“The Life Cycle of Upper-Level Troughs and Ridges: A Novel Detection Method, Climatologies and LagrangianCharacteristics”

Reply to review #1

We appreciate the positive comments from the Reviewer and also the critical and detailed comments made, which have led to an improved manuscript. In response to the comments made by you and the other Reviewers, we have made considerable changes to the text and figures, and we will mention the relevant ones in our point-by-point response. We also uploaded a tracked-changes version of the manuscript.

Reviewer: My biggest concern is with the authors’ choice of 500 hPa geopotential height (Z500) as the variable for analysis of so called "upper-level" troughs and ridges. As far as I can see, no justification is provided for the choice of this variable. Historically Z500 was widely used in the 1970s and 1980s for analyses of troughs and ridges. However, since PV thinking became mature (cumulating in the seminar paper by Hoskins et al. 1985), synoptic dynamicists have generally accepted that troughs and ridges are manifestation of PV anomalies that have largest amplitudes either at the tropopause or at the surface, and in recent decades, most analyses have focused on analyzing variables either near tropopause level or near the surface (e.g. Hoskins and Hodges 2002, Fig. 1). In this paper, the authors pick the mid-level (500 hPa) for analyses of upper level troughs. Can the authors please provide justification why they pick a mid-troposphere variable instead of an upper troposphere varable to analyze?

Authors: The 500 hPa geopotential height (Z500) has traditionally been used for analysis of troughs and ridges. It is still used amongst forecasters and researchers when describing the synoptic-scale weather evolution in terms of high- and low-pressure systems, and troughs and ridges. Because of its historical significance in synoptic meteorology, we have chosen Z500 in this study for demonstration purposes. Depending on the specific research question, researchers may choose other suitable variables or levels as input data. For example, potential vorticity (PV) or potential temperature on the dynamical tropopause. The use of Z500 in our manuscript is not a statement on its superiority over other variables. The equation used for the computation of the quasi-geostrophic omega forcing however, which is shown in several figures throughout the manuscript, relies on the geopotential height. The combination of the quasi-geostrophic forcing of vertical motion and the geopotential-based troughs and ridges yields a powerful research tool in addition to its use in descriptive synoptic meteorology.

- We supplement the revised manuscript with the results for Z300 (as supplementary figures) and comment briefly on changes between Z500 and Z300 where appropriate.
- We further added a statement that clarifies the use of Z500 for demonstration purposed and that the user of our tool is free to choose other suitable variable or levels depending on the exact research question.

Reviewer: Also, why use curvature of geopotential height contours? Why not use relative vorticity or PV instead? Given that dynamically, we can easily write equations for vorticity or PV tendency while it is difficult to write an equation governing the tendency of curvature of Z500 contours, what are the advantages for picking such a variable to analyze? Can't trough/ridge axes be defined based on vorticity or PV?

Authors: Our approach is to identify troughs and ridges using geometric means in an effort to mimic the human eye. Geometric means have successfully been used to identify streamers (e.g., Wernli and Schwierz 2006), Rossby wave breaking (e.g., Barnes and Hartmann 2012, and many others), Rossby wave initiation (Röthlisberger et al. 2015) and also surface cyclones and anticyclones (closed SLP contours are used in many routines; Neu et al. 2013). Potential vorticity and its curvature could also be used as input data and the user of our tool is free to choose PV instead of Z. In general, vorticity could also be used, but given its rather uneven and noisy distribution over neighboring regions it comes with its own difficulties. In high-resolution NWP data, in particular, the many small-scale relative vorticity maxima become a hindrance in unambiguously identifying troughs and ridges. This is somewhat similar to the challenges of identifying cyclones in high-resolution data based on relative vorticity.

