Weather Clim. Dynam. Discuss.,
https://doi.org/10.5194/wcd-2020-30-RC1, 2020

Interactive comment on "The three-dimensional life cycle of potential vorticity cutoffs: A global ERA-interim climatology (1979–2017)" *by* Raphael Portmann et al.

Anonymous Referee #1

Received and published: 23 August 2020

General comment

The authors present a comprehensive global climatology of potential vorticity of cut-offs that considers various crucial aspects of these structures, including their global geographical frequency distribution, where they are generated and dissipate, their life cycles, tracks, decay and absorption processes, vertical structure, the stratospheric tropospheric exchange associated with them and their links to surface processes. The methodology employed is sound and the fact that the limitations of the tracking algorithm have been outlined is also good. This is a critical contribution to the current body of work in cut-off low climatologies because, not only does it broaden the scope of

Printer-friendly version

Discussion paper

published COL climatologies, it has the potential to contribute to regional predictability studies at the medium range forecasting time scale and shorter. I think that the paper is very well written. I therefore recommend publication subject to the following issues having been adequately addressed.

Specific comments

The comments in the section of this are considered major in the sense that they might require additional calculations:

1. Line 125: The feature shown in Figure 3 off the coast of Brazil, is in my opinion, a very important phenomenon for that part of the SH but might be under estimated on the 350 K isentropic surface and previous experience suggests that it is most clearly seen on the 360 K isentropic surface. If one plots the climatology 360 K PV = -2PVU contour for DJF for instance, a clear PV overturning region (or at the very least a region of very strong undulations) is seen in that part of the SH, thus showing the dominance of that structure. I therefore suggest that, at least for the SH, the analysis be extended to include the 360 K isentropic surface.

2. Section 3 is good but could benefit from integrating the effect of the zonal flow. So I suggest that the authors consider superimposing the zonal flow isotachs in Figures 3 to 5 and consider seasonal changes in the flow and the impact of these on the seasonal frequencies discussed in this section. I think that we would see dominance of these frequencies in areas of diffluent and confluent flow. It would also provide an opportunity to view the frequencies relative to the jet core. These can be inferred by the experienced reader, but to render this discussion accessible to those readers who are not familiar with RWB dynamics (as noted in the manuscript, the cut-offs are residues of RWB events), this could improve the readability and impact of the study.

3. The discussion in Section 4.1 is very interesting and has interesting and important implications for the various regions considered there in. The region off the coast of Brazil might benefit from such considerations (Figure 18 illustrates the importance of

this region and as reviewer I feel it requires as much attention as the other regions that have been considered in the study). Many studies have shown that it is characterised by cyclogenesis. Please consider an additional regional analysis for that region.

4. Could the authors consider composite evolution of at least some of the regions? For instance, with no bias from myself as a reviewer but based on some experience of knowledge of the region thus my use of it to illustrate my point. There exists an interesting phenomenon that occurs in the South Atlantic Ocean/South African domain referred to as the ridging of the South Atlantic Ocean Anticyclone (SAOA) that has been shown to be influenced by lower stratospheric processes. It is most clearly seen in the MSLP pressure fields and is defined as an extension of the SAOA eastward and this ridging process has important implications for the region. Also in South America there is a strong presence of Rossby wave trains that propagate into that continent from the west of Drake Passage, aspects of which could be characterised by this analysis (the 250 hPa anomalies indicate this in Figure 6). I am certain that there are other interesting dynamical processes in the other regions that are unique to those regions that could be alluded to in this discussion, if the authors so please. All in all, I think that evolving composite could reveal some of these regional dynamical issues.

Minor comments

1. In Fig 6 please use hatched instead of dashed. Also are the composite means statistically significant in that figure?

2. Line 372 please replace South Pacific with Indian Ocean.

Interactive comment on Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2020-30, 2020.

Printer-friendly version

Discussion paper