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

Anonymous Referee #1

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1 Introduction

The authors analyze the convergence zones and precipitation patterns in South America, using an approach coming from the dynamical systems and Lagrangian analysis: the Lagrangian Coherent Structures. This approach detects the stable/unstable manifold in a time-dependent flow trying to find the skeleton or the main features in the fluid flow that organize the transport. The authors employed here the LCS detection method based on the FTLE ridge extraction with some approaches. The authors focus on a case study of a precipitation rainfall event over Brasil and try to characterize this event in terms of LCS after describing the methodology employed. Then analyze further from a climatology point of view the link between the persistence of this attractive

LCS with rainfall patterns. To that end, they perform a correlation between the presence of LCS and rainfall and other variables. Finally, to analyze the synoptic meteorological mechanism that shapes the wind flow and hence the convergence, they analyze the role that the geopotential height anomalies play. The authors found that exists a link between the LCS and the rainfall patterns on that area.

General comments I found the article interesting and must be considered for publication, however, I found key points that should be addressed before publication. The main points are that, in my opinion, there is a lack of connection between sections, I do not find a true motivation to go from a case study to a climatological study in the introduction. At this moment, it looks more like a sequence of isolated sections more than a logical connection between them. I suggest the authors introduce a short paragraph in the introduction, briefly explaining the motivation to go from a case study to a climatic study. For example, maybe the authors are interested in analyzing the impact of LCS through the different time-scales or how the mixing patterns affect the local and climatic scales. Also, I do not found a connection between the FTLE or LCS analysis for a case study (Section 5) and the extension to a climate analysis (Section 6). What period do the authors analyze? How do they extend this analysis from a local case? Do they do the same analysis at every time step? Finally, in my opinion, the conclusions should be improved. A simple characterization of this phenomenon in terms of LCS is a poor conclusion. I suggest the authors comment on the potential applications of this methodology to find convergence patterns in other regions and, more importantly, their viability as a forecast indicator.

2 Specific comments

Line 22: "Despite these historical associations with coherent trajectories". In my opinion, the authors should consider the use of "winds" instead of trajectories. The "convergence zones" are the consequence of coherent winds.

Line 23: "…frequently identify by "heuristic rules". Do the authors consider that the previous authors focused more on analyse the results qualitatively? or, do the previous authors base on arbitrary thresholds based on the experience?

Line 130: Cauchy-Green Formulation The spherical formulation of the FTLE based on the Cauchy Green deformation tensor includes and additional transformation matrix. Could the authors check if this formulation included in Haller 2001? As far as I know, this formulation is done in https://doi.org/10.1016/j.physd.2012.06.012.

Line 169: We identified candidate convergence zones as ridges of the FTLE scalar field by relaxing the criteria proposed by Shadden et al. (2005). The criteria involve isolating curves parallel to the FTLE gradient (condition SR1) and normal to the direction of most negative curvature of the FTLE scalar field (condition SR2). The latter is given by the eigendecomposition of the Hessian matrix. However, in practice, Shadden's criterion is too restrictive (Peikert et al., 2013). Peikert and Sadlo (2008) suggest relaxing SR1 by admitting a tolerance angle () between the curve representing the ridge and the gradient. We employed$(\epsilon_\theta = 15)^\circ$

The authors are right in their approach however I found it a very strong approach. The authors go from 90° to 15°) without more explanation. For example, how do the authors ensure that the detected LCSs on the FTLE field are not close to shear stress between the Andes mountains and the neighbouring wind flow as we can see in some LCS like in Figure 5, b,c? Did the authors perform a parameter analysis changing those values? Could the authors explain this choice very shortly?

Line 176: "subsetting the candidate FTLE ridges by size and average intensity. Ridges with average FTLE below 1.2 day-1 and length shorter than 500 km were discarded. While these thresholds are arbitrarily defined to filter out weaker and shorter structures, the relative distribution of convergence zones are "robust to

slight perturbations of the order +-20$\%$

Do the authors refer to the previously mentioned parameters? The authors do a great job presenting a flow map of the work. I suggest the authors should include a short table within the main parameters employed to analyse these phenomena with T, ϵ_θ, Ridge intensity and length of the LCS.

Figure 6: In my opinion, the authors should improve what is the meaning of the "frequency of occurrence" of LCS? Is the number of timesteps that LCS was detected at grid-cell divided by the number of total time steps? $\frac{t_{iLCS}}{t_i}$

Line 202: "Attracting LCSs are structures that shape the evolution of passive tracers in turbulent flows."

They do not have necessary to be turbulent flows. Is the double gyre from Shadden a turbulent flow? There is no cascade of energy transfers frow higher flow scales to the lower ones, however, there are attracting LCS. The author should consider using "in turbulent or chaotic flows" or more general "in time-dependent flows".

Line 210-214. The authors mentioned the LCS on figure 5 such as convergent LCS. It looks like this LCS is a consequence of the different wind flow of the particles over the Andes where the speed seems to be lower (close to 0) with the speed on the neighbourhood. This causes a shear LCS, between the Andes and the surrounding wind flow. In my opinion, this is the reason why the authors have a 0-Pearson correlation coefficient with rainfall over that area. The LCS are not true LCS. However, the deflected flow goes from south to North converging with the Northwind being a truly attractive LCS. Could the authors clarify this point?

Line 241: "correlating the FTLE with rainfall and moisture is a simple way to quantify their dependence on mixing." Correlate the FTLE against rainfall one-one is does not seems an indication that you are linking their dependence against mixing. In my opinion, to talk about mixing, you should use an integrated measure based on the FTLE because the time variability of the FTLE is what introduces mixing, not instantaneously value. Could the authors clarify the choice of this method or also cite previous work where this methodology has been used?

3 Technical comments.

The quality of the figure should be improved in general. The authors should consider adding the labels, and at least the minor ticks on these figures.

Figure 8: The authors should indicate what red polygon means. I suggest to the authors append to the left a figure with the composition of the LCS detected for all these events.

Figure 4: The y-labels should be placed on each figure or at least the minor grid ticks to know the association between the data. In any case, instead of the use of “departure lat”, I suggest appending the mathematical formulation from where the FTLE derives. It is the flow map evaluated at a time r(t) = $\phi_{t_0}^{T-t_0}(r(t_0), t_0)$