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 1

General comment

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 evolu3on 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 cutoff 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 cutoff 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?

Thank you for raising these critical points. We recognize that the original version lacked explanation, in this revision we have attempted to improve this.

Employing a single direction search scheme may not capture all types of coupling, but this does not necessarily invalidate our findings. COLs interact with lower-level cyclonic features in a variety of complex ways, resulting in different coupling patterns.

More advanced methodologies, such as that used by Lakkis et al. (2019), have the potential to offer a more comprehensive understanding of all staked cyclones in the atmosphere, but this is not particularly our goal. We focus specifically on the upper-level forcing driving surface cyclone development (interconnected vortex structure) and the mechanisms driving this interaction. While we may not have sufficiently clarified this in the first version, we have emphasized this focus in our revised manuscript (for detail, please see Section 2.2).

Still related to the methodology, using a vorticity-based feature tracking method with less restrictive temporal overlaps allows us to capture as much of the stacked lifecycles as possible, irrespective of the bottom-to-top or top-to-bottom orientation. This aligns with the findings of Lakkis et al. (2019), who obtained similar though not exactly identical results using both stacking approaches for multi-levelled events. This is also discussed in Section 2.2.

Another aspect of our approach is the possibility of including preexisting cyclones due to the relatively small temporal threshold used for matching. This allows the detection of a broader range of multi-level stacked lifecycles. Techniques based on clustering could potentially provide insights on the spatial patterns of interaction between upper- and lower-level cyclones, as well as the different upper-tropospheric flow patterns associated with upper-level jets, such as equatorward/poleward entrance/exit regions. Some studies (e.g. Sinclair and Revell 2000; Studholme et al. 2015; Catto 2018) have explored these patterns, and this could be investigated in further work. In the revised version, we discuss and compare our current findings with those from earlier studies.

We considered examining events prior to T = -48 hours to identify the atmospheric precursor mechanisms. However, extending the compositing window beyond this timeframe introduces noise that arises from differences in COL lifetimes. Alternatively, considering tracks with lifetimes exceeding four or five days would significantly limit the sample size available for compositing.

We acknowledge that our discussion of the similarities and differences between shallow and deep COLs could be more detailed. However, our revised manuscript does explore their developmental mechanisms in more detail, including a deeper discussion on the role of jet streams for COL deepening.

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 preexisting low-level cyclone?

You raise a valid point. While the decrease in track counts does not definitively establish that coupling primarily occurs during the mature phase, it suggests that COLs likely associate with lower-level features for a limited time. This is consistent with the increased interactions observed when relaxing the threshold. We have reorganized the text and removed the contested sentence, acknowledging the lack of conclusive evidence.

Regarding multiple COLs acting with a single cyclone, we cannot definitively dismiss the possibility, but based on current knowledge, it seems less probable. This interpretation aligns with our understanding of these atmospheric phenomena.

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.

Thank you for the feedback. We agree that the discussion of these figures and the processes at play could be more detailed. We have expanded the discussion in the revised manuscript to provide a more detailed analysis.

Also, we understand that it may be difficult for readers to follow our arguments without having read our previous work. We provided some additional information as supplementary material to help readers understand our findings without consulting multiple references.

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”.

Agree, this has been changed.

• L57: “ageostrophic fluxes” is used throughout the manuscript. Is “ageostrophic geopotential fluxes” a more accurate description of this term?

To avoid confusion, we included the term originally proposed by Orlanski in the first citation, although both terms have the same physical interpretation and have been used interchangeably.

• 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.

This sentence was rewritten for clarity.

• L76: Is there a reason the authors are not using the latest reanalysis (ERA5)?

This work was originally started sometime ago before ERA5 was available and used ERA-Interim data to identify COLs. To be consistent with our previous work which used the same COL dataset we have continued with ERA-Interim based analysis. Whilst it would certainly be of interest to repeat this study and our previous studies using ERA5 instead doing this would take some time to complete all the calculations. We hope to continue the work using ERA5 reanalysis in the future.

• L82: “similarly as done before” -> “as done in previous work”?

It has been changed as suggested.

• 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 scall for the u and v components to be unrelated to the cyclonic circulation identified?

