Atmos. Chem. Phys. Discuss., 4, S3268–S3271, 2004
www.atmos-chem-phys.org/acpd/4/S3268/
European Geosciences Union

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #3

Received and published: 11 January 2005

General Comments

This manuscript estimates the vertical diffusivity in the lower stratosphere by applying a relatively new method to a large aircraft database. The magnitude of these diffusivities is an important issue for modeling ozone and other trace gases, and previous estimates have varied by several orders of magnitude. This manuscript provides new estimates for diffusivities both inside and outside the polar vortex, and also examines the sensitivity of transport calculations to the temporal resolution of the winds. These are significant contributions to an important area of research, and warrent publication in ACP. The overall presentation of the analysis is good, however, as outlined below, I think some minor changes are required before the manuscript can be published.

Specific Comments

1. Comparison with Previous Results

I think there needs to be more discussion comparing these results to those of previous studies. In the conclusions it is stated that these results are consistent with Legras et al. (2003), but there is no discussion of the fact that the current result of D $\sim$ 0.1 m^2/s outside the vortex is inconsistent with the results of Waugh et al. (1997), Balluch and Haynes (1997) and Alisse et al. (2000) which estimate D is around or less than 0.01 m^2/s. This difference is significant, and puzzling as Waugh et al. and Balluch and Haynes analysed similar ER2 measurements, and more discussion is needed. Do the authors have an explanation for the difference?

Does it have anything to do with the fact that the previous analysis of ER2 data examined individual filaments/laminae, rather than all data? The analysis of the individual filament shown in figure 9 suggests that if a single value was used D $\sim$ 0.01 would more appropriate than D $\sim$ 0.1 (D=0.1 produces weaker gradients at edge of filament and much higher N2O within the filament). So, for this filament the current analysis yields a value similar the above mentioned studies.

Although it is probably beyond the scope of this study I think it would useful to apply the same technique to the data analysed by Balluch and Haynes. This would help identify whether the above difference is due to the different methods or to different tracer data.

2. Roughness Function

I find the explanation of the roughness function a bit confusing, and as this is the main diagnostic used I think you need to include a better description (may be in an appendix). The description in Legras et al. (2003) is a little better, but still doesn't show how ϕ(p) varies for curves with differing amount of small scale structure. Also, the definition of ϕ(p) varies between the two papers.

ACPD

4, S3268–S3271, 2004

Interactive Comment

Why are osculating curves in figure 5 shifted (magenta to blue curves) when calculating ϕ, and what determines the magnitude of this shift? Also, what value of p is used in this calculation (do the black curves around 0.4 show parabola for chosen p)?

I think it might help to have a 4-panel figure showing osculating curves (and roughness function) for two different values of p and for different curves (with differing small scale variability). This would illustrate how ϕ varies with p, and how this variation depends on the characteristics of the original curve.

3. Figures.

Several of the figures show too many, very small plots; in particular figs. 3, 6, 7, and 8. The number of panels should be reduced and size of individual plots included. Also, the order of some of plots seems strange.

Fig 3: Do all 8 values of τ shown need to be shown? I'd suggest dropping $\tau = 47$, 72, and 88 days. The similarity between $\tau =$24 and 147 days shows that calculations have converged by around $\tau =$24 days.

Fig 6: It is very hard to see some of the features in these small plots. Also, the order that the reconstructions are presented is confusing, For example, N2O reconstructions for different D are not shown in increasing order, i.e. D= (b) 0.1, (c) 0, (d) 0.01, and (f) 0.001. Furthermore the variance is shown in between N2O reconstructions.

Similar comments about figures 7 and 8. Note also comment below about values of τ shown.

Does the variance need to be shown in figs 6-8? There is only brief mention of this in the text, and I think the variance plots could easily be dropped.

Minor Comments

pg 8288, line 2: I am not sure "circumventing" is the appropriate word to use. Circumventing implies the jet skirts around (avoids) the polar vortex but the intense jet in the

ACPD

4, S3268–S3271, 2004

Interactive Comment

polar stratosphere IS THE stratospheric polar vortex.

pg 8290, l 17: Why is interval $\pm\sqrt{3}$ used for random variable r?

pg 8293, l 21: “Norvegian" should be “Norwegian"

Figs 6-8: Why is a different τ used for each calculation?

pg 8301 l 23-: It might be useful to show PDFs of Lyapunov exponents for inside and outside the vortex to illustrate the differences in this quantity. This would also allow comparison with other papers that have calculated Lyapunov exponents for stratospheric flows.

pg 8304, l 7: Does the fact that the fluctuations are mostly seen in the vertical wind mean that isentropic calculations may be less sensitive to temporal resolution of the winds?

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