Weather Clim. Dynam. Discuss.,
https://doi.org/10.5194/wcd-2020-57-SC1, 2021

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

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Received and published: 5 March 2021

I appreciate the authors' interesting take on large-scale mixing in their submission. The submission tackles an impressive range of topics on the SACZ and kinematics, with a wide range of cited literature. My hat is off to the authors for assembling such an impressive range of scientific content.

I have broken my review into three parts.

A. The first part relates to literature that should be cited.

1. Other derivations of quantities that serve the same function or are similar to the FTLE have been published, but have not been described by the authors.

Okubo, A., 1970: Horizontal dispersion of floatable particles in the vicinity of velocity singularities such as convergences. Deep-Sea Res., 17, 445–454

McWilliams, J. C., 1984: The emergence of isolated coherent vortices in turbulent flow. J. Fluid Mech., 146, 21–43.

Benzi, R., S. Patarnello, and P. Santangelo, 1988: Self-similar coherent structures in two-dimensional decaying turbulence. J. Phys. A: Math. Gen., 21, 1221–1237.

Weiss, J., 1991: The dynamics of enstrophy transfer in two-dimensional hydrodynamics. PhysicaD, 48, 273–294

Cohen, R. A., and C. W. Kreitzberg, 1997: Airstream boundaries in numerical weather simulations. Mon. Wea. Rev., 125, 168–183.

Cohen, R. A., & Schultz, D. M. (2005). Contraction Rate and Its Relationship to Frontogenesis, the Lyapunov Exponent, Fluid Trapping, and Airstream Boundaries, Monthly Weather Review, 133(5), 1353-1369.

Arnup, S. J., & Reeder, M. J. (2007). The Diurnal and Seasonal Variation of the Northern Australian Dryline, Monthly Weather Review, 135(8), 2995-3008.

2. Although not directly related to the FTLE, climatologies of upper-level cut-off lows and moisture flows into South America have been discussed recently by Muñoz and collaborators. The second may be more relevant to this manuscript.

Muñoz, C., D. M. Schultz, and G. Vaughan, 2020: A midlatitude climatology and interannual variability of 200- and 500-hPa cut-off lows. J. Climate, 33, 2201–2222, doi: 10.1175/JCLI-D-19-0497.1.

Muñoz, C., and D. M. Schultz, 2021: Cut-off lows, moisture plumes, and their influence on extreme precipitation days in central Chile. J. Appl. Meteor. Clim., https://doi.org/10.1175/JAMC-D-20-0135.1.

3. There is a whole body of atmospheric river literature led by Reginald Newell and

Marty Ralph that is not cited here. Ralph, in particular, is credited with promoting the atmospheric river concept in the past twenty years and publishing dozens of papers on the topic. I am sorry to see that the authors did not cite a single one of his papers, despite being the leading authority on atmospheric rivers. There is also a book by Springer on atmospheric rivers, recently published.

Moreover, a recent study by Valenzuela and Garreaud (2019) presents the linkage between atmospheric rivers and heavy precipitation in Chile.

Valenzuela, R. A., and R. D. Garreaud, 2019: Extreme daily rainfall in central-southern Chile and its relationship with low-level horizontal water vapor fluxes. J. Hydromet., 20 (9), 1829–1850, doi:10.1175/JHM-D-19-0036.1.

See also the following article.

Viale, M., R. Valenzuela, R. D. Garreaud, and F. M. Ralph, 2018: Impacts of atmospheric rivers on precipitation in southern South America. J. Hydrometeor., 19, 1671–1687, doi:10.1175/ JHM-D-18-0006.1.

4. The authors discuss the concept of airmass interfaces at line 156–158, but fail to discuss the previous literature on air masses, airstreams, and airstream boundaries. In addition to the Cohen papers already mentioned, there are the airstream models of Carlson (1980) and Browning (1990), among many others, let alone the concepts of airmass analysis discussed by the Bergen School meteorologists, especially Bergeron (1928, 2020) (Schultz et al. 2020 discusses the importance of this article to airmass analysis and interpretation in more detail).

Carlson, T. N., 1980: Airflow through midlatitude cyclones and the comma cloud pattern. Mon. Wea. Rev., 108, 1498–1509.