Good point. The thresholds used to identify cyclonic systems are determined based on the type of cyclonic systems typically observed in the region, supported by a limited sensitivity analysis. In general, decreasing the distance from the vorticity center reduces the lifetime and/or the number of identified tracks, meaning that some COLs will be missed.

Objective methods work well for COLs with more symmetric circulation, but some issues arise with tilted troughs, particularly in the early stages. The problem described above can be minimized by avoiding relatively small distances for the wind direction search. However, the authors recognize that this is a difficult task, as the method to identify COLs is somewhat arbitrary.

• L136: “It could also” -> “Errors could also…”?

It has been changed.

• Figure 1: Panel b) is labelled as panel a) in the figure title

We opted to relocate this plot to the supplementary material to prevent elongation of the paper.

• 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?

We agree it is possible that some of the identified COLs in the Mozambique channel are transitioning tropical cyclones, however, we believe that the majority of these COLs are distinct phenomena.

Note that the identification based on circulation is a post tracking step, the tracking of all systems is done first and then the identification. In a sensitivity analysis, a cold-core condition has also been imposed to the identified tracks as a post-tracking step. This is done by searching a temperature minimum over a spherical cap region within a 5.0° geodesic radius from the vorticity center, as detailed in Pinheiro et al. (2017). Figure 1 shows that the track density is reduced by adding a cold-core condition, but the spatial pattern looks quite similar to the standard method with only winds. This is expected as most tracked systems are essentially cold air cut-offs.

Pinheiro, H. R., Hodges, K. I., Gan, M. A., & Ferreira, N. J. (2017). A new perspective of the climatological features of upper-level cut-off lows in the Southern Hemisphere. Climate Dynamics, 48, 541-559 (https://link.springer.com/article/10.1007/s00382-016-3093-8)

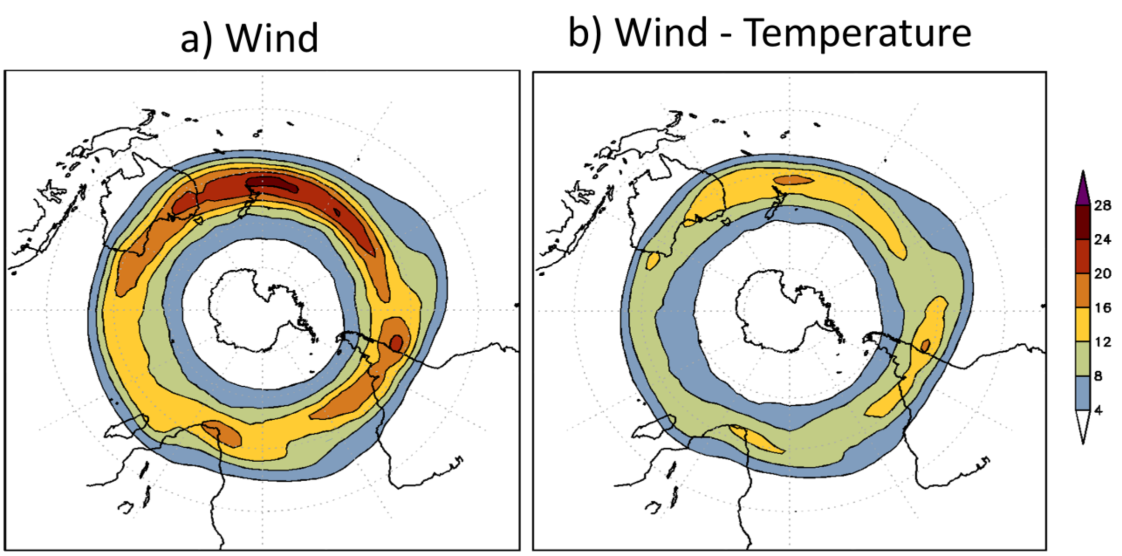

Figure 1: Annual track density of SH COLs based on the 300-hPa relative vorticity using (a) 300-hPa horizontal wind components only and (b) 300-hPa horizontal wind components with 300-hPa temperature. Track density in shaded and solid line for contour interval of 4.0 units. Analysis is performed using the ERAI reanalysis for a 36-yr period (1979-2014). Unit is number per season per unit area, the unit area is equivalent to a 5° spherical cap (≅ 10^6 km^2).

Another point is that using a cold core condition leads to uncertainties between studies since the cold core search is generally performed at different layers, which seems to be chosen arbitrarily in different studies.