Reviewer: A potential issue is that results can be affected by high terrain, e.g. the Tibetan Plateau. Previous studies (e.g. Chang and Yu 1999, Hoskins and Hodges 2002 (HH02 hereafter); Hakim 2003) have shown that there are upper level waves that propagate along the subtropical jet in winter near the southern edge of the Tibetan Plateau. These waves are clearly missing from this study (Fig. 3a and 4a). These waves are potentially important in understanding Pacific cyclongenesis (e.g. Chang 2005) and the mid-winter suppression (Nakamura and Sampe 2002).

Authors:

(i) The 500-hPa indeed intersects the Tibetan Plateau and other high mountain ranges such as the Rocky Mountains and the Andes. Caution must therefore be given when interpreting the results over these regions. Troughs and ridges are therefore not computed at times and places where missing data values are present in the data.

(ii) It is correct that the North Pacific storm track is fed by two upper-level seeding branches, one coming from the Siberian storm track and another one along the subtropical jet (HH02, Chang 2005). However, it is the northern seeding branch that dominates surface cyclogenesis between Nov–Mar (Hakim 2003, Chang 2005 see discussion on p. 2004/05). The waves along the southern branch are best seen in bandpass-filtered data at 300-hPa along 25° S, typically in composites preceding cases of rather deep cyclogenesis (Chang 2005). Sometimes both seeding branches even interact (Fig. 4 in Chang 2005). In the seasonal mean climatology, we do not see these waves at the 500-hPa level. Most likely the key difference is the time filtering, which excludes waves that act on time scales longer than the typically synoptic times. Thereby these climatologies emphasize a subcategory of eddies, which otherwise are masked in unfiltered climatologies. This subcategory of waves however is still part of our data, as is shown below for a trough-ridge train along the southern seeding branch between 60–90° E and near 120° E on 26th January 2017. The downstream trough and ridge even connect the northern with the southern seeding branch, which is in agreement with Fig.4 in Chang (2005). In the revised manuscript, we placed a comment on this in the corresponding section.

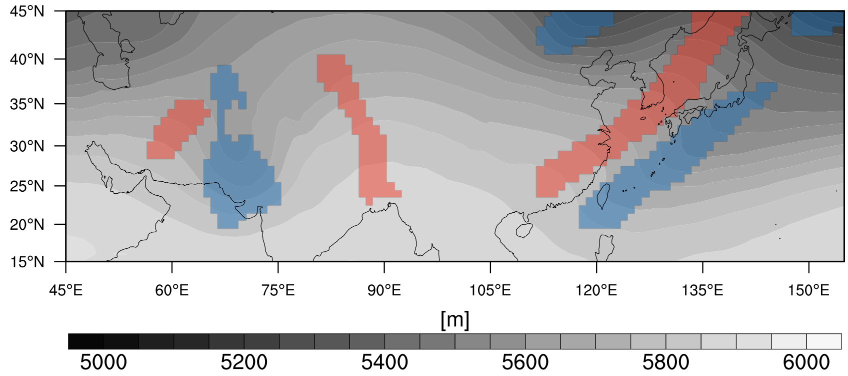

Figure 1: Troughs (blue) and ridges (red) along the southern seeding branch on 26th January 2017.

Reviewer: While the authors discussed some consistencies with previous studies for summer climatology (section 3.2), for the much more researched winter, they didn't provide much comparisons with previous studies. More reference to previous studies should be made in section 3.1. There are some differences that could potentially be due to differences in methodologies. For example, comparing their Fig. 3a to Fig. 11d of HH02, it is apparent that the authors' results are quite a bit further north than those presented in HH02. Also, the authors' results (Fig. 4a) show nearly absence of ridges to the south of Japan, while HH02 shows that 250 hPa Z positive tracks have a maximum south of Japan over western Pacific. Can the authors please explain what might be the reason(s) behind such differences? In any case, comparisons with results from previous studies should be discussed more.

Authors: Thank you for pointing us towards this imbalance between the discussions of the summer and winter climatologies. In the revised version, we now make more comparisons with previous studies, in particular with climatologies of cyclone frequencies and streamers. We do not expect Z500-based troughs and ridges to yield similar climatologies as for bandpass filtered 250-hPa Z eddies (HH02), see our discussion of the southern seeding branch over the Pacific above.

