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Interactive comment on "The three-dimensional life cycle of potential vorticity cutoffs: A global ERA-interim climatology (1979–2017)" *by* Raphael Portmann et al.

Anonymous Referee #2

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General comments

The authors present a global climatology of cutoffs for the ERA-interim period. The introduction nicely motivates cutoffs as highly relevant atmospheric features. The study contains several novel and valuable aspects: I commend the authors on taking a potential vorticity and Lagrangian (trajectory) perspective to identify and track cutoffs. This approach enables to authors to provide a global perspective because the different levels on that cutoffs "live" in different latitudes is automatically accounted for. Furthermore, the 3D aspect of the cutoff evolution is of particular interest. A further potentially intriguing result, the classification of cutoff life cycles, however, remains mere specula-

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tion.

My assessment of the manuscript in its current version is much more critical than that of the Anonymous Referee #1 (available online at the time of this writing). To me, the authors miss most of the potential that a feature-based climatology offers to link the evolution of cutoffs to other dynamical features of interest. My main conceptual concern here is that the authors stratify the data with respect to small geographical regions and then present “composites” centered on these regions, instead of centering composites on the identified feature (here: the cutoffs), which is an often-used and more powerful approach to feature-based analysis. The authors’ geographical approach severely affects the quality of this study. Consequence of this approach are that i) only a small percentage of their data contributes to this analysis, casting doubt on the robustness of the results; ii) the relation to other dynamical features is diagnosed in a rather indirect sense and the differences between regions are often not clear enough to provide sound evidence for the authors’ results. Too much of the presentation is thus suggestive, indicative, speculative; iii) pages of presentation are spend on discussing differences between, e.g., cutoffs over the Sea of Okhotsk as compared to cutoffs over the Baltic Sea or the Hudson Bay, making for a tedious, cumbersome read. For a manuscript submitted to a journal with “Dynamics” in its name, I would have hoped that this study provides a much more direct assessment of dynamical characteristics and relationships. I have further concerns with respect to a conspicuous lack of testing the sensitivity of results to (inevitable) free parameters and statistical significance. In addition, I question the accuracy of the authors’ computation of nonconservative tendencies as a residuum from diagnosed adiabatic evolution.

I elaborate on these criticisms below in the section “Critical specific comments”, where I also suggest means to alleviate my concerns. Some of this elaboration duplicates comments given above. In addition, I have several “Major specific comments” for the authors’ consideration. A few “Minor specific comments” are provided to parts of the manuscript that I believe do not need to undergo major revisions, but I refrain from providing comprehensive minor and technical comments on the manuscript in its current form.

Comprehensively addressing my concerns will require substantial further analysis and revisions. I am optimistic, however, that this work will eventually be publishable. It will then make a very valuable contribution to a weather-system perspective on climate dynamics. I leave it to the editor's or the authors' discretion whether the manuscript should be rejected or withdrawn at this point and re-submitted after substantial revisions, or whether these revisions can be successfully implemented during the limited time of the regular review process.