We believe that simpler schemes based only on winds should be more representative of reality since they simply impose on the detection the presence of a cyclonic circulation regardless of the physical and dynamical characteristics. The impact of multiple criteria schemes is discussed in this paper:

Pinheiro, H. R., Hodges, K. I., & Gan, M. A. (2019). Sensitivity of identifying cut-off lows in the Southern Hemisphere using multiple criteria: implications for numbers, seasonality and intensity. Climate Dynamics, 53, 6699-6713 (https://link.springer.com/article/10.1007/s00382-019-04984-x)

• 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.

We agree with reviewer. However, we removed this analysis from the paper because it was outside the scope after modifications.

• 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.

We acknowledge the reviewer's insightful comment and have expanded our discussion in the revised manuscript to encompass the role of subtropical and polar front jets for deepening COLs. We delve into the specific effects of each jet on different COL types. Please refer to Figures 2, 3, and 4 and the accompanying discussions for further details.

• L243: “Figure 1c” -> “Figure 3c”?

The figure caption has been corrected for accuracy and the figure itself has been moved to the Supplementary Material.

• 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.

This has been removed from the text.

• 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?

The authors are not sure if they understand the reviewer's comment. Composites are produced for particular fields offset from the time of maximum intensity of each track (maturity stage), though it can be defined as maximum growth. Note, however, that the lifecycle stages of deep COLs are more distinguishable due to their stronger gradients and longer lifetimes compared to shallower COLs.

PS: Figures 4 and 5 have been renumbered to Figures 5 and 6, respectively.

• 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.

Yes, deep COLs are often embedded in strong westerlies, as demonstrated throughout the study. This is because strong westerlies provide a source of baroclinicity and a favourable environment for their deepening.

The revised manuscript discusses the vertical baroclinic structure in deep COLs and the interdependence between upper- and lower-tropospheric systems due to feedback mechanisms. Specifically, we discuss how the dynamics of deep COLs differs from that of shallow COLs, and how this difference can affect the vertical coupling.

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 2, which shows minimal or even no contribution from barotropic conversion to the energy decay of COLs.

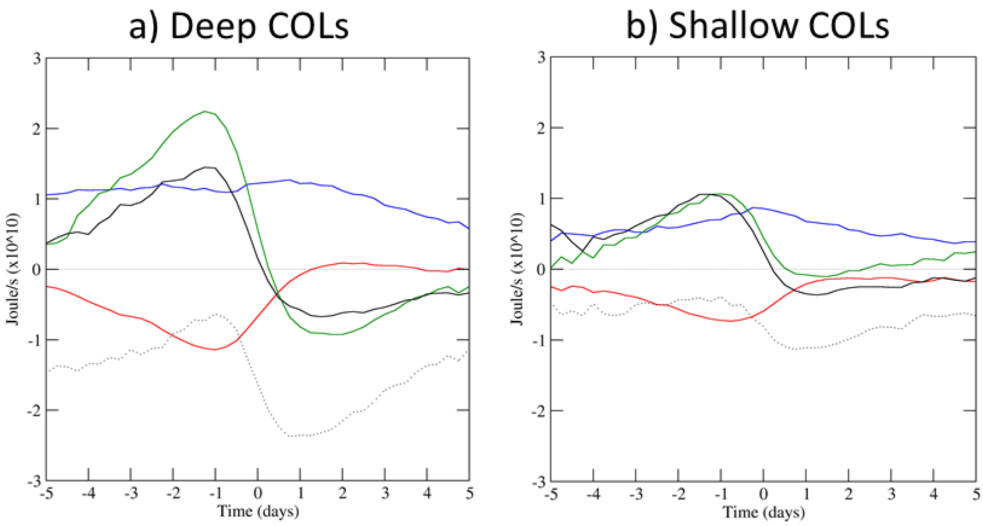

Figure 2: Temporal evolution of the main EKE terms for a) deep and b) shallow COLs. The terms are baroclinic conversion (blue line), barotropic convertion (red line), agesotrophic flux convergence (green line), convergence of kinetic energy (black line) and residual (dotted line). Fields are vertically averaged within a 15◦ spherical cap region centred on the COL location. Unit is Joule·s^{-1}, scaled by 10^{10}.