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

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.

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.
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.
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?

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?

Specific comments

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

- 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?
- 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.
- 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.
- 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?
- 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?
- L192-196: There is a large maximum over Italy and the Mediterranean Sea. Are any of these Medicanes or other kinds of cyclones?
- 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)?
- L205-209: Given the number grid points, would the 99.9th percentile not just be the maximum value?
- 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 ⅔ months of winter on average!
- 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.

- 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.
- 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.
- L350: “slowing of mid-latitude dynamics” - what dynamics? What do you mean here? Be specific.
- 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.
- 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?
- 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?
- L413-416: In my view, there is not enough evidence in the current analysis to make this statement.
- 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?
- 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.
- 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?