

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

H. Garny et al.

Received and published: 2 August 2007

Anonymous Referee #1 Received and published: 14 June 2007 This paper analyzes the mixing properties of the stratosphere for 1979–2005 using the finite-time Lyapunov exponents computed from the NCEP/NCAR reanalysis winds. The length of the dataset allows the authors to perform a meaningful multiple regression analysis, which includes the climatology, linear trend, QBO, solar cycle, and ENSO. The climatology and the QBO signals agree reasonably well with the previous studies based on shorter datasets. The trends in the Lyapunov exponents and in the zonal mean winds are new, which reveal interesting features such as increased mixing in summertime southern surf zone in the lower stratosphere. Although the paper does not contain substantive results on the solar cycle and ENSO signals, the overall treatment is thorough and the paper is publishable with minor revisions. I do have a few comments/questions con-

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cerning the analysis techniques. (1) In Section 5.6 averaging along equivalent latitude is introduced and it is shown to improve the definition of the Arctic vortex. (The same point is emphasized again in Section 6.) If equivalent latitude improves the analysis, why not use it from the beginning?

We did use the equivalent latitude analysis from the beginning and in fact performed the entire analysis in two parallel streams, one using true latitude zonal means and one using equivalent latitude zonal means. However, we focussed the presentation of the results on true latitude zonal means for two reasons: 1) Many readers are not comfortable with the interpretation of results in an equivalent latitude coordinate system. This coordinate system is less familiar than true geographic latitude and if we were to present the results in equivalent latitude only, it would discourage some readers. 2) The only significant differences between the equivalent latitude and true latitude zonal mean results is close to the vortex edge where the use of equivalent latitude preserves the steep meridional gradients in the mixing diagnostics. It was in such cases that we did include the equivalent latitude results. So, in summary, we decided to show results primarily as true latitude zonal means since these would be most familiar to most readers, and then, when the equivalent latitude zonal means showed features that were significantly different to those calculated from true latitude zonal means, the equivalent latitude zonal mean results were shown and discussed.

Perhaps more to the point, the paper does not quantify the improvement brought about by the equivalent latitude (other than showing the differences from the zonal-mean analysis).

It is true that we do not quantify the improvement brought about by using equivalent latitude, but we felt that there was nothing to be gained by explicitly doing so. Our intention was simply to use an equivalent latitude coordinate system to show more clearly, in cases where gradients across the vortex edge are steep, that the features in mixing are very narrow and narrower than would be seen in true latitude zonal means only.

An important point is that after one month of advection, the particle pair can drift significantly from the original latitude. Thus, the 30-day Lyapunov exponent does not necessarily represent the stretching rate at the fixed latitude. Associating it to the original latitude and taking the zonal averaging will introduce errors. This problem will be (in principle) alleviated by using equivalent latitude, because the particle pair should stay close to the original PV level (equivalent latitude) as long as the particles and PV are advected by the same wind. Thus, the question is whether the meridional dispersion of particles from the original latitude after 30 days is indeed smaller if equivalent latitude is used. The modest difference between Figs.8a and 11 suggests this may not be the case in most of the domain due, for example, to the nonadvective sources/sinks of PV. On a related matter, does the lack of strong mixing in the summer NH subtropics on 450 K (Section 5.2) and of ENSO signal (Section 5.5) change when equivalent latitude is used?

The regression analysis of the Lyapunov exponent fields was done as well for averaging over equivalent latitude as for zonal means. The comparison of the two fields revealed that the only relevant difference lies in the polar regions, where the equivalent latitude averaged mixing fields show stronger gradients at the vortex edge (so the answer to the last question is no, there are no significant changes for the ENSO signal, and mixing in the summer NH is, if anything, even slightly weaker in the equivalent latitude based plots). Therefore, it was chosen to show only one example for the equivalent latitude based plots, which shows the one relevant difference (figure 11). It is of course true, that the advection of the parcel within the 30-day period will make it impossible to interpret the Lyapunov exponents as the mixing rate at a fixed latitude. The more correct interpretation is that the Lyapunov exponent values represent the amount of mixing that an air parcel originating at a specific position will experience over the next 30 days. So taking for example an air parcel originating at the vortex edge, the resulting Lyapunov exponent can tell us to what degree this air parcel will be mixed into its environment, but there is no information in which direction the mixing occurs (i.e. into lower latitudes or into the interior of the vortex). This also explains why using

