

Review of “Exploring the vertical extent and deepening mechanisms of cut-off lows in the Southern Hemisphere: insights from eddy kinetic energy analysis” by Pinheiro et. al.

Suggested outcome: Major Revisions

Scientific significance: Good

Scientific quality: Good

Presentation quality: Good

This work creates a climatology of cut-off low depth over the Southern Hemisphere and studies the vertical extent and mechanisms that lead to deep cut-offs from an energetics perspective.

General comments:

The authors present a different methodology for tracking cut-off depth and vertical extent using an established cyclone tracking algorithm. Although the methodology and climatology are relatively rigorous, there seems to be little discussion of the mechanisms and processes that lead to cut-off vertical extent that the authors pose. I do concede that the authors refer to arguments made in previous work, but these need to be fleshed out more and discussed more fully here for the reader to understand their arguments. Importantly, I feel there are still gaps in the evolution of the coupled upper-lower tropospheric processes.

Major comments:

1. Upper-level processes in relation to the lower-level processes

The methodology and the results of the cut-offs in relation of the lower-level processes are obviously critical to the results of this work. There however appear to be some gaps in the authors arguments as to how well the methodology captures this link and/or separation. The authors should consider expanding on this process to enhance the value of this work.

The authors use a top-down approach when searching for vertical extent of cut-offs. This is a sensible choice of course. However, the authors also admit that this approach may not capture all coupling types. Do the authors see any evidence of other coupling types in the data they have collected? For example, Figure 5 shows a closed surface circulation at the “upper-level trough” phase (T-48). Does this show evidence that the surface is developing and closing prior to the cut-off and thus is developing from the surface, upwards towards the upper troposphere? Or that the cut-off enters a region of a pre-existing surface low? Is discussion of what occurs prior to T-48 required to explain the potential differences in deep and shallow cut-offs, since the cyclonic circulation seems relatively mature (although not cut-off) by T-48? Additionally, the shallow cut-off composites in Figure 4, show that some degree of surfaceward extension is occurring since there a cyclonic zone, albeit weak, at the surface. Is all we are seeing simply an intense (for deep) versus weak (for shallow) cyclonic circulation in the upper levels with “action at distance”? If so, are the dynamical processes really that different?

Further to this, the authors suggest that the decrease in tracks when expanding the requirement for temporal coherence suggests that the coupling is most frequently in the mature phase. Could an argument not be made that this decrease could be the result of their independence from one another. Ie. could the larger number of extended COLs that occur with a small temporal coherence could result from many COLs simply moving over a low-level baroclinic zone or pre-existing low-level cyclone?

2. Depth of dynamical reasonings

Figures 4 and 5 are great, but the discussion of them and the processes at play are never really

fully discussed. One should really go into detail in the framework chosen as to how these processes play out.

Often dynamical reasons are brief and simply reference the authors previous work. This is fine of course, however, I found it difficult to follow some of these arguments and reasonings without jumping between several different papers. The manuscript would be fleshed out significantly by extending and fleshing out some of these arguments somewhat to provide a fuller picture to the reader.

Specific comments:

- L33: “high potential vorticity anomalies” – ambiguous in the southern hemisphere as there we deal with large negative values of PV. Suggest the use of “large magnitude” or “cyclonic”.
- L57: “ageostrophic fluxes” is used throughout the manuscript. Is “ageostrophic geopotential fluxes” a more accurate description of this term?
- L57-L62: Use of multiple adverbs started sentences in a row (ie. “Furthermore,...” and “Additionally,...”). Suggest to rewrite so that this paragraph flows more easily.
- L76: Is there a reason the authors are not using the latest reanalysis (ERA5)?
- L82: “similarly as done before” -> “as done in previous work”?
- Methods: The authors explain throughout the manuscript the advantage of vorticity tracking to include small-scale cyclonic circulations. Is there a sensitivity of the choice of 5-degrees when looking at whether that circulation is closed? I.e. is it possible if the vorticity minimum is small-scale for the u and v components to be unrelated to the cyclonic circulation identified?
- L136: “It could also” -> “Errors could also...”?
- Figure 1: Panel b) is labelled as panel a) in the figure title
- Figure 1: The most intense density of COLs is located on the Mozambiquan channel. As the authors use a “cyclonic circulation only” type tracking without taking into account core temperatures, are the authors picking up transitioning Tropical Cyclones in this region?
- Figure 2: The presentation of these results as well as some of the wording in the explaining paragraphs (ie. L209-211) could be improved to make the point of extension to low-levels without extension to the surface clearer. The “sharp decreases” in regions A and C (L211) are difficult to see.
- Figure 3: It may be useful to plot some proxy for the jets on this figure as this is a large part of the authors argument for why deep COLs preferentially occur in specific regions. Does the seasonality of these COL depths coincide with when the split jet occurs (during the cool season)? This discussion should also be expanded.
- L243: “Figure 1c” -> “Figure 3c”?
- L242-243: “southeastern Pacific, where deep COLs observed at more northern latitudes” – there doesn’t seem to be that much change in latitude from Figure 3. Consider some latitude statistics to prove this point.
- Figure 4 and Figure 5 - do both of the timesteps provided represent the relevant phases that the author suggests in L254-255? For example shallow COLs at T0 seem to be similar (at least in the upper-levels) to deep COLs at T-48? Do shallow COLs actually ever reach maturity?
- Figure 6: Deep cut-offs appear to be embedded somewhat in really strong westerlies? Is this true? And does this have an impact on the associated baroclinicity? This point is very briefly mentioned (L276), but could be expanded on.