Critical specific comments

1) Stratification by (small) geographic regions

The authors stratify the data almost exclusively by (small) geographic regions of cutoff genesis. As the only motivation for this approach, the authors claim that these "regions are selected each such that a broad spectrum of regions is covered" (line 310). No qualification of this claim is provided. This choice of stratification has several implications that severely affect the quality of the manuscript. i) Most of the presentation reads like an enumeration of characteristics and pages of discussion are spent on describing, e.g., characteristics of cutoffs over the Sea of Okhotsk as compared to those of cutoffs over the Baltic Sea or the Hudson Bay. This approach makes for a tedious, cumbersome read. More severely, it remains opaque to me what can actually be learned from this kind of presentation. ii) For a manuscript submitted to a journal with "Dynamics" in its name, I would have hoped that this study provides a much more direct assessment of dynamical characteristics and relationships. I would find it much more interesting to learn about how cutoff occurrence, characteristics, and associated transport relates to other features of the large-scale circulation, rather than how they differ in several geographic regions. The authors too rarely attempt to make such direct links to other synoptic-scale features or to characteristics of the large-scale circulation. iii) The authors do attempt to relate cutoffs to other features of the large-scale circulation in an indirect way by providing "composites" of relevant other features. (It remains unclear if the authors re-center the data on the cutoffs or if they merely provide averages of fields whenever a cutoff occurs in the respective region in subsection 4.1. This reviewer suspects the latter.) From this approach we learn that in some regions cutoffs co-occur 50-70% of the time with feature A and that the occurrence frequency of feature B is 5% higher than climatology. Accordingly, rarely any strong conclusion can be drawn and much of the presentation needs to remain suggestive and indicative. Overall, much variability is found and if statistical significance testing were applied to differences between regions, I believe that little of the signal would stand that test. Strictly speaking, the presented observations are not incorrect. To this reviewer, however, this choice of presentation gives away much potential of the feature-tracking approach and provides currently little insight.

On a constructive note, I suggest to more fully exploit the potential of the feature-tracking approach by constructing composites centered on cutoffs (as is done in subsections 4.3 – 4.5) and, importantly, stratify the cutoffs directly by their relation to other dynamical features, e.g., co-occurrence of wave breaking or streamers, their distance to and location relative to the jet, etc. Another advantage of this approach is that lagged composites can be created easily, too, such that this approach would tell us much more directly how cutoff life cycles associated with, e.g., cyclonic wave breaking differ from that associated with anticyclonic wave breaking. Another approach would stratify the cutoffs by cutoff characteristics or by their relative location to, e.g., characteristics of the storm tracks. I believe that such approaches centering on dynamical rather than geographical features will substantially enhance the significance of the results and will make for a much more succinct and concise presentation.

2) Lack of sound evidence: Presentation is suggestive, indicative, speculative

This point partly relates to comment 1). In addition, however, further analysis within the chosen approach could avoid speculations made by the authors. In section 3, e.g., the

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authors speculate on cutoff track characteristics based on genesis and lysis location but do not substantiate this speculation with an analysis of the actual tracks (which are available and presented later in the manuscript). Similarly, a claim is made that cutoffs behave differently over land than over the ocean, which could be easily tested with the authors' data. The analysis of cutoff tracks is a further example: Interpreting and drawing conclusions on different track characteristics from visual inspection of Fig. 8 is a stretch, to say the least. I strongly recommend that the authors reduce speculations and hypotheses to a minimum and extend their analysis to substantiate their conclusions whenever possible. Most severely, I find one of the main results, the proposed classification of cutoff life cycles, to be mere speculation. This result appears prominently in the abstract and the conclusions but, strictly speaking, is not a synthesis of the results presented in the main text. Cutoff characteristics in this study are largely characterized by substantial variability. It seems inconsistent that this large variability can be synthesized into the classification of three types of life cycles that then govern other characteristics (stratospheric-tropospheric exchange) also. A sounder analysis would have established statistically significant difference between different aspects of cutoff life cycles before presenting such a strong result. Life cycle type II seems to be particularly dubious. In the results section, one and a half sentences are spent to establish the fact that this life cycle is associated with anticyclonic wave breaking followed by cyclonic wave breaking. I am not aware that this sequence of wave breakings is a well-established concept and I have a hard time to follow the authors description of the respective figure to clearly identify this sequence. The underlying issue is that the classification of life cycles is proposed based on a by-product of mean characteristics in different geographical regions (see comment 1 above). If composites were created, globally not only within the small selected regions, with respect to the wave breaking characteristics, and time-lagged composites were presented that showed the actual mean-evolution, the authors would probably find a much cleaner signal and overall the presented results would be much more convincing. Such an analysis could establish different types of cutoff life cycles based on sound evidence and not on speculation and would be a great result of this study.

