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Weather and Climate Dynamics Discussions

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Interactive comment on "The Life Cycle of Upper-Level Troughs and Ridges: A Novel Detection Method, Climatologies and Lagrangian Characteristics" *by* Sebastian Schemm and Michael Sprenger

Anonymous Referee #1

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In this paper, the authors developed a new method to track trough and ridges. They defined ridge and trough axes using curvature, and tracked ridges and troughs and record both the areas covered by the objects (based on a threshold of curvature) as well as the orientation of the axes. In so doing their method provides additional information on top of those provided by previous studies. They applied this method to compile climatological statistics for the Northern Hemisphere extended cold and warm seasons, and examined ENSO impacts on trough and ridge orientations as well as the winter seasonal cycle in the Pacific. Finally they used Lagrangian back trajectory from within

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Discussion paper

trough and ridge areas to examine the behaviour of air parcels making up the trough and ridges over the previous 24 hours. Overall the results are interesting and the method is new and can provide new insights over previous methods. However, I do have concerns which should be addressed before this paper can be accepted for final publication.

Major concerns: 1) 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?

References:

Hoskins et al., 1985, QJRMS, 111, 877

Hoskins and Hodges, 2002, JAS, 59, 1041

2) 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?

3) 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).

References:

Chang and Yu 1999, JAS, 56, 1708

Hakim, 2003, Mon Wea Rev, 131, 2824

Chang, 2005, Mon Wea Rev, 133, 1998

Nakamura and Sampe, 2002, GRL, 29, 1761

4) 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.

5) 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 algo-

rithm. Details of how tracking is done should be presented - perhaps as an appendix or supplemetal material.

6) 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.

7) 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?

8) 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.

9) 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?

10) 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.

11) 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 more clear at the tropopause level?

Other comments: 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?

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 already exists for a longer time period than the up- and downstream troughs". Which troughs and ridges are they referring to in these descriptions?

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).

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 maxma, one west of 90E and he other over eastern Siberia, northeastern China.

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

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.

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

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?

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.

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

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.

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