Authors' response to reviewers' comments

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Exploring the vertical extent and deepening mechanisms of cut-off lows in the Southern Hemisphere: insights from eddy kinetic energy analysis

New suggested title:

"Deepening mechanisms of cut-off lows in the Southern Hemisphere and the role of jet streams: insights from eddy kinetic energy analysis"

The authors gratefully acknowledge the insightful feedback from the anonymous reviewers and the editor, which significantly strengthened the revised manuscript. We have carefully considered each comment and implemented substantial changes to address them, particularly regarding the influence of jet streams on COLs. We believe the revised manuscript is now significantly stronger and more impactful.

We have made adjustments by removing certain figures (or relocating them to the Supplementary Material) and shortening the text to prevent excessive lengthening of the paper. Furthermore, we have replaced the term "mid-latitude jet" with "poleward jet" during the discussion, as it is a more precise description.

In this document, the responses to reviewers' comments are highlighted in red font.

Reviewer 2

General comment

The study considered COLs in the SH and then categorized them according to how deep they. These categories were shallow, medium and deep COLs and the latter were shown to extend to the surface. The study further undertakes an energetics analysis to gain insights into the mechanism that could be responsible for the extension of COLs to the surface. This is a well written and succinct paper. It has the potential of making a contribution to the current work that is ongoing on COLs in the SH. I recommend that it be considered for publication, provided the comments below are adequately addressed.

Comments

2. Eq 1. This study aims to consider the three dimensional structure of COLs, so why are the authors then taking the volume integral? This will average our processes that I believe are important to consider if the vertical structure is to be assessed. So, I challenge the authors to consider the EKE that is not integrated to reveal vertical processes (as will be mentioned again later in this review). So I am suggesting that consider the diagnostics used here carefully.

We calculate energetics for both vertically integrated quantities and each pressure level separately. This approach enables us to analyze the EKE budget in a vertical cross-section, as illustrated in Figures 5 and 6. This information can be found in Section 2.3.

3. Top panels of Fig 4 and 5. How were the vertical profiles produced? Did you average over a range of latitudes. Please include the jet in this plot, they are, in my opinion, an important piece of the puzzle.

Thank you for pointing out this gap in our study. We produced west-east vertical cross-sections using a system-centered compositing method. A 25° latitude-longitude grid is initially set up on the equator and then rotated to the 300-hPa COL center. The data is then interpolated to this grid at each level from 1000 hPa to 100 hPa. These cross-sections are created at multiple time steps during each COL lifecycle, relative to the time of maximum 300-hPa vorticity intensity. We have clarified the steps involved to produce composites in in Section 2.3.

4. Paragraph 270: I agree with the notion that the eddy driven jet is a primary source of EKE and please use the ridge axis as a reference to differentiate between the energy centre that develops from baroclinic conversion and the one that arises as a result of ageostrophic flux convergence. Recent studies in the SH (even though they have focused on regional issues) have shown that there is very weak or no baroclinic conversion downstream of this ridge axis. The authors should explain the cause for the vertical circulation that leads to baroclinic conversion. This is particularly important in this paper because it looks like the authors argue that the midlatitude jet streak is responsible for differences between shallow and deep COLs, and as a reviewer, I totally agree with that.

Thank you for your thoughtful comments. Firstly, we agree that the ridge axis is a useful reference for differentiating between the two energy centers, and we have added this to our discussion.

The reviewer is correct that mid-latitude disturbances associated with cyclones typically exhibit weak baroclinic conversion on the upstream ridge, as shown in the studies by Chang (2000, MWR) and Danielson et al. (2006, Atmosphere-Ocean). There are, however, some important differences between the characteristics described above and the mechanisms that act on COLs, which are predominantly influenced by stationary Rossby waves.

As these Rossby waves break, the trough-ridge system deepens and induces anticyclonic barotropic shear and potential vorticity overturning, as suggested by the reviewer in the following comment. This, in turn, triggers stronger ageostrophic fluxes and momentum from the upstream midlatitude jet into the COL system. Additionally, localized baroclinic processes become prominent on the western flank of COLs, primarily due to descent of cold air on the upstream ridge, enhanced by radiative processes, as outlined in our manuscript. This phenomenon is a robust feature of COL systems, seemingly irrespective of their vertical extent, though stronger ageostrophic fluxes can strengthen the COL and make it more persistent. Further discussion on this topic have been incorporated into the revised manuscript.

5. Paragraph 275: I think that question of why the jet is stronger for deeper COLs must be addressed.

Still on this issue: The authors mention and show that the ageostrophic fluxes are stronger for deeper COLs. This makes perfect sense but why? Would the authors agree with the hypothesis that a stronger jet causes increased anticyclonic barotropic shear, which in turn causes higher strain rates and therefore the higher likelihood for RWB (Nakamura and Plumb 1994)? This means that the flow across the ridge axis becomes more super geostrophic, implying a stronger ageostrophic flow across the ridge. But the breaking increases the intensity of the geopotential height anomalies. Thus, combining these two issues leads to stronger ageostrophic fluxes and stronger EKE in the COLs.

We agree with the hypothesis proposed, as it aligns well with our findings. In addition to the previous comment, we believe that the vertical baroclinic structure in deep COLs is also a manifestation of eddy feedback mechanisms arising from an interdependence between upper- and lower-tropospheric eddies. This discussion has been included in the revised manuscript.