equivalent latitude results in changes mainly at high latitudes, since by averaging over equivalent latitude all air parcels originating at the vortex edge are at the same latitude and consequently a steeper gradient in mixing is obtained than when averaging over geographic latitude.

(2) The authors use five predictors with specified basis functions in their regression, and in this framework, their analyses are thorough. I wonder if they also considered EOF analysis. Given that the observation is incorporated for the QBO and ENSO, and that the annular mode (not included) has no preferred frequency, there may be a merit to letting the data determine their own basis functions. I am not suggesting that the authors should use EOFs, but given the available choices, some rationale for the current regression method would be nice.

Yes, we did consider EOF analysis. The advantage of EOF analysis is that the data will determine which basis functions describe the most variability and these basis functions will be orthogonal. Mathematically, this is very attractive. The disadvantage is that no geophysical meaning can be ascribed to the resultant basis functions. So while we could say “This pattern describes x% of the variability” we could not say what relevance this has to the real world. One of the goals of this work was to reveal the geophysical drivers of long-term changes in stratospheric mixing and so that is why we decided to use a regression approach constructed from basis functions representative of real world geophysical drivers of changes in mixing.

Other points: p.6193 LL9-10: Would backward trajectory calculation give similar results (mixing should represent the history of particle trajectory of recent past)?

Since backward trajectories are calculated simply by reversing the sign of the zonal and meridional wind components, and running time backwards, we believe that backward trajectories would give very similar results and only the interpretation would need to be changed i.e. instead of interpreting the Lyapunov exponents as the degree of mixing that will occur over the coming 30 day period, they would need to be interpreted as

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the mixing that had occurred over the preceding 30 day period. We have no reason to believe that using backward trajectories would change in any way the conclusions drawn in this paper.

p.6194 LL9-10: “ only diffusion and not advection is accounted for ” This is confusing because the method is purely advective. Please rewrite as “ only differential and not mean advection is accounted for”

True and good correction, the text has been changed as suggested.

p.6194 LL18-20: The disagreements occur not necessarily when the scales are small, but when the residence time of the particle pair in the region of interest is shorter than the time used to compute the exponent (in this case 30 days).

The formulation ‘disagreements in the small scale structure’ was not a good choice of words and this has now been changed to ‘disagreements in the detailed structure’, and a sentence about the disagreements caused by strong advection within the time period of calculation of the FTLE has been added.

p.6195 L6: There should be some rationale for $\Delta x(0) = 1$ km. From Fig.3, it appears that a reasonable upper bound for the Lyapunov exponents is 0.2 (1/day). This will give a separation of 400 km at the end of the 30-day period. This final separation must be smaller than the size of the region of interest. For example, this is sufficiently smaller than the radius of the polar vortex.

A sentence discussing this issue has been added to the paper.

Anonymous Referee #2 Received and published: 31 May 2007 This is a technically sound and well-organized paper that investigates long-term variability of fine-scale mixing in the lower stratosphere. The technique is simple and straightforward: It uses a measure of the separation of two parcel trajectories with time from initially adjacent starting points (Lyapunov exponents). Once time series of this quantity are constructed at all locations on a given isentropic surface, a multiple regression statistical model is

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used to estimate trends and other natural components of interannual variability on the 450K (15–17 km), 550K (22 km), and 650K (25 km) surfaces. The resulting regression coefficients are then discussed with respect to physical processes, especially the QBO, that influence planetary wave activity in both hemispheres. Specific comments:

(1) The only statistically significant and seasonally persistent trends occur at southern middle to high latitudes at the 450 K level. They are slightly positive ($0.01 \text{ (day}^{-1}\text{)}/\text{decade}$). The authors note that this trend is “consistent with an increase in winter-time wave 1 amplitudes at 60S over the same time period” (Bodeker et al., 2007). A positive trend in zonal wind occurs near 60S in October (Figure 14), which has been attributed to polar cooling associated with ozone depletion (Thompson and Solomon, 2002). The authors therefore suggest that the anthropogenic increase in zonal wind may be increasing the small-scale mixing at middle latitudes. This seems to be a reasonable hypothesis that could be investigated further.

We agree that this hypothesis could be investigated further and this is the subject of follow-up work that we have initiated. Including this analysis in this paper would be a large and significant extension of the paper and we believe beyond the intended scope of this paper. We are pleased that the reviewer has also identified this as an avenue for additional research but we would prefer to separate out this new research into a second paper.

(2) In general, one would expect that small-scale mixing would correlate with planetary wave activity as measured by E-P flux or eddy heat flux. Near 45N, some evidence for negative trends in January eddy heat flux at 100 hPa, implying a weakening Brewer-Dobson circulation, has been reported (e.g., Randel et al., 2002; Hood and Soukharev, JAS, 2005). These negative trends in the B-D circulation have been suggested to be a contributor to negative ozone trends at middle latitudes in that hemisphere. According to Figure 7 (center panel), there is a statistically significant negative trend in smallscale mixing during December and January near 40N. This result may therefore be consistent with the eddy heat flux analyses reported by the above authors.

We thank the reviewer for bringing this confirmation of our conclusions to our attention. A sentence has been added to highlight the consistency of our results with these earlier studies.

(3) Although the Lyapunov exponent technique of measuring the rate of small-scale mixing is a useful tool, it should be emphasized that this parameter does not fully characterize planetary-scale or synoptic-scale wave activity. Specifically, it does not distinguish between anticyclonic poleward and cyclonic equatorward wave-breaking events, which commonly occur in the lower stratosphere (e.g., Peters and Waugh, JAS, 1996). The type of wave-breaking event that dominates depends on the meridional wind shear, which is modified if there is a trend in the polar vortex strength. Increased numbers of anticyclonic poleward events can produce increased numbers of dynamically forced ozone minima, including “mini-holes”, which also contribute to midlatitude ozone trends (Hood and Soukharev, JAS, 2005). So, more detailed studies are needed beyond that reported in the present manuscript in order to understand long-term changes in wave activity (as opposed to small-scale mixing) and their effects on ozone and other longlived trace gases.

We agree with the reviewer that the diagnostic of Lyapunov exponent does not give any indication about the physical processes behind mixing (see e.g. p. 6208, LL. 12-15 and p. 6211, LL. 9-10). A detailed study on the relationship between wave activity and mixing, and their influence on trace gases will be necessary to understand these processes and this is something that we are focussing on in followup work (see comment above on extension of this research). The study presented here aims mainly to analyse the trends and variability in mixing over a long time period, and the ideas for dynamical processes behind the findings as well as the implications that are suggested in the Discussion (Section 6) need further analyses. We thank the reviewer for pointers to these key papers that will aid our ongoing studies in this field.

Anonymous Referee #3 Received and published: 24 June 2007 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 Pierrehumbert 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 λ (the largest Lyapunov exponent) for all orientations of the initial vector $\mathbf{x}(t_0)$, except for a certain $(n-1)$ dimensional subspace S . For initial vectors $\mathbf{x}(t_0)$ in subspace S , all orientations will result in another unique value for λ (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, fifth 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 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.