Browning, K. A., 1990: Organization of clouds and precipitation in extratropical cyclones. Extratropical Cyclones: The Erik Palme ÌĄn Memorial Volume, C. W. Newton and E. O. Holopainen, Eds., Amer. Meteor. Soc., 129–153.

Bergeron, T., 2020: Three-dimensionally combining synoptic analysis. First part: Fundamental introduction to the problem of airmass and front formation [originally published as Über die dreidimensional verknüpfende Wetteranalyse. Erster Teil: Prinzipielle Einführung in das Problem der Luftmassen- und Frontenbildung, Geofys. Publ., V (6), 1930]. Edited by David M. Schultz, translated by Gerald Prater, Bull. Amer. Meteor. Soc., 101 (Suppl.), doi: 10.1175/BAMS-D-20-0021.2.

Schultz, D. M., H. Volkert, B. Antonescu, and H. C. Davies, 2020: Defender and expositor of the Bergen methods of synoptic analysis: Significance, history, and translation of Bergeron's (1928) "Three-dimensionally combining synoptic analysis". Bull. Amer. Meteor. Soc., 101, E2078–E2094, doi: 10.1175/ BAMS-D-20-0021.1.

5. A very similar figure to Fig. 6 was published by Thomas and Schultz (2018, Fig. 15). Your manuscript should discuss this comparison. More generally, the discussion of how to analyze airmass boundaries and airstream boundaries are more thoroughly discussed in this article, which would seem to be highly relevant to your manuscript. Some discussion of these issues, citing this article as needed, should likely be included in your manuscript.

Thomas, C. M., and D. M. Schultz, 2019: Global climatologies of fronts, airmass boundaries, and airstream boundaries: Why the definition of "front" matters. Mon Wea. Rev., 147, 691–717, doi: 10.1175/MWR-D-18-0289.1.

This article may also be relevant.

Thomas, C. M., and D. M. Schultz, 2019: What are the best thermodynamic quantity and function to define a front in gridded model output? Bull. Amer. Meteor. Soc., 100, 873–895, doi: 10.1175/BAMS-D-18-0137.1.

6. There is an abundant and important literature on the SACZ that is not cited. I'm not familiar with all of this literature, but the authors should see if any of the results from these articles (as well as others) are relevant to their own conclusions in sections 5

and 6.

Robertson, A. W., & Mechoso, C. R. (2000). Interannual and Interdecadal Variability of the South Atlantic Convergence Zone, Monthly Weather Review, 128(8), 2947-2957.

Liebmann, B., Kiladis, G. N., Marengo, J., Ambrizzi, T., & Glick, J. D. (1999). Submonthly Convective Variability over South America and the South Atlantic Convergence Zone, Journal of Climate, 12(7), 1877-1891.

Grodsky, S. A., & Carton, J. A. (2003). The Intertropical Convergence Zone in the South Atlantic and the Equatorial Cold Tongue, Journal of Climate, 16(4), 723-733.

Liebmann, B., Kiladis, G. N., Vera, C. S., Saulo, A. C., & Carvalho, L. M. V. (2004). Subseasonal Variations of Rainfall in South America in the Vicinity of the Low-Level Jet East of the Andes and Comparison to Those in the South Atlantic Convergence Zone, Journal of Climate, 17(19), 3829-3842.

Carvalho, L. M. V., Jones, C., & Liebmann, B. (2002). Extreme Precipitation Events in Southeastern South America and Large-Scale Convective Patterns in the South Atlantic Convergence Zone, Journal of Climate, 15(17), 2377-2394.

Castro Cunningham, C.A. and De Albuquerque Cavalcanti, I.F. (2006), Intraseasonal modes of variability affecting the South Atlantic Convergence Zone. Int. J. Climatol., 26: 1165-1180. https://doi.org/10.1002/joc.1309

B. The second part deals with the consistency of this manuscript with the previous literature.

1. Is VIMF the same as IVT, used in the atmospheric river literature? If so, stick with conventional terminology already published. If a quantity has already been published, please don't introduce new terminology to describe the same features. Don't make the literature more impenetrable than it already is. If not, relate your quantity to previously published metrics of moisture transport.

2. In the same vein is the introduction of the quantities such as the strain tensor. It seems these topics have been published before by others. Follow consistent variable names with the previous literature for ease of comparison, where possible. If your derivation comes from previous literature, please cite that literature more closely.