I also curious about the ageostrophic fluxes associated with the sub-geostrophic flow that the categorization suggested here appears to be revealing. What does that mean for the energy centre at the inflection point immediately downstream of the ridge axis? No mention is made of this and it looks like it is playing some role.

An interesting observation. The subgeostrophic flow at the top of the trough, indicated by the westward flow, seems to be a result of the negative geopotential height anomalies, contrasting with the supergeostrophic flow on the upstream ridge where positive geopotential anomalies exist, thus inducing fluxes oriented north-eastward into the closed circulation region.

While our discussion focuses on certain aspects, there could indeed be additional factors at play that influence the energy dynamics in these systems, which is still to be explored through further investigation. We have included a brief comment on this issue so that it can guide future work.

Why would a stronger jet streak lead to stronger baroclinic conversion: The authors could invoke jet streak dynamical theory to explain this. For instance the cross front quasigeostrophic theory of Keyser and Shapiro (1986) could used to explain this. Also the location of the baroclinic conversion relative to the jet should be considered to link the strength of jet and the strength of the conversion. The authors should consider the possibility that the midlatitude jet might be curved, which has implications for vertical motion.

6. In point 1 above an issue about the diagnostics used was raised. EKE is not visible at the surface for shallow COLs, that is reasonable. They are however very clearly in existence for deep COLs, even though they are weaker than aloft. So, what causes them? Where does this energy come from? That is why taking the volume integral of the energetics terms is not the appropriate thing to do here. I invite the authors to have a look at Fig 7 in Reviere et al (2015). There is a clear downward flux of EKE and yet the authors say that this flux is small; yet this seems important in explaining where the surface EKE might be coming from.

In conclusion, I think that the use of energetics has a lot of potential in explaining what causes some COLs to extend to the surface and others not. However I am of the opinion that the energetics framework used here is not adequate and I invite the authors to employ the energy equation derived by Reviere et al (2015), without the volume integration to reveal the vertical structure, in particular attempt to explain where the EKE at the surface is coming from. That is lacking in the manuscript.

My suspicion is that deeper COLs last longer and as the authors have raised dissipation issues here, barotropic conversion must be considered.

Therefore, the first two research questions have been addressed, but the third one has not been adequately addressed and I invite the authors to consider the points made above.

We appreciate the reviewer's valuable comments and suggestions. We agree that the current energetics framework requires further refinement to fully elucidate the mechanisms behind COL deepening. Additionally, we acknowledge the potentially significant role of vertical ageostrophic fluxes in COL deepening. While direct computation of these fluxes remains challenging with current data and resources, requiring adjustments to incorporate them, the revised manuscript recognizes their potential importance and emphasizes the need for future research.

We believe the revised manuscript provides sufficient evidence to adequately address the third research question regarding the mechanisms of COL deepening. The incorporated analyses have significantly enhanced the quality of our research and contribute to a more comprehensive understanding of the dynamics of both shallow and deep COLs.

Regarding the barotropic contribution, the previous study by Pinheiro et al. (2022, QJRMS) examined the mechanisms governing energy changes in COLs. They found that ageostrophic fluxes and baroclinic energy conversion can counteract the substantial damping effects of barotropic energy conversion and friction, particularly in stronger systems. During the decay phase, diabatic processes and dispersive fluxes emerge as the primary contributors to COL dissipation. This is evident in Figure 1, which shows minimal or even no contribution from barotropic conversion to the energy decay of COLs.

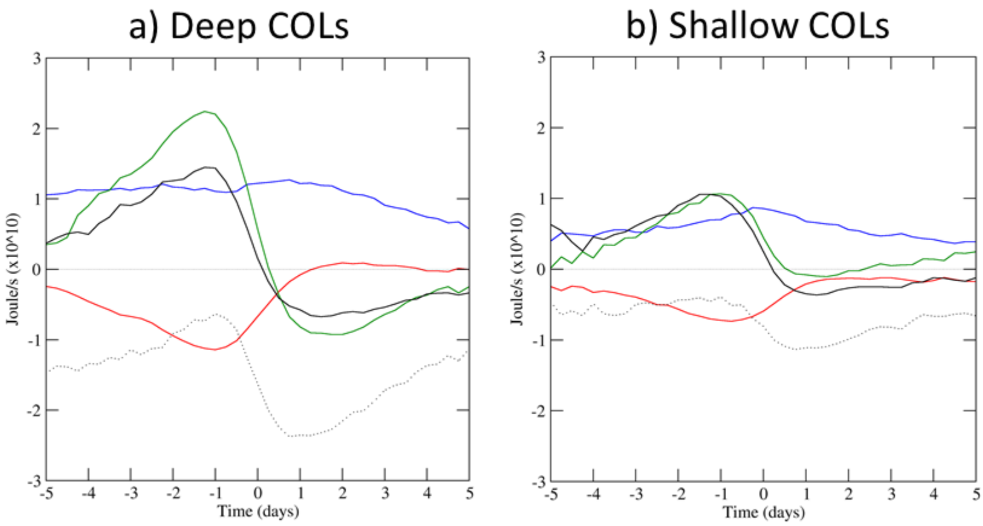

Figure 1: Temporal evolution of the main EKE terms for a) deep and b) shallow COLs. The terms are baroclinic conversion (blue line), barotropic convertion (red line), agesotrophic flux convergence (green line), convergence of kinetic energy (black line) and residual (dotted line). Fields are vertically averaged within a 15◦ spherical cap region centred on the COL location. Unit is Joule·s^{-1}, scaled by 10^{10}.