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Interactive Comment

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

Anonymous Referee #2

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This paper is devoted to an estimation of a lower-stratospheric turbulent diffusivity based on aircraft measurements of N_2 O and O_3. Diffusive backward trajectories are used to reconstruct concentration transects for several values of the diffusivity. The estimated diffusivity is then the value for which the reconstructed transect best matches the observed one. This leads to values of 0.1 mˆ2 sˆ{-1} in the surf zone, and 0.01 mˆ2 sˆ{-1} inside the polar vortex.

The paper is interesting and well written, and I support its publication. My only major criticism concerns the lack of detailed discussion comparing the new estimates with those obtained earlier, in particular by Waugh et al and Balluch & Haynes. These authors used aircraft data similar to those used here, and methods which do not differ

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Interactive Discussion

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much from that used here. Yet, they found diffusivities in the surf zone lower than 0.01 m^2 s^{-1}. The discrepancy is large enough to require an explanation: does it reflect differences in the data (which would suggest large year-to-year variations in the diffusivity), or in the estimation methods? The authors stress that their diffusivity estimate is in fact an upper bound, because spurious concentration fluctuations are probably generated by the 3-hourly wind they use. Can a bound be put on this effect (which presumably also affected Waugh et al and Balluch & Haynes's results)?

I list a number of minor points below.

1. p8290, l6: The assumption of independent noises for neighbouring particles (which are likely to have experienced the same turbulent regions) is questionable. The authors could comment on this.

2. p8291, l3: (4) assumes incompressibility.

3, section 8: I am not convinced of the interest of this section. It seems dubious that the diffusivity be related in a predictive way to the stretching properties of the flow. Turbulence is thought to be caused by shear instabilities and gravity-wave breaking, and I see no reasons why these should appear preferentially in regions of large, persistent horizontal strain.

4. p8299, l28, and p8301, l15: "number of unstable directions". Is it not the case that the vertical velocity is so weak that one finite-time Liapunov exponent is best considered to vanish (i.e. that the stratosphere is in regime (ii) of p8301)? I am confused by the use of a norm magnifying the vertical direction: the infinite-time Liapunov exponents are independent of the choice of norm, so the impact of multiplying the vertical displacements by \Lambda/\gamma depends on the integration time. It is difficult to see, then, what justifies it.

5. p8301, l27 and p8302, l3: The two variances referred to are different (variance of the Liapunov exponent, and of the concentration). Please clarify.

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6. p8306, l11: "the need to filter...". The sentence is unclear: the turbulent diffusivity results from the combination of unresolved turbulent motion and of spurious motion contained in the analyzed winds.

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