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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Turbulent vertical diffusivity in the sub-tropical stratosphere" *by* I. Pisso and B. Legras

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1 Answer to referee 1

1. SF stands for short flight.
2. Our Lagrangian method provides a numerical solution of Eq.(5). A sentence has been added.
3. Correction done.
4. The correlation shown in the lower right panel of Fig. 7 means than fluctuations are of dynamical rather than chemical origin, thus comforting our interpretation. The sentence has been modified.

5. Meridional exchange is favoured at 400K because there is no significant barrier at this altitude between tropics and extra-tropics. This is due to the vertical gap between the top of the subtropical jet at 380K and the bottom of the tropical pipe at 420K. As the campaign occurred during the southern hemisphere summer, the polar vortex cannot have any innfluence but it is plausible that it does dring winter.

6. The parameterization of mixing in CLAMS is certainly a better representation of physical processes than a parameterization based on Richardson number or Smagorinsky formula. Our result show than even then, the properties of diffusion can differ from the prediction of such parameterization. As this is not a central point in the discussion, we prefer to leave the reference to our previous paper where this is discussed with more details with references to CLAMS. See also answer 14 to referee 2.

7. Correction done.

8. Correction done.

9. Figure 8 (now 9) and its caption have been corrected.

10. Caption of Fig. 9 (now 10) has been corrected.

2 Answer to referee 2

1. We agree and have reworded the sentence. Two references have been added to meet a request of referee 4.

2. We agree and have changed the text accordingly using the recipe transport=advection+mixing.

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3. The sentence has been modified.

4. "Natural" has been replaced by "intrinsic effective" to avoid confusion with the