the closed isohypse retained by the iterative contour-search procedure after applying the successive detection criteria described in the methodology. We have clarified this point in the caption of Figure 2 (previously Figure 3; see the caption of Material 1 above).

Lines 169- 177: Please show these moisture flux diagnostics you are referring to. Thank you for this comment. We agree that the original wording was misleading, as we did not compute or present moisture flux diagnostics. Our intention was to refer to the orientation of the precipitation bands associated with the cut-off lows. We have therefore reformulated this section accordingly (lines 236–238):

These three recent COL-associated extreme events demonstrate how the algorithm links extreme precipitation to cut-off lows. They also illustrate the wide range of possible spatial configurations between the COL center and the associated precipitation bands, highlighting that intense precipitation can occur in various locations relative to the upper-level circulation.

Lines 189 – 191: For a journal focusing on atmospheric dynamics, the authors should be expected to offer an explanation (or at the very least a plausible one) for the anomalous behaviour during the 1970s. This is actually quite critical. What happened in the background flow that couls have led to more waves breaking etc.

We thank the reviewer for this valuable comment. We agree that the discussion of the 1970s anomaly should be expanded. The manuscript has therefore been revised to provide additional context and discuss plausible mechanisms proposed in the literature (lines 444–450):

This period has also been identified in previous studies as a phase of major circulation changes over the North Atlantic–European sector associated with a climate regime shift (e.g. (Dai et al., 2018; Sarkar and Maity, 2021)). Although the exact causes of this shift remain uncertain, it has been linked to changes in the large-scale background circulation. Dai et al. (2018) suggest that it likely resulted from a complex interplay between oceanic and atmospheric circulation variability. In addition, Sarkar and Maity (2021) hypothesize that Arctic amplification may have favoured a more meridional atmospheric circulation, potentially enhancing Rossby wave breaking and the persistence of cut-off lows, thereby contributing to an increase in extreme precipitation events over Europe.

Lines 201 – 202: This statement needs to be unpacked analytically, in fact. It seems to this reader that MAM to SON contribute the most to the 70s anomaly. Again, in my opinion, it is not sufficient to merely describe the statistics, and an explanation should be offered. Why do we see such a large jump from DJF to MAM?

We thank the reviewer for these insightful comments. We agree that the seasonal behaviour of the 1970s anomaly deserves further discussion.

Therefore, we have extended the Discussion (lines 427–433) to provide a possible dynamical interpretation of these seasonal differences:

The seasonal distribution of COLs found in this paper is consistent with previous studies (Nieto et al., 2005), with a lower frequency of COLs during winter. This behaviour has been attributed to weaker jet streams during summer, which allow greater meridional flow and increase the probability of COL formation (Nieto et al., 2005). The enhanced COL activity during the 1970s is mainly observed during MAM, JJA and SON, suggesting that the circulation changes associated with the 1970s climate regime shift primarily affected the seasons in which the background flow is already more favourable for COL formation.

Lines 211 – 214: COLs during the winter months, though occurring less frequently, can in actual fact, lead to very extreme precipitation. So without showing that this is note the case, this statement is not, supported. Please consider this.

We thank the reviewer for this valuable comment. We agree that, although COLs occur less frequently during winter, individual winter COLs may still produce very intense precipitation events. To address this point, we have extended the seasonal analysis by including the corresponding DJF results in Figures 5 and 8 and discussing them in the Results section.

Lines 224 – 225 and 230 – 236: Is it enough to use rainfall as a measure of COL intensity?

We thank the reviewer for this comment. The objective of this study is not to quantify the dynamical intensity of cut-off lows themselves, but rather to characterize the extreme precipitation associated with them. For this reason, we use precipitation-based metrics throughout the manuscript. We agree that other metrics could be used to describe COL intensity (e.g. geopotential height anomaly or vorticity), but this falls outside the scope of the present study.

Why are the most intense (or extreme) COLs increase over time? This really needs to be explained in detail, please!

We thank the reviewer for this important comment. Following your suggestion, the Discussion has been substantially expanded to better explain the physical mechanisms that may account for the observed increase in the most extreme COL-associated precipitation.

In particular, we now clearly distinguish between changes in COL occurrence and changes in the precipitation produced by these systems. The revised discussion places our findings in the context of recent studies showing that the intensification of extreme precipitation is primarily driven by thermodynamic processes associated with climate warming, including increased atmospheric moisture availability and enhanced precipitation efficiency, whereas changes in COL frequency appear to be comparatively small. We also discuss the remaining uncertainties regarding possible dynamical changes, including the representation of COLs in current-generation ESMs, and have consequently adopted a more cautious interpretation of the projection results (Section 4.2, lines 451–505).

