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Interactive comment on "Atmospheric convergence zones stemming from large-scale mixing" *by* Gabriel M. P. Perez et al.

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article

Reply to David Schultz

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We appreciate the interest of Dr. Schultz in our research article and thank for his insightful comments. We will address the comments we judge more relevant for the WCD's audience below.

Dr. Schultz suggests that we use the word "automated" instead of "objective" to describe our methodology. We strongly disagree with this. We suggest the reading of the seminal work of Shadden et al. (2008) on Lagrangian Coherent Structures (LCSs). The authors demonstrate the objectiveness of FTLE ridges by deriving a formula for the flux across them; they go on to show that FTLE ridges are material lines to a very good degree of approximation.

It is suggested that we cite a wide body of literature regarding flow kinematics, atmospheric rivers and fronts; many of such studies were produced by Dr. Schultz's group and collaborators. We appreciate the quality and value of the contributions presented in these suggestions and will consider adding some of them in the revised manuscript. However, we do not aim to provide an extensive review on each of the concepts we explore: our aim is to introduce the FTLE and the concept of LCSs to the broad meteorology community as well as providing sufficient background literature to support the interpretation of our novel results. Moreover, most of the suggested literature is around Eulerian metrics, such as the Okubo-Weiss criterion or the instantaneous Lyapunov exponent. While these are powerful diagnostics for instantaneous features or steady flows, they have limited ability to diagnose structures of tracer accumulation in unsteady flows. This is especially the case considering that moisture in the atmosphere has an average residence time of at least a few days, which is enough time for the moist parcels to explore large-scale turbulence and be shaped accordingly.

It is suggested that Figure 15 of Thomas and Schultz (2019) is, quoting Dr. Schultz, "very similar" to our Figure 6. Albeit somewhat related through the concept of airmass interface, the figures differ in more than one aspect: (1) the Atlantic ITCZ is not visible in their plot; (2) they capture a signal dominated by topography over South America. We believe that the difference between our results reflect that different methodologies were employed. The authors employed the asymptotic contraction rate, which is equivalent to the instantaneous local Lyapunov exponent, and, therefore, an Eulerian quantity. The authors also perform their analysis at the vertical level of 850 hPa. Our methodology employs a fully Lagrangian metric in a vertically integrated flow.

We appreciate the suggestions around the flow of the text and connection between chapters. These will be considered in the revised manuscript.