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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Trends and variability in stratospheric mixing: 1979–2005" *by* H. Garny et al.

Anonymous Referee #3

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General comments

The paper addresses the inter-annual trends and variability of stratospheric mixing, covering the tropics through the mid-latitude to the high latitudes, emphasizing the differences between the northern and southern hemisphere where necessary. The technique used is the finite-time Lyapunov exponent. Linear trends of stratospheric mixing and correlation with the QBO signal are significant findings. The overall quality of the paper is high and the discussions of the results are pertinent and illuminating.

Specific comments

1. The Lyapunov exponent has a long history in the literature: they were widely used

in dynamical systems theory to characterize the sensitivity of chaotic systems to initial perturbations. But it has an important shortcoming: the theoretical requirement of taking an infinite time limit (a "self-evident" procedure in asymptotic analysis in mathematics) is not met in practice for aperiodic systems observed over a finite time domain. This raises the question of how to generalize the Lyapunov exponent to apply to practical problems. The introduction of the paper should bring out this important issue and cite a few pieces of work that attempt to overcome it. Two helpful references that come to mind are Koh and Plumb (2000) [deformation exponent] and Joseph and Legras (2002) [finite-SIZE Lyapunov exponent], besides the older and already cited Pierrehummbert and Yang (1993) [finite-TIME Lyapunov exponent]. In a way, the finite-time Lyapunov exponent is only a first cut at generalizing the (original) Lyapunov exponent (cf. Appendix B of Koh and Plumb, 2000). Some mention should also be made that all the above "generalized exponents" are useful in identifying Lagrangian flow structures pertinent to mixing. This means that these exponents have good physical basis within the Lagrangian flow kinematics and hence provide further justification for their use as indicators of mixing.

2. The definition of the Lyapunov exponent in the first line of Section 2 is not correct. Taking the infinite-time limit on the right-hand side will result in a unique value for lambda (the largest Lyapunov exponent) for all orientations of the initial vector $\delta x(t_0)$, except for a certain (n-1) dimensional subspace S. For initial vectors $\delta x(t_0)$ in subspace S, all orientations will result in another unique value for lambda (the second largest Lyapunov exponent), except for a certain (n-2) dimensional subspace T of S. So on and so forth for the third, fourth, fifthĚ largest Lyapunov exponent, assuming all Lyapunov exponents are distinct. Any mathematical text on dynamical systems and chaos theory would provide better reference for a rigorous definition of the Lyapunov exponent.

3. Two different Lyapunov exponents of a 2D flow are associated with two unique directions. But these directions need not be orthogonal. Hence, line 18 of page 6193 ("initially perpendicular") is not correct. In the sentence that follows, the mention of

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Pierrehumber and Yang (1993) which dealt with finite-time Lyapunov exponent is inappropriate: the reader might become confused between the rigorous (original) Lyapunov exponent and the ad-hoc generalizations to realistic applications.

4. Line 9 of page 6194: The computed FTLE in this paper uses NCEP/NCAR reanalysis winds and so represent advective effects, albeit at mesoscales (>1km). So they do not account for diffusion. What the authors mean is probably that these mesoscale advective transport might be parameterized as a diffusive term when considering global scale transport.

5. The use of error estimates on Lyapunov exponents and providing a statistical measure of significance are excellent efforts!

6. The authors could provide an explanation why N is chosen to be 4 only for the a_1 term and not for the other terms in equation (2).

7. 2nd paragraph of Section 4: inspection of the southern hemispheric plot in August 2002 at 550K (Fig. 2) shows that spiral arm-like structures of high FTLE seem to extend cyclonically out of from the polar vortex. It is very plausible that FTLE (like FTSE and other Lagrangian measures) is able to pick out stable manifolds in the stratospheric flow. [cf. Fig. 5 (left panel) and Fig. 7 (blue lines) in Koh and Legras (2002)]. Note that PV tongues are usually associated with unstable manifolds (which spiral anticyclonically out of the polar vortex) and so are not expected to be picked out by FTLE computed from forward-time trajectories in this paper. In contrast, the stable manifolds picked out in Fig. 2 are associated with the steepening of tracer gradients and strong mixing. The connection between FTLE and stable manifolds serve to strengthen the physical basis for using FTLE as a diagnostic of mixing.

8. Given that QBO effects are sensitive to the seasonal cycle of the year and the seasonal cycle of the year is out-of-phase between the northern and southern hemisphere, there is a concern that using the same Δt (which are of a few months in magnitude) for both hemispheres in the regression with the QBO index may obscure the diagnostics

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of some essential dynamics. The authors may wish to discuss briefly this point and if possible, address it by optimizing the regression for QBO using different Δt for the northern and southern hemispheres.

9. Good and clear dynamical reasoning at the end of page 6210.

Technical corrections

1. Line 25 of page 6201: the sentence "The first coefficient Ě for all latitudes and times" should be moved to the caption of Fig. 7 for easy location of this information, since the significant levels of the other two coefficients are mentioned in the caption too.

2. Line 2 of page 6209 should continue from the last paragraph; line 6 of the same page (starting from "The mechanismĚ") should start in a new paragraph. This will separate and cluster the discussion on the QBO West and East phases better.

3. Line 23 of page 6209: insert "QBO" before "east minus west" in the brackets to make clear that the directions are referring to the QBO phase and not the wave fluxes.

4. Line 27 of page 6209: the "positive correlation" is hanging in the paragraph without explicit reference to what is correlated to what. It is clearer to spell out the correlation explicitly, e.g. "Lyapunov exponents are enhanced during the QBO west phase and reduced during the QBO east phase".

5. Line 9 of page 6211: I think the clause should read as "higher **Lyapunov exponents** will result not only **from** higher **wave activity** but also **from** higher shear due to a stronger polar jet" rather than as written.

References

Koh, T. Y. and R. Plumb (2000), "Lobe dynamics applied to barotropic Rossbywave breaking", Phys. Fluids, Vol. 12, 1518-1528.

Joseph, B. and B. Legras (2002), ‘‘On the relation between kinematic boundaries, stirring, and barriers for the Antarctic polar vortex,’’ J. Atmos. Sci. Vol. 59, 1198-1212.

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 6189, 2007.