The three-dimensional life cycle of potential vorticity cutoffs: A global ERA-interim climatology (1979–2018)

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Author comments

We thank the three reviewers for their careful evaluation of the revised manuscript. We are pleased to see that this version has been more convincing and easier to read than the initial submission.

Additional minor changes have been made to the manuscript according to the reviewers' comments. We address all comments separately below. The comments by the reviewers are given in black and our replies in blue.

Reviewer 1: accept as is.

Reviewer 2:

Prelude

First of all, it is important for me to state that the tone of my first review was certainly not meant to be harsh. In fact, I took time and efforts and had tried to maintain a constructive note throughout (although, to me, the initial manuscript was a long, tedious read). It appears to me that my suggestions, which meant to be constructive, were misinterpreted as an attempt to impose my "personal viewpoint of how such a climatological study should have been performed". Of course, the authors are free to perform their analyses to their liking and to focus on aspects that they consider to be the most relevant for the current study. My reviewer's obligation, on the other hand, is to check the evidence that is provided by carefully understanding the authors method, description of figures, explanations and conclusions drawn. If I as a reviewer fail to do so too often, I feel obliged to provide (very) critical comments

The tone of my initial review might have been very critical at times because I *was* very critical about the initially submitted version. I am not sure that I consider it helpful, though, to point out a critical tone of a critical review in an authors' response. As a reviewer I can only evaluate the manuscript at hand, i.e., the information, explanations, motivations, interpretations provided in the manuscript. Lack of clarity in writing, omission of information, etc. may add up to the extent that the intentions of a study do no longer become sufficiently clear to a reader. From the authors' response I gather that some of the main intentions of this study did not become clear to me, which has certainly led to much criticism from my side. I understand that the authors may thus have considered my first review to be overly critical. I would hope that the authors understand that my misinterpretations of their initial manuscript were at least partly due to lack of information and clarity and a suboptimal organization of the presentation of the material in the initial submission.

A word on "expectations": I had used this term somewhat carelessly – or at least ambiguously – in my first review. I certainly did not mean to imply that the authors need to meet my expectations as in standards that would need to be met. I meant to use the term to refer to a tacit context that one may

have in mind when reading a manuscript. That tacit context was formed for me by the name of the journal (including the term “dynamics”) and the title of the manuscript (“global climatology”). Without grasping the motivation of the authors, I had evidently a very hard time to fit a detailed and mostly descriptive discussion of very regional aspects into that tacit context. I agree that my wording to express this idea in the first review was poor.

We thank the reviewer for his/her remarks. We fully agree with the reviewer’s view about the obligation to provide critical comments. In fact, most of the comments helped us a lot in reshaping and improving the paper, in particular its structure and the readability of the results.

Comments on the revised version

The manuscript has improved tremendously in all aspects. This reviewer finds it extremely helpful that there is now a clear distinction between the global aspects of the climatology (as heralded in the title) and regional aspects. In addition, there is now a clear motivation of why these regional aspects are of interest and why detailed descriptions are well justified. Furthermore, I find it extremely helpful that the classification of life-cycle types is now based on the global data and that there is more discussion of relevant previous work that puts the authors contribution into much better context than in the previous version of this manuscript. More, clearer, and better motivated links to other atmospheric phenomena, both in terms of the larger-scale circulation and potential impacts, further strengthen this study. Adjustments to the filtering of the data and some reference to statistical significance round off what is now a very well written quality manuscript.

Basically, the manuscript could be published as is. I have one remaining question, though, and addressing this question may somewhat alter one of the authors’ conclusion. I thus recommend accepting this manuscript for publication after minor revision. In addition, I have a few suggestions for the authors’ consideration that may help to provide full clarity and further improved readability for future readers.

Thanks for this very positive assessment of our manuscript. We respond to the mentioned aspects below.

Minor comment

Sect. 3.2: The authors compare diabatic decay with reabsorption. I would think that diabatic decay, i.e., diabatic PV erosion, is a gradual and relatively slow process whereas reabsorption is identified at the moment when the 2PVU contour merges and thus I would thus think that this is a very fast process. If my reading is correct, the authors evaluate the two processes instantaneously at the time at which a PV cutoff disappears. Then the relative importance/ frequencies of the two processes are compared. It seems to me, however, that the instantaneous evaluation of the gradual diabatic PV erosion does not do this process full justice, i.e., that the relative importance of processes is biased towards reabsorption. I understand that the 3D cutoff may gradually decay diabatically because the definition here is layer-wise on each isentrope. But does this layer-wise perspective fully take into account the gradual nature of diabatic erosion? My feeling is that a fairer comparison would integrate both processes over the lifetime of the cutoff. I believe that it would be helpful if the authors commented on this issue, in particular because their conclusion based on this result is rather different from synoptic experience (as noted by the authors).