3) Robustness and statistical significance of results

The authors do discuss limitations of their tracking method but further discussion and tests of the robustness of the results are missing. There are five major issues: i) About 70% of potential cutoffs are dismissed based on an arbitrary criterion (section 2.3.2). No doubt there is a need to filter out spurious cutoffs. The authors filter out cutoffs that have small vertical extent based on the flawed argument that these cutoffs are not "dynamically relevant“. Theory does not support this argument. According to QG theory, the aspect ratio of a PV anomaly determines the ratio between the associated stability and wind anomalies, but not the extent to which the anomaly is "dynamically relevant“. The vertical penetration depth, e.g., depends on the horizontal (not the vertical) scale of a PV anomaly. And the magnitude of a PV anomaly is directly related to the magnitude of the stability and wind anomalies. These two characteristics would thus be more suitable to identify dynamical relevance. In any case, for any free parameter that is introduced to filter out a large amount/ the majority of potential data, the robustness of the results to reasonable variations of this parameter needs to be demonstrated. ii) After showing the geographical locations of genesis, lysis, and tracks (not that I am mistaken: I consider this an important documentation of the occurrence characteristics of cutoffs.) the authors restrict their analysis to small geographic subregions during DJF and JJA. These subregions constitute maxima in genesis frequency, but no information is given what percentage of total cutoffs occur within these regions. Visual inspection of Fig. 4 suggest that this percentage is low. (It’s hard to estimate a number but I guess less than 20%.) Cutoffs during MAM and SON are not considered. So overall only about 10% of the data is used to draw conclusions from. In combination with the lack of justification of why these 10% are selected I fail to see how the results can be considered to be robust and how the results would meet the expectation of a global climatology, as suggested by the title. iii) Basing the tracking of cutoffs on trajectory calculation is a major strength of this study. The trajectories are used also to determine the adiabatic evolution of cutoffs. Nonconservative contributions to the evolution, including stratospheric-tropospheric exchange, are estimated as the difference between the observed and the diagnosed adiabatic evolution, i.e., as a residuum. Estimating nonconservative terms as a residuum requires a very accurate diagnosis of the conservative dynamics and is usually associated with substantial uncertainties in the results. No discussion of this issue is provided in the manuscript. Trajectories are calculated for 6h, which is seemingly short but in fact 6h is the temporal resolution of the underlying data. This means that trajectories, irrespective of the time step used in the calculation, are based on linear interpolation between the start and end times. Linear interpolation is a poor approximation when the displacement of strong gradients needs to be represented, here: the movement of the strong PV gradient and associated winds associated with the jet, wave breaking, and the cutoff itself. In addition, any nonlinearities in the adiabatic 6h evolution will erroneously be diagnosed as nonconservative contributions. Such nonlinearities can be expected during genesis (wave breaking) and reabsorption of cutoffs and I wonder to what extent putative maxima of nonconservative processes at the beginning and the end of cutoff life cycles, e.g. the u-shape signal in Figs. 16 and 17, are related to inaccuracies in the trajectory calculations. The robustness of the results should here be demonstrated preferentially by using higher-resolution ERA5 data for a sufficiently large subset of cases or, as a less accurate but computationally less expensive option, by reducing the temporal resolution of the ERA-interim data to 12h. iv) The authors examine stratospheric-tropospheric transport (STT and TST) based on 6h trajectories. Previous authors considered results based on this relatively short time scale as spurious and advocate using longer time scales. For this issue, the authors do provide a discussion in their final summary and discussion section. To avoid readers' confusion, I would find the discussion more helpful closer to the method or the results section. More severely, I am not sure that I find the comparison of rather general characteristics with previous results to be convincing enough to demonstrate that the transport characteristics diagnosed in the current manuscript for different "types" of cutoffs is robust to the time scale used in the trajectory calculation. A sound test of the robustness would extent the trajectories used in the analysis to longer times, again at least for a sufficiently large subset of cases. v) There is a complete lack of testing the statistical significance of characteristics of cut-offs in geographical regions/ different types of cutoffs. This lack is significant because the descriptions/ results rely on the statistics of the data only and it is often not clearly evident to the trained eye that differences are sufficiently large. The revised version of the manuscript requires such tests.

