Review of manuscript egusphere-2024-1348, with title “Lagrangian Coherent Structures to examine mixing in the stratosphere”, by Jezabel Curbelo and Marianna Linz.

This paper introduces a metric based on the density of singular features of a Lagrangian descriptor to diagnose mixing in the stratosphere. The new metric is compared with existing mixing diagnostics in a zonal mean (or equivalent latitude) perspective using the output of the climate model WACCM, and the results show good qualitative agreement over all. The manuscript is well written and the results are potentially interesting, but some extra analysis and clarifications are needed. After these, it should be suitable for publication in ACP.

Main comments:

1) Although stated in several parts of the text (lines 76-77, 314), it is not clear “what new perspective the new method provides on stratospheric mixing”, since the comparison with other mixing diagnostics gives an overall agreement.
I think the paper does not explore one obvious strength of the LCS-density diagnostic: the fact that it can diagnose large-scale stirring in longitude-latitude. The paper would greatly benefit from analyzing maps of LCS-density and relate the diagnostic with the main features of the stratospheric circulation, such as the location of the polar vortices, jets, regions of wave breaking, etc.

2) About section 3.2.1. If I understand correctly, the idea is that since equivalent latitude (ϕ_{eq}) has a seasonal cycle in PV-coordinates, then the seasonal cycle of effective diffusivity (κ_{eff}) is affected by this feature and might be “contaminating” the physical significance of κ_{eff}.

Latitude and equivalent latitude are not equivalent indeed, as stated in lines 242-243. The former is a geographical coordinate, the latter is a tracer contour-based coordinate: it represents the latitude at the edge of a polar cap enclosing the same area as a given tracer contour. Or in other words, it represents the area enclosed by a given tracer contour. Given a “well-behaved” tracer, at a given time there should be a one-to-one correspondence between a tracer contour value and ϕ_{eq}. So if I am not mistaken, plotting the time-evolving PV as a function of ϕ_{eq} should be equivalent to plotting the time-evolving ϕ_{eq} as a function of PV (as in Fig. 5), it is a matter of remapping one as a function of the other. All this to say that the seasonal cycle of ϕ_{eq} in PV-coordinates should be the same as the seasonal cycle of PV in ϕ_{eq}-coordinates. In fact, since the zonal-mean PV is monotic in latitude, one could perform a similar exercise and obtain a variable called “latitude” as a function of a “zonal-mean PV-coordinate”; one would expect an equivalent seasonal cycle in PVzonalmean(lat) and in lat(PVzonalmean).
The point I am trying to make is that κ_{eff}, as long as it is computed using PV as a tracer, will indeed be affected by the seasonal cycle of PV (lines 257-260), but this should be independent of κ_{eff} being defined in equivalent latitude coordinates, since PV has a seasonality in any coordinate.
[By the way, it is surprising that the negative contour of PV at 800K in the last panel of Fig. 5 is placed at ϕ_{eq}~40ºN, it does not look consistent with the fourth panel of Fig. 5 where the ϕ_{eq}=40ºN-contour is located at positive PV values.]

3) This takes me to my next comment. That band of high κ_{eff} at ϕ_{eq} = 20º-40ºN in JJA (Fig. 6b) is indeed quite interesting. As reflected in the text, there should be no dynamical reason for high mixing to occur in that region since Rossby waves are filtered out below in the summer stratosphere. In fact, that band does not appear in the reanalysis ERA-Interim (see Fig. 1c in Abalos et al. 2016). However, the κ_{eff} calculations in Abalos et al. were performed by integrating a diffusion-advection model to evolve a passive tracer, as conceived by Nakamura (1996). So the question remains whether that band appears when using PV to calculate κ_{eff}.

I happen to have calculated κ_{eff} for the same WACCM runs (CCMI refc1) that are used in the present study, as well as for ERA-Interim, that I used in a study some years ago. I have plotted in Fig. R1 a similar plot as Fig. 6b, but for a 60-year mean in WACCM (Fig. R1a), and for a 34-year mean in ERA-Interim (Fig. R1b). I also get that high κ_{eff} over 20º-40ºN in WACCM in a 60-year average, but that region is not present in the reanalysis, either computing κ_{eff} with PV or with a passive tracer (see Fig. 1c in Abalos et al. 2016). This suggests that the high κ_{eff} in the summer stratosphere is not a characteristic feature of κ_{eff} computed with PV, but it is true that it does show up in WACCM. Moreover, Fig. R1a (WACCM) also displays a region with very high κ_{eff} right inside the austral polar vortex (south of ϕ_{eq}=70ºS), with larger values than the ones at the surf zone (ϕ_{eq}=50ºS-30ºS). This behavior does not seem physical, and does not show up in ERA-Interim (Fig. R1b).

