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

By Pinheiro HR, Hodges KI and Gan MA.

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.
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.
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.
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.

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.

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 quasi-geostrophic 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.