The more poleward location of our climatology (Z500) compared to Fig.11d in HH02, which is based on bandpass-filtered 850-hPa meridional wind, relates to the general westward and poleward tilt with height of baroclinic waves and the relative location of the 500-hPa trough to the surface warm sector.

Note that there is a good agreement between the genesis and lysis hotspots found by HH02 (Fig.5c, d) with the trough and ridge frequencies in our climatologies.

Reviewer: In my opinion, the methodology is not described in sufficient details. How the tracking was done was not really presented and readers are referred to a Ph.D. dissertation (221 pages long). This seems to me an important step since one of the parameters the authors emphasized is age of the system which depends critically on the tracking algorithm. Details of how tracking is done should be presented - perhaps as an appendix or supplemetal material.

Authors: In the revised version, we added a full new and detailed paragraph on the feature tracking in the method section. We also point the reader towards specific pages in the Ph.D. thesis were necessary.

Reviewer: As the authors mentioned, there are always rather arbitrary cutoff values in any objective algorithms. For this algorithm, they mentioned three - the minimum curvature, the size of the trough/ridge objects, and the length of the axis. How sensitive are the results of these choices? How are these choices set? For example, in the example shown in Fig. 2, the trough over the Mediterranean in panel 2a looks to me a very legitimate trough, but it was not identified. I would guess that even 6 hours earlier that trough was already apparent. So, the question is: what is the justification for the authors to pick those particular cutoff values? This is important for genesis and lysis, and for trough ages and lifetimes. In my opinion, sensitivity to these cutoff parameters should be explored and discussed.

Authors: There is unlikely an ultimate answer to this question. Even trained experts will not agree in every situation on the presence of a specific flow feature, which is a lesson learned from the discussion surrounding the detection of fronts (e.g., Schemm et al. 2018), but we do agree that it is important that the cutoff values are discussed. In general, the patterns seen in the winter and summer climatologies (Figs. 3 and 4) are robust. Reducing/increasing the thresholds will emphasize or de-emphasize the centers of action, i.e., detection frequencies will increase or decrease, but they will not change locations. Note also that the thresholds might easily be adjusted for individual case studies to obtain an optimal match to a subjective analysis. This, however, is far more challenging if a climatology of troughs and ridges is compiled. On the other hand, the amplitudes in a climatology might be sensitive to the chosen thresholds to some degree, but we expect the general pattern to be rather robust.

The example in Fig. 2 was *not* used for the tuning of the cutoff values in the algorithm. It was chosen intentionally because we want to present with maximum transparency the difficulties arising from the need to define these cutoff values. This transparency would be missing if we would present a “perfect” case that was used for the tuning (in fact we used, among others, the cases shown in Bue and Xie (2015)). If we reduced the cutoff values to catch the trough already 6 hours earlier, we could almost certainly identify another situation where we would tend to increase the cutoff value again. We believe that it is important that the cutoff values are openly communicated and they are discussed in the method section and also in the illustrative case example.

- Bueh, C. and Z. Xie, 2015: An Objective Technique for Detecting Large-Scale Tilted Ridges and Troughs and Its Application to an East Asian Cold Event. Mon. Wea. Rev., 143, 4765–4783, https://doi.org/10.1175/MWR-D-14-00238.1
- Schemm, S., M. Sprenger, and H. Wernli, 2018: When during Their Life Cycle Are Extratropical Cyclones Attended by Fronts?. Bull. Amer. Meteor. Soc., 99, 149–165, https://doi.org/10.1175/BAMS-D-16-0261.1

Reviewer: Many objective tracking algorithms (e.g. HH02) employ other cutoff values, like feature lifetime and distance travelled. As far as I can see no such cutoffs are employed here. Could the statistics be heavily contaminated by very transient short-lived features like heat lows, or quasi-stationary climatological features?