All of it evidences a rather nonphysical behavior of κ_{eff} in WACCM in specific parts of the stratosphere, but is it a limitation of the effective diffusivity itself, or is there something in the PV field in WACCM that makes it unsuitable to be used as a well-behaved tracer for these calculations? A way to check would be to calculate κ_{eff} using a diffusion-advection model to evolve a passive tracer (as in Abalos et al 2016, but for WACCM), and compare the results with κ_{eff} calculated using PV. It is just a suspicion, but since the LCS-based diagnostic has been calculated using the wind field and it does not show the high mixing band in the summer stratosphere, it would not be surprising that κ_{eff} calculated using the same winds does not show it either. I am not asking the authors to perform those new κ_{eff} calculations, which are computationally costly, but I suggest to edit the discussion on this issue to take these comments into account, specifically about the equivalent latitude coordinates. All in all, the absence of that nonphysical behavior in the other two mixing diagnostics speaks well of both of them.

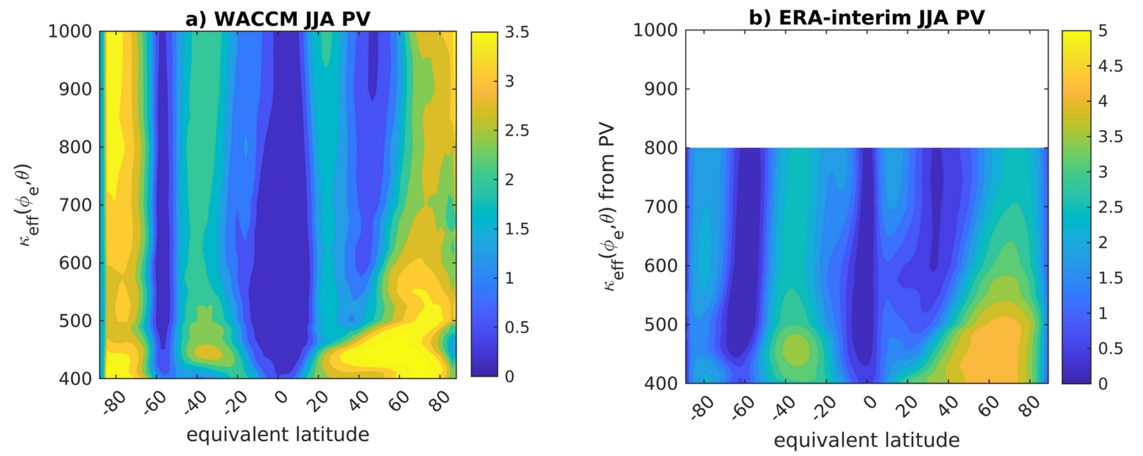

Fig. R1: Mean effective diffusivity in JJA as a function of equivalent latitude and potential temperature, for a) WACCM (60-year mean), and b) ERA-Interim (34 year-mean). Both metrics have been calculated using potential vorticity fields on isentropes.

Other comments:

- Why is the analysis performed using a climate model output, and not a reanalysis? Are there any limitations (expected errors) because of the use of daily mean velocity fields for the trajectory calculations? Why using daily mean fields instead of instantaneous output?

- Calculation of the Lagragian Descriptor: What time interval [-tau, tau] has been used for the calculation? Is there a strong sensitivity of this parameter? The radiative timescales are much shorter in the upper than in the lower stratosphere, so the consideration of PV as a passive tracer is a better approximation in the lower than in the upper stratosphere.

- Line 117: What is the “corresponding time interval” used to calculate the PDF of the norm of the gradient of M?

- Lines 120: LCS is signaling chaotic advection, not diffusion. Although both should be related.

- Fig. 2. I would suggest to plot the zonal mean zonal wind (in LCS-density and Deff) to easily relate the transport barriers and high mixing with the presence of zonal-mean jets.

- Fig. 2. Also, why showing only up to 60º latitude / equivalent latitude? It would be interesting to analyze mixing inside the vortex.

- Line 193. core of the polar vortices → polar jets .

- Fig. 3: Would it be useful to simply show the seasonal evolving average of the few years analyzed? It is difficult to discern summer or winter in this figure.

- Lines 324-325. On the relation between LCS and diffusion, which also appears in other parts of the text. Please correct if I am wrong, but LCS are defined for Hamiltonian (conservative) systems, so the trajectories performed are purely advective. What would be the tentative approach to quantitatively reconcile this paradigm with the notion of diffusion, which is an intrinsically non-conservative process?

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