Major specific comments

1) The conclusion that can be drawn from this study are hard to identify

The final section contains both, an extensive discussion of results in the light of previous studies and the conclusions drawn from the current study. As a consequence, the final section is unusually long and the summary is little helpful for a reader who is interested in the main outcomes of this study. To me, most of the discussion of previous result would have been more useful in the results section, after presenting the relevant result. This re-organization of the text would improve readability of the results section because more context for the results presented therein is provided. More severely, in the current version, it is hard to identify what the main results of the current study are. If the authors chose to keep a long final discussion section, then at least final conclusions should be clearly separated from the comparison with previous studies.

2) Section 3

It is hard for me to see what we learn in section 3. I understand the importance to show and describe the geographical distribution of feature occurrence and characteristics. In its current version, however, the presentation is mostly show and tell and thus rather unsatisfactory. One issue that can be remedied is that the authors partly use very specific geographic references (e.g. New Foundland, Iceland). First, an accuracy of the analysis is suggested that I believe is not robust to reasonable changes of free parameters used for cutoff identification. Second, using more general geographic references (over land – over ocean, subtropics – polar – midlatitudes) seem more related to potential differences in the dynamics. Most preferably, a description in relation to large-scale features (start – end of storm tracks, at the edge of subtropical anticyclones) would be given.

3) Subsection 4.1: Cutoff genesis

Unfortunately, I have a very hard time to follow the authors' arguments in this subsection based on occurrence frequencies of other features. In addition, I struggle to clearly identify the features described in the text in the figure. In particular, I struggle to identify wave breaking, which plays a crucial role in the authors' argument. Do the authors mean to indicate different types of wave breaking by tropospheric and stratospheric streamers? Inter alia in this particular subsection the authors draw strong results based on rather speculative arguments. To illustrate the temporal evolution leading up to genesis, time-lagged composites (going back in time) could be created, in a feature-centered framework e.g. centered on genesis location. Similar composites are used later to illustrate temporal evolution and are very helpful (Figs. 10 ff.). I recommend substantial revision of the complete subsection.

4) Subsection 4.2: Track analysis

Visual inspection alone is insufficient to draw conclusions about different types of tracks. A cluster analysis of tracks, or at least showing the average tracks of cutoffs in distinct end regions are required to make a point here.

5) Subsection 4.3 – 4.5 are the strongest result subsections but still suffer from the general critical issues noted above.

6) Subsection 4.6: Link to surface cyclones

There have been decades of research based on first principles to understand the interaction between upper-level PV anomalies on surface cyclones. What do we learn here about this interaction by considering mere proximity of the systems? What aspects of the interaction do you consider? Why is it relevant to consider the age of the cyclone? In what sense does the analysis provide new insight? What is the significance of this result? I see potential in this subsection but there is a severe lack of motivating the important I questions and why proximity of the systems is a useful metric. Potentially the issue here is mostly a matter of wording. The current version, however, leaves the reader guessing why this subsection would provide interesting results.

Minor specific comments:

Fig. 1 is very helpful, but Fig. 2 did not help me to better understand the tracking procedure.

The introduction reads in general very well. Except for the two middle paragraphs on page 3. There it is not clear to me what the open question is and/ or how the respective paragraph motivates the current study. Please clarify.

How do you handle a lost child? Are they simply ignored? If yes, how much of the volume of cutoffs are dismissed due to a lost child?

I fail to understand the first half of subsection 2.3.2 (Construction of tracks)

What motivates your choice of definition of cutoff area? Why not using the vertical average of the area on the individual isentropes? A single isentrope with large area would dominate your metric and I would not consider the area as representative in that case. Or take, e.g., a tilted structure with a similar area at all levels. Your method artificially enlarges these features. Please add some discussion for clarification.

Avoid technical language when describing a physical feature. E.g., the term “cutoff track” is often used to describe what I believe is simply the life time of the cutoff (e.g., line 301 and in subsection 4.3).

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