3. Cohen and Kreitzberg (1997, section 4d) used 12 h in their calculations to understand airstream boundaries, but you chose 2 days. They explained their rationale in a more substantive way than you did. Perhaps some reference to their discussion is worthwhile, as well as a comparison between the differences in your respective choices.

C. The third part presents other concerns related to terminology and readability.

1. The title gives the appearance of a much more general study than the one that is presented. The abstract clarifies that the authors specifically are interested in the "role of large-scale turbulence in shaping atmospheric moisture in South America." It would seem to make sense that "moisture" and "South America" appear in the title. Doing so will ensure that the authors find the most interested and appropriate readers for their manuscript. That's a win–win for readers and authors!

2. Through the mass continuity equation, low-level convergence zones require ascent. Thus, the implication that "organized cloud bands...are often regarded as convergence zones" and that "flow kinematics are not usually taken into account" (lines 2–4) seems a bit unusual. Convergence is a kinematic quantity, so I'm unclear on the point here. Furthermore, it would make sense for the authors to downplay this association because of the clear relationship between convergence and ascent (and hence, clouds and precipitation). Deformation cannot produce ascent through direct kinematic effects. There are places in the manuscript where I get the sense the authors understand this, but this message could be presented more clearly, helping to make the point that I believe that they are trying to make.

3. I found the large number of acronyms difficult to follow as the paper went on.

Some terms do not require introduction of an acronym. Too many unfamiliar acronyms makes it difficult for readers to follow your manuscript, as readers encountering an acronym that they can't remember are left to flip back through the paper to track it down. Also, acronyms make it difficult for readers who don't read the manuscript linearly from introduction to conclusion, but instead skip around through the manuscript to get the relevant information they require. Please eliminate many, if not all, nonstandard acronyms when you revise your manuscript. Doing so will improve the readability of your manuscript.

4. I had a hard time navigating the introduction. It seemed to be a series of paragraphs on various topics and citations rather than a coherent narrative that motivated the paper and got me as a reader interested in pursuing it further. Consider section 1.2, just as an example. The paragraphs appear to be about the following topics.

1. general South American geography and atmospheric rivers

2. Amazon convection and SAMS

3. extratropical cyclones and cold fronts

4. cloudiness algorithms and problems with EOFs

There is no flow or coherence between these paragraphs. I don't see how these topics come together to form a unified subsection on "Aspects of the moisture transport in South America". This unity and coherence needs to be improved and the organization of the introduction needs to be rethought. Please read Gopen and Swan (1990) for improving your coherence.

https://www.americanscientist.org/blog/the-long-view/the-science-of-scientific-writing

5. The purpose of this paper is only stated in the last sentence of the introduction, 100 lines in. "This study is motivated by the need to objectively link convergence zones to atmospheric flow features, which cannot be done using existing definitions." Furthermore, this statement hasn't been justified sufficiently for the readers to have buy-in

at this point in the manuscript. Simply put, this statement has not been sufficiently motivated at this point in the manuscript, further evidence that the introduction needs reorganization and rethinking.

6. I would caution the authors about using "objective" when they really "automated". In reality, most objective techniques are quite subjective. See section 18.2 in Schultz (2009) for further discussion of this distinction.

Schultz, D. M., 2009: Eloquent Science: A Practical Guide to Becoming a Better Writer, Speaker, and Atmospheric Scientist. American Meteorological Society, 412 pp. http://bit.ly/EloqSci.

7. Lines 156–157: I didn't really get the sense that this paragraph was a well-reasoned discussion of this issues. More development of these concepts is needed.

8. Section 7 is called "Summary and conclusions". What's the difference between summary and conclusions? Could just one word suffice?

9. I found section 7 a bit disappointing. It needs a better organization and narrative to tell the story of what the authors found.

10. In that regard, maybe it is the structure of the rest of the paper, but it seemed more like a series of sections that were not very well connected. I'm not sure what the solution is, but any revisions to the manuscript that the authors could make to tell a better, more engaging story would help. I feel that they know the narrative, but might they be assuming that the readers know as much as they do and automatically know the issues that they face and wish to address? In some ways, that's how it feels to me. Just remember that not all readers are intimately familiar with the issues that you are trying to solve. Take a step back in your explanations and present it to us in a way that makes us understand the same difficulties with the state of the science that you already know.

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2020.