Thank you for mentioning this aspect. First, we would like to note that we do integrate decay and reabsorption along the entire lifetime of the cutoff. Let’s look at the following example: A PV cutoff

forms at 310 K and 320 K and diabatic decay occurs after 2 days at 310 K but the cutoff persists at 320 K and is reabsorbed at that level after 3 days. In this case, our approach both considers one event of diabatic decay and one of reabsorption.

However, we agree that, considered on a single isentrope, diabatic decay is a gradual process and identifying decay as disappearance from an isentropic surface does not account for the fact that the actual loss of mass may have occurred at an earlier time. Viewed on a single isentrope, a PV cutoff may start to shrink (i.e., lose mass due to stratosphere-to-troposphere transport) several hours/days before it actually disappears from the isentropic surface. Also, it can occur, for example, that a PV cutoff shrinks for a certain time period but grows again thereafter (due to troposphere-to-stratosphere transport) and is finally reabsorbed. We quantified these potentially complex evolutions of "shrinking" and "growing" with our STT/TST diagnostic, which we presented in the original paper but removed in the current version to enhance the focus of the paper. These results, however, are documented in the openly available PhD Thesis of the first author: https://www.research-collection.ethz.ch/handle/20.500.11850/466735). For the analysis presented in the paper, we argue that growing and shrinking of a PV cutoff is different from the actual lysis scenario (decay and reabsorption). Of course, this is clear for reabsorption, but it may appear a bit artificial for decay (as decay occurs basically at the end of a period of strong shrinking).

We reconsidered this aspect but still, in our opinion, defining decay and reabsorption as two different scenarios of how a PV cutoff can "disappear from an isentropic surface" is the most appropriate and fairest one. This definition does to some extent incorporate the gradual nature of both, decay and reabsorption, in the sense that it identifies these scenarios separately on each isentropic surface. Our approach can capture the evolution of a PV cutoff that decays first on, e.g. 310 K, then 315 K etc., as well as of a cutoff that is reabsorbed first on 330 K, then on 325 K, etc.

Finally, there may be several reasons why the result that reabsorption is as frequent as diabatic decay deviates from synoptic experience:

1) Synopticians may focus on "classical cutoffs" equatorward of the jet stream for which decay is indeed more frequent than reabsorption.
2) They may focus on lower isentropic levels, on which the cutoff is typically well separated from the main stratospheric reservoir. At higher levels, where the cutoff is closer to the reservoir (because there the reservoir larger), reabsorption is more frequent and can also occur transiently (i.e. attachment to the reservoir is followed by detachment). The PV evolution on these levels often looks "messy" and complicated and most PV cutoff studies therefore focus on the lower levels where diabatic decay dominates.

Comments for further consideration

Title: The authors may want to consider extending the title to point potential future readers also to the regional aspects, which form a substantive part of their study.

Thanks for this suggestion. We adopted the title to: The three-dimensional life cycles of potential vorticity cutoffs: A global and selected regional climatologies in ERA-Interim (1979-2018)

l24: suggest adding „as defined above" after "types" for full clarity.

Suggestion has been adopted.

top of page 7: For full clarity: It would be helpful to explicitly define STT/TST as the PV change of an air parcel that crosses the threshold of 2 PVU because not every reader may be familiar with this. (I might have missed such a definition above in the manuscript.)

Thanks for pointing this out. We added the following sentence: “Note that here, STT is defined as the Lagrangian PV change of an air parcel from more to less than 2 PVU, and vice versa for TST.”

Sect. 3.1: I fully agree with the authors that the in-depth discussion in this subsection is justified. For the reader’s convenience, the authors may want to consider introducing subsubsection for, e.g., frequencies, genesis/ lysis, and comparison with previous studies. The discussion at the end of this subsection now motivates very well the classification of life-cycle types presented below.

Thanks for this very useful suggestion. We introduced subsubsections to better structure this rather long subsection.

l407: “During lysis of 3D PV cutoffs”: It would be helpful to clarify: Is this the instance at which the 3D cutoff completely disappears, e.g., the moment at which the cutoff disappears from the last remaining isentrope?

