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Interactive Comment

Interactive comment on "Variability of the Lagrangian turbulent diffusivity in the lower stratosphere" *by* B. Legras et al.

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This paper attempts to estimate vertical diffusivity in the lower stratosphere by comparing ER2 observations of tracer spatial structure to Lagrangian simulations created from a modeled tracer field at an earler time. The diffusion is incorporated by superimposing a vertical random walk on back trajectories. Each point along the reconstructed transect is an average over a set of 1000 points in the tracer field at the earlier time. This amounts to a smoothing of the reconstructed transect, relative to the D=0 case, because nearby tracer values are more similiar than they would be if each point went back in time and fetched a single value from the tracer field at the earlier time. The paper is well-written and raises some interesting questions about how to extract information about mixing processes from tracer measurements. For this reason I support its publication, provided the authors address the questions related to the sampling by the aircraft platform (comment 1) and clarify the role of diffusion (comment 2) as discussed below.

1) My main objection to the work here is the lack of discussion about the strong correlations between the structures seen in ozone and N2O and similiar structures in the potential temperature and altitude data. There are many segments for which the ozone and theta structures line up almost perfectly, for example the Feb 2 flight. After about March 11 the correlations are not so clear, but prior to this I would estimate that about 40% of the data shows strong correlations.

1.a) As discussed by Sparling and Bacmeister, GRL, 2001, a possible explanation is that the aircraft is moving through a tracer field perturbed by (linear) gravity waves. The detailed structure of the tracer - as measured by the moving platform - depends on the local vertical gradient, the amplitude of the gravity waves or the vertical motion of the aircraft for whatever reason. This gives rise to structure on all scales, with values up to 10-20x instrumental noise level for ozone. While the ultimate origin of this structure is geophysical, it is "spurious" in the sense that details about the motion of the platform could impact the roughness measure used here. It is important that the authors make clear how this was taken into account, whether or not the parcels along the flightpath have been initialized at the actual altitude of the measurement (except for obvious cases, such as the dips), and how this impacts the conclusions of this study.

1.b) p.8297, line2, and Fig 6: For the Jan 27 N2O transect, "It is more difficult to associate observed and reconstructed structures inside the vortex for time $t > 13$ h...". The potential temperature data shows that the N2O structure in this portion of the flight is VERY strongly correlated with potential temperature.

1.c) I am not sure much can be learned about diffusion via the "single-filament" approach, as in Section 7. I am not surprised to see filaments that are not symmetric.

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The ER2 potential temperature data in the vicinity of this structure shows a 5K variation that may give some insight about the different slopes.

2) The method of adding diffusion to back trajectories seems intuitively appealing, but I am not clear on the role of the added diffusion.

2.a) The discussion in section 8.1 and Fig. 10 suggests that the effect of the diffusion is most important in the first few days back. Its main role seems to be to puff up the initial point parcel so it can be acted upon by the shear. The 1000 subparcels are subsequently dispersed mainly by the winds (see also Figure 14). Is this correct? I would think that the altitude variance at t=t_o-tau for the points along the initial track shown in Figure 2 is a measure of the model vertical dispersion alone when D=0 (properly taking into account the initial altitude variance along the track). In this example, the "single realization" vertical variance is on the order of the variance shown by the yellow curve which also includes diffusion. Is this true for the other flights?

2.b) How important then is the integrated effect of diffusion along the parcel paths compared to the initial spreading? If the initial spreading is the dominant effect, then would the end result be any different if a vertical line of parcels was initialized along the flightpath instead, with a length of 2sqrt(2Dt*)?(˜ 150 meters for D=0.01 mˆ2/s)

2.c) Figure 2 also shows that the origin of the tracer variability along the transect for the D=0 case is the variability in the vertical displacement. The discussion of Figure 7 suggests that the model is producing excess structure from spurious vertical transport, and it is acknowledged that the added diffusion might be an upper bound because it has to damp out the spurious vertical dispersion. On the other hand, this same spurious vertical transport leads to averaging over a wider domain at t=t_o-tau which leads to a smoother reconstruction, suggesting a that smaller diffusion would suffice after the initial time.

2.d) The D=0.001mˆ2/s transects differ substantially from the D=0 reconstructions compared to the D=0.01mˆ2/s (Fig. 6). Very small, but non-zero, D seems to be a kind of

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singular perturbation owing to the initial splitting; Figure 14 shows us what happens to a single point that is initially puffed up only by molecular diffusion. Please show a reconstructed transect for D=0.0001mˆ2/s (molecular diffusion). (I suppose we may have to start worrying about the concept of an "air parcel" in this limit.)

3) I don't understand the explanation in terms of the Green function for the convergence of the small scale structure when the calculation goes back 147 days.

3a) For large tau, why don't we expect parcels that began within a few hundred km of each other on the flighttrack to sample more or less the same region at time t_o-tau and so have the same tracer average? The authors say (p.8294,line20) that the Green function dependence on y gets smoother as tau increases, but doesn't the Green function dependence on x also get smoother?

3b) How close is the sum of the 1000 parcel delta functions to the Green function? I don't see how a tracer measurement can be reconstructed from 1000 samplings of the tracer field up to 4 months in the past.

3c) Could a scatterplot of N2O(t_o-38 days) versus N2O(t_o-147 days) be provided for the case shown in Figure 3?

4) Roughness measure: 1/ p is the curvature of the local parabola, so p corresponds to scale, i.e. small p˜small scale. This shows up in a greater sensitivity ot roughness to a change in the shift, i.e. the noise at small p. The measure has units of timeˆ2/ppbv, so it strongly emphasizes the time separation relative to an increment measure. It's an interesting concept, but hard to interpret, compared to a vanilla Mexican hat wavelet for example which would integrate over the small scales and thus also be less sensitive to the noise. In any case, do we expect one value of D to be able to match the roughness over the whole range of p? In some of the cases shown, it clearly doesn't, for example Fig. 6o shows that D=0.1 m2/s matches the data best at large p while D=0.001mˆ2/s is best for small p. For this case, D is reported in Table 1 as D˜>0.01m2/s. In Fig. 6p, would the roughness curve be a better overall match to a D=0.5mˆ2/s, or D=1.0mˆ2/s

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curve (not shown)?

5) Please define "inertial volume" (Figure 10). It must be something other than regular volume if it is growing exponentially in a flow which is incompressible (following page). Does this imply that the tracer concentration is decreasing exponentially when diffusion acts perpendicular to the main axis of strain, and how then should the change of concentration of parcels be taken into account?

Other comments:

- Fig 4: right panel says tau=47 days, but caption says 147. - Fig 11. Caption says 3 Nov 2000 flight, text says 11 march 2000 flight. - In many of the figures, it is not the tracer variance that is being shown, but its square root. - The stretching history (Fig. 11) is an interesting thing to think about. The discussion there should include a reference to Bacmeister et al, JGR, 104, 1999.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 8285, 2004.