Authors: We believe that it is natural for quasi-stationary features to dominate the climatology and we regard their presence not as a contamination. They are legitimate members of the trough and ridge family. The transient troughs and ridges are also contained in our data. If necessary, to address a specific research question, the stationary features can easily be removed from the data in a post-processing step or bandpass filtered input data could be used.

Heat lows are excluded. These features are associated with closed contours at the 500-hPa level and are removed from the data. Additional cutoff values like lifetime and distance travelled, which are invoked by some cyclone tracking schemes, are legitimate and can be useful, but they will introduce new degrees of freedom resulting in the same need for a justification as discussed above (How far travelled? How long-lived?). In fact, to obtain the frequencies for troughs with an age between 0-24 hours (Figs. 3 and 4), the trough objects are filtered according to their lifetime.

- The revised manuscript contains a supplement with the trough and ridge climatologies but with short-lived objects removed (a minimum lifetime of 24 hours is required) – see Fig. S3

Reviewer: Some of the statistics shown are not clearly defined. For example, what does trough detection frequency mean? Is that defined based on trough axis or trough objects? Also, what does the "selected frequencies of troughs with an age between 0 and 24 hours (yellow)" in the figure caption of Fig. 3 mean? Without clear definitions of these parameters it is difficult for readers to interpret these figures. Mathematical definition should be provided.

Authors: Thank you for pointing this out.

(i) Detection frequencies are based on the 2d objects. Grid points outside every coherent 2d object are flagged with zeros, grid points insides with ones. Time-averaging over the obtained binary fields yields detection frequencies, which indicate the fraction of time steps affected by a trough or ridge relative to all time steps. We now provide this information in the method section.

(ii) (ii) The single yellow contour denotes the frequency that is half the maximum detection frequency for troughs with an age between 0 and 24 hours. To this end, we remove all 2d trough and ridge objects that are older than one day and re-compute the detection frequency based on the remaining troughs. The yellow contour is half the value of the maximum detection frequency of the thereby filtered troughs.

Reviewer: For the trough age, it is surprising to see that eastern Pacific/Gulf of Alaska is apparently a region where there is high frequency of 0-24-hour troughs (Fig. 3a). I would imagine only regions where there is frequent trough genesis would be highlighted, and that is certainly not a region that is associated with frequent trough genesis. As far as I can see the authors did not mention that region. Can the authors please discuss that?

Authors: The Gulf of Alaska is in agreement with climatologies of streamers (Fig.3 in Wernli and Sprenger 2007) and cycloysis (Fig.5d in Hoskins and Hodges 2002). These references are now mentioned in Section 3.1. Our interpretation is that trough genesis in the Bay of Alaska is resulting from wave breaking and consecutive downstream development during the decaying phase of mature extratropical cyclones over the central Pacific. In this sense, the Bay of Alaska shares some similarities to the trough genesis region over the UK, which, in our opinion, is also a follow-up development driven by upstream decaying mature extratropical cyclones over the eastern North Atlantic.

Reviewer: Apart from the Gulf of Alaska, it seems that "young" troughs and ridges are most frequent over regions with high trough and ridge frequencies (Fig. 3 and 4). Those regions are expected to be regions where troughs and ridges are quasi-stationary and hence presumably "old" rather than "young". Nevertheless, it depends on what the yellow contour really shows as it is not really defined, but this point should be further discussed.

Authors: This is a good point. Downstream of mountains troughs are frequent and thus quasi-stationary, but downstream of mountains troughs are also generated due the flow across the mountain ridge. We believe it is not too surprising that downstream of mountains a trough genesis region is identified. Locally, both the genesis and lysis frequencies are large and troughs growth and decay depend on the flow across the mountains, but they remain stationary at the same time.

Reviewer: In the Lagrangian analysis, one of the authors' goals is to quantify the diabatic impact (p. 14, line 8). My question is whether 500 hPa level is the best level to do that? It is still within the cloud layer at a location where strong heating is still going on. Wouldn't the full impact of diabatic heating be clearer at the tropopause level?