Thanks for suggesting clarification. We added the following statement: “During lysis of 3D PV cutoffs, i.e., if only the last time step of each PV cutoff life cycle is considered, …”

l409ff: reabsorption occurring predominantly at higher levels: Just a thought for the authors’ consideration: What you describe here, is that consistent with the partial erosion of PV at the lower levels of the 3D cutoff and the final decay by reabsorption then being associated with the remaining parts of the cutoff at higher levels?

Thanks for this way of rephrasing. Yes, this is a possible evolution. First diabatic decay at lower levels and then reabsorption at higher levels. However, it could also be the other way around: reabsorption occurs first at higher levels and then decay at lower levels. It would be an interesting further study to look at the temporal sequence of decay and reabsorption during the life cycle of a 3D PV cutoff in detail.

l420: for full clarity: I suggest adding „of the 3D cutoff“ after “life cycle”.

Thanks for this suggestion, we adopted it.

Sect. 3.3: denomination of life-cycle types: The authors may want to reconsider their life cycle names and consider introducing more telling acronyms, e.g., just for illustration, types POL, 2J, EQ. Not being familiar with the authors’ life-cycle terminology, I found myself switching back and forth while reading to remind myself how types I, II, III are defined.

We agree, roman numbers are not intuitive. However, we also failed in finding really good acronyms and we therefore decided to remind the reader once per section about the meaning of types I, II and III by writing “Typ I (equatorward of jet), Typ II (between jets), Typ III (poleward of jet)".

l581: “active cutoff tracks“. I am not sure I fully understand the meaning here. Consider revising.

Thanks, we clarified with "(…) number of cutoff tracks that still exist at a certain time after genesis (hereafter: active tracks)".

l736ff: The discussion here reminds me on a third type of baroclinic life cycle that was noted in Shapiro et al.'s chapter in the book The Life Cycle of Extratropical Cyclones. The third author of this study was a co-author of that study so he can best evaluate if reference to that work is relevant here.

In retrospect, introducing LC3 in this book chapter was maybe misleading, and the suggested 3-type terminology has not been picked up by the community. We think it is safer to not refer to LC3 in our paper.

Reviewer 3:

This is my first review of this manuscript, although it has already been through one round of reviews.

This is a thorough and comprehensive analysis of PV cutoffs that includes consideration of their 3-d structure, which has not been attempted before. The method used to track the features (trajectory analysis) also enables the authors to determine in what way the PV cutoffs are disappearing at the end of their lives – do they decay or are they reabsorbed. The method is complex but is very clearly described, and I appreciated the schematic diagrams to help with this. A global climatology is presented, which shows the locations of cutoffs and their general characteristics. The features are then grouped according to their position relative to the jet, which yields three quite distinct classes with different characteristics and surface impacts. I particularly like that this way of categorizing the PV cutoffs allows the authors to demonstrate the importance of the two baroclinic life cycles, LC1 and LC2. Finally the authors show variations in the PV cutoffs in different geographical regions. This is complementary to the other type of classification used.

I know the manuscript has undergone significant reworking after the first reviews, and I find it to have a very clear structure. The conclusions are nicely presented to demonstrate the key takeaway messages of the paper. Overall I very much enjoyed reading it and think the dataset will be very useful to the community.

I only have a few very minor suggested edits.

Thank you very much for this feedback on our work and that you enjoyed the reading. And many thanks for accepting to carefully read the paper in the second round. We address your minor points below.

1. Line 57: Meditarranean > Mediterranean.

2. Line 163: do > to?

3. Line 173: foreward > forward.

Thanks for the above corrections, we adopted them.

4. Lines 203-205: I find this sentence confusing. Please can you reword or clarify?

We are not 100% sure which part is confusing. We tried to rephrase the sentence and hope it is clearer now.

5. Lines 407-408: I think this sentence needs some more commas to be clearer.

We added two more commas: “During lysis of 3D PV cutoffs, reabsorption is, with a share of 54 %, even a little more frequent than diabatic decay”

6. Figure S1 – I think this would be useful to have in the main paper rather than in the supplement.

We agree that Figure S1 is important and could also be placed in the main manuscript. However, the manuscript has already 17 Figures, which is quite a lot. In addition, there is some redundancy in the information in this Figure and Figure 3 (both contain climatological frequencies of cutoff occurrence). Therefore, we decided to reduce the information on the frequencies of the three types to descriptions in the text with reference to the supplement.

7. Line 669: Is the 10% value given found by adding the values in the two boxes (0.5-1 and 1-1.5)? I was a bit confused initially – could you clarify this?

Thanks for pointing towards this slight confusion. We clarified it.

8. Line 674: Sentence starting “A similar scenario…” is unclear and I think needs rewording.

Thanks, we rephrased this sentence.