All three comments above seem to result from inadequate differentiation in the paper between the Lyapunov exponent spectrum defined as the limit to infinite time and the finite-time Lyapunov exponent and its realization of the calculation of FTLEs. The text in section 2 of the paper has been changed extensively to better distinguish between the theoretical definition and its practical realisation. Furthermore, in the introduction, a few sentences about this problem have been added. The definition of the Lyapunov spectrum is set in the right context, with emphasis on the directions of the n Lyapunov exponents in the spectrum being the principal axes of a deforming n -dimensional ellipsoid, and not, as originally stated, any orthogonal vectors. This also corrects the error stated in comment 3. By clearly distinguishing between the theoretical definition of the Lyapunov spectrum and the FTLE as its realisation, the reference of Pierrehumbert and Yang (1993) in line 19 of page 6193 has been set in the right context.

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 (>1 km). 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.

The reviewer is correct and the sentence has been changed according to the suggestion of referee 1.

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

We thank the reviewer for their encouraging comment.

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).

The annual cycle in the signals is a very robust underlying structure with very small statistical uncertainty. Therefore 4 Fourier pairs could confidently be fitted to the offset term which then describes the mean annual cycle. The annual dependence of the other basis functions has much greater statistical uncertainty and choosing $N=4$ in such cases would very likely result in 'overfitting' of the regression model. We therefore selected only 2 Fourier pairs to describe the seasonal dependence in these remaining basis functions as these are sufficient to describe the broad scale intra-annual structure without subjecting the regression to the likelihood of overfitting. This does mean e.g. that if the trend in mixing was much larger in October than in November, the regression model would tend to smooth over this, under-estimating the trend in October and over-estimating the trend in November, but it would still capture the gross features of the seasonal cycle in the trend. Our interpretation of the seasonality in the mixing coefficients is cognizant of the resolution achieved in the regression model by setting $N=2$ for the terms other than the offset (which incorporates the annual cycle). We did test the model with higher values of N for the other terms and found that the results did not change.

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 anticy-

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clonically 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.

We thank the reviewer for this very interesting comment that highlights another use of FTLEs. Since this paper focuses on the analyses of the long-term variability in zonal averaged mixing, including this aspect would go beyond the intended scope of the paper. But this gives another explanation why the direct comparison of monthly mean PV and monthly FTLE need to be made with care. For one, the FTLE needs to be compared with the PV field on the starting day of the trajectories and not the monthly mean, since the calculated FTLE over the one month period are associated with their starting coordinates. And since your comment suggests that PV and FTLE as calculated here capture different types of manifolds, not much could be expected from a comparison of them anyway.

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 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.

The shift in the phase of the QBO is estimated by varying the shift between -12 and +12 month and searching for the Δt with the minimum sum of squared residuals. Determining Δt separately for the northern and for the southern hemisphere results in the same values of Δt for 550 K, and in a difference of 2 and 3 month, respectively, for 650 K and 450 K. Looking at the resulting function of the sum of squared residuals of Δt shows that it has broad minima, which match quite well for the northern and

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southern hemisphere. So the difference of the sum of squared residuals for the 2 or 3 month difference in Δt is small, and it is a good assumption to use the same value of Δt for all latitudes. This is confirmed by running the regression model with the slightly different Δt for the two hemispheres, which results in almost the same pattern of significant correlations between the QBO and the FTLE field. Note however that using the same Δt for both hemispheres does not impose the same annual seasonality in the QBO regression coefficient. Certainly the seasonal cycle in the QBO term is permitted to differ between hemispheres - and it does, maximizing in the winter-time of each hemisphere.

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

Thank you for this positive comment.

Technical corrections 1. Line 25 of page 6201: the sentence “The first coefficient... 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.

The suggested change has been made.

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

We prefer to leave the paragraphs as they are, since in the first paragraph (p. 6208, L.16 to p. 6209, L.1) talks about effects on trace gas distributions, while the next paragraph (p. 6209, L. 2 to p. 6209, L.24) talks about the QBO modulation of mixing in high latitudes.

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.

The suggested change has been made.

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”.

The suggested change has been made.

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.

The suggested change has been made.

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

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