Line 238: The world "been" is missing in the first sentence

Thank you for pointing this out. The sentence has been corrected and now reads (line 296):

COLs have contributed to a large proportion of extreme precipitation events across Europe.

Lines 276 – 279: What exactly is causing the bias in the simulations? This is yet another area that would have benefited from the dynamical understanding of COLs in the study.

Thank you for this comment. We have substantially expanded the discussion of the biases observed in the ESMs and added several references discussing their possible physical origins (lines 525–535):

Similar biases in the simulated spatial distribution of European COLs have also been reported by Mishra et al. (2025). More generally, comparable biases have been identified in CMIP6 simulations of storm-track densities (Priestley et al., 2023), and have been linked to deficiencies in the representation of atmospheric blocking over Europe (Zappa et al., 2014). In addition, diabatic processes play a key role in the life cycle of COLs (Portmann et al., 2018). Priestley et al. (2023) showed that coupled air–ocean interactions enhance cyclone moisture uptake and latent heat release, whereas latent heating associated with condensation is still believed to be underestimated in many CMIP6 ESMs. Model horizontal resolution may also influence the representation of COLs by improving the simulation of horizontal temperature gradients and synoptic-scale circulation (Bracegirdle et al., 2021; Baker et al., 2019; Priestley et al., 2020; Zappa et al., 2013). However, our results indicate that horizontal resolution alone cannot explain the differences in model performance. The absence of a simple relationship between horizontal resolution and model performance suggests that deficiencies in model physics and dynamics also contribute substantially to the simulated biases.

Fig 7 and associated discussion should have the equivalent of or a similar plot to Fig 8 for the period 1981 – 2014 (perhaps extended to 2025, if that is possible), defined here as the current climate in the simulations and the comparison with ERA5 should be a standalone item rather than embedded on Figure 8, it is very difficult to determine who the models are doing in capturing the variability in current climate.

We thank the reviewer for this comment. We believe there may have been a misunderstanding regarding the content of Figs. 7 and 8. Figure 7a already presents the historical evolution of the annual COL counts over the 1981–2014 period for both ERA5 and the selected ESMs, allowing a direct comparison of the simulated and observed variability under present-day climate. Figure 8, on the other hand, shows the evolution of the number of COL-associated precipitation events using the continuous ESM simulations, including both the historical period (1981–2014) and the SSP5-8.5 projections (2015–2100). We chose to display these periods continuously so that the future evolution can be interpreted relative to the simulated historical baseline. To make this clearer, the figure caption has been revised accordingly.

Lines 308 – 309: Again why?

We thank the reviewer for this comment. We agree that the physical interpretation required further clarification. In the revised manuscript, we now better distinguish between changes in COL frequency and changes in the precipitation generated by these systems. Section 4.2 has been substantially expanded to discuss the thermodynamic processes that may contribute to the observed increase in COL-associated extreme precipitation, supported by recent studies such as Calvo-Sancho et al. (2026). In addition, Section 4.3 now discusses in greater detail the possible origins of the underestimation of COL occurrence in current-generation ESMs, based on recent literature, while acknowledging the remaining uncertainties.

As reviewer, I would suggest that Section 4.2 and 4.3 do not add value to the paper. I suggest, again, that the authors undertake a proper dynamical analysis to explain the trends they observe here and also use dynamics to explain the differences between ERA 5 results and models. For instance, only one mode shows consistency with the ERA5 data. I think that there is a dynamical reason for this but it has not been offered.

We thank the reviewer for this valuable suggestion. We agree that the original discussion placed too much emphasis on the statistical results and did not sufficiently address their physical interpretation. Rather than removing Sections 4.2 and 4.3, we have substantially revised them to provide a more physically based discussion of our findings.

In particular, Section 4.2 (*Toward a Better Understanding of Physical Drivers Behind COL Impacts*) has been extensively rewritten to better discuss the mechanisms that may explain the observed evolution of COL-associated extreme precipitation, including recent studies on the respective roles of thermodynamic and dynamical processes.

Similarly, Section 4.3 (*Implications of the Results*) has been substantially expanded to discuss possible dynamical origins of the ESM biases reported in this study and to better place our findings in the context of previous work. While we agree that a dedicated dynamical analysis (e.g. based on Rossby-wave breaking or PV diagnostics) would provide additional insight, we believe such an analysis would constitute a separate study beyond the scope of the present work.

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