Authors: A comparison between the results based on 500-hPa and 300-hPa troughs and ridges would indeed be insightful. We agree that the full diabatic impact integrated across the entire depth of the troposphere will be more complete if the parcel trajectories were released at a higher level. However, we expect to catch a major part of the condensation, which peaks typically between 850–700 hPa, and also the below-cloud evaporation. Indeed, the trajectories will to a much lesser extent experience diabatic modification due to, for example, freezing. However, if consideration is given to diabatic modification of 500-hPa trough and ridges, it seems natural to release the parcel trajectories from this level.

Overall, this section gives a unique look into a potentially powerful combination of two tools (troughs & ridges plus parcel trajectories), which opens new possibilities for exciting research, but this proof-of-concept is far from being compete. An in-depth analysis would also require several years of trough, ridge and trajectory data and a comparison between trajectories of different lengths and released from different levels or at different trough ages. This would be a study of high scientific merit, in particular if diabatic tendencies from different microphysical processes were available, but a full-fledged stand-alone study on its own would be needed.

Other Concerns:

Reviewer: i) p. 4, line 21: 20S-70N: The authors discussed that they focus on mid-latitude troughs/ridges, but apparently did the analysis in the tropics also. In the tropics, wouldn't the fields be very noisy given the weak geopotential gradient? Why is it necessary to perform the analysis down to 20S?

Authors: The frequency of trough detection drops to zero between 20–25S, the tropics are therefore climatologically not relevant. However, sometimes streamers might elongate far equatorward and we think it is useful to catch those cases as well.

Reviewer: ii) p. 6, lines 4-5: I'm not sure I understand what the authors meant by the sentence "Interestingly here it is a trough over the North Atlantic and the ridge downstream al- ready exists for a longer time period than the up- and downstream troughs". Which troughs and ridges are they referring to in these descriptions?

Authors: We removed the first part of the sentence, it now reads "Interestingly, the ridge downstream already exists for a longer time period than the up- and downstream trough". The synoptic situation appears to be a downstream-development scenario; however, we would expect the upstream trough to be the oldest feature followed by the downstream ridge followed by the next downstream trough. The ridge however is the oldest features, which we did not expect.

Reviewer: iii) While the trough/ridge tilt is indeed closely related to the E-vectors, fundamentally tilted troughs/ridges are related to eddy momentum fluxes (which make up the E-vectors) and have been discussed as early as Jeffries (1926) and Starr (1948).

Authors: The fact that the tilt orientation and the corresponding orientation of the E-vector are fundamentally a result of the eddy momentum flux is mentioned several times throughout the manuscript. In the revised version, we now also define the E vector, and point the reader to the two original papers by Jeffries (1926) and Starr (1948). Thank you for mentioning these two seminal studies.

Reviewer: iv) p.8 line 25: Here the authors write that there is a second maximum near Lake Baikal, which to me is not really accurate since Lake Baikal apparently is located between two relative maxima, one west of 90E and he other over eastern Siberia, northeastern China.

Authors: Yes, corrected and changed to "west of 90E".

Reviewer: v) Fig. 5: Are the anomalies shown statistically significant? Some of the anomalies seem rather small.

Authors: The anomalies are in good agreement with the anomalies seen in E vectors (Drouard et al. 2015) and we therefore consider them physically meaningful and a good indicator for a consistent change in the orientation during ENSO. The idea here is to compare the results of the two diagnostics against each other and not to show that ENSO produces statistically significant eddy anomaly fluxes. We now set regions where the trough and ridge frequency are below 2% to missing data value.

Reviewer: vi) p. 10, lines 22-23, "more systems will nnow grow on the poleward flank of the jet" this should be quantified or reference cited. Both the jet and storm track shifts equatorward during mid-winter so it is not entirely clear that more systems grow on the poleward flank of the jet. Perhaps a histogram showing the frequency as a functio of tilt would quantify this.

and vii) p. 10, lines 25-26: Same comment as above.

Authors: Please find below a figure for the meridional wind shear of the zonal wind (dU/dy) over the Pacific. During midwinter (black contour), wider parts of the main growth and propagation sector of extratropical cyclones (30-60N) are affected by stronger cyclonic shear, which led us to the interpretation that all poleward tracking storms develop more consistently on the poleward flank of the subtropical jet, assuming that genesis is at a relatively fixed location over the Kuroshio. In agreement with the enhanced cyclonic shear over the Pacific (see below) are the more cyclonically oriented troughs and ridges during midwinter.

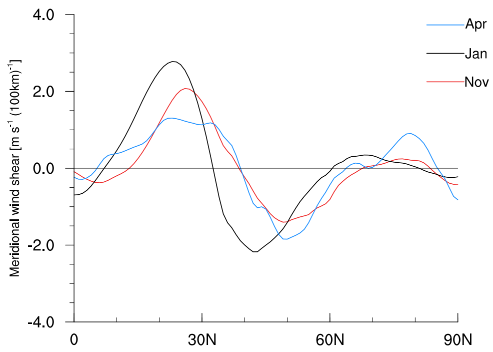

Figure 2: Meridional wind shear of the zonal wind over the Pacific in winter.

Reviewer: viii) p. 10, lines 31-32: "there is no marked reduction in the number of troughs and ridges" This is surprising given the strong decrease in number of troughs found in

Penny et al. Can the authors show the results and provide some explanation on why there is such disagreement between their results and those of previous results?

Authors: This result is an agreement with Schemm and Schneider (2018) who note that the number of surface cyclones in the central Pacific is not decreasing during midwinter. Penny et al. (2010) analyzed upper-level eddy activity at the 300-hPa level in spatially and temporally filtered geopotential height. As is the case with many measures, the midwinter suppression is seen in bandpass-filtered data (eddies and related eddy quantities) but not in measures of the mean flow (mean baroclinicity, mean zonal wind at 300 hPa). It seems as if features identified in raw data, without any frequency filtering (surface cyclones in SLP, Z500 troughs and ridges), exhibit no well-marked suppression, while frequency-filtered measures do. We cannot guarantee that this is the answer, but it appears to be the main methodological difference. In the revised version we added the detection frequencies into the corresponding panels as yellow contours. We find a new localized detection maximum over the eastern North Pacific, which potentially relates to wave breaking and lysis, since detection maximum show a good agreement with cyclolysis frequencies (Fig.5 in HH02). This underpins the presented hypothesis of accelerated, or interrupted, life cycles during midwinter over the eastern North Pacific.

Reviewer: ix) p. 11, line 6: "a fast and intense deepening phase followed by a rapid decay". The familiar LC2 lifecycle of Thorncroft et al (1993) shows a cyclonic lifecycle that decays very slowly. Perhaps the authors should provide references that show rapid decay of cyclonic lifecycles.

Authors: Yes, it is in fact the LC1 that shows an EKE peak followed by a rapid decay (Fig. 4 in Thorncroft et al. 1993). We find however more cyclonically oriented troughs but the reduction in cyclone lifetime was shown in Schemm and Schneider (2018). We thus agree that the familiar and idealized LC2 concept seems not to apply. The LC2 life cycle eventually gets dismantled.

Reviewer: x) p. 12, line 14: Should refer to Fig. 9 here.
Authors: Included.

Reviewer: x) p. 12, line 14: Should refer to Fig. 9 here.
Authors: Corrected.

Reviewer: xi) p. 12, lines 19-20: The windspeed in summer is also weaker so even with the same tilt the vertical motion would still be reduced. This again is related to weaker baroclinicity through the thermal wind relation but still this should be mentioned. Both reduced tilt and reduced wind speed can contribute to decrease in vertical displacement.

Authors: Good point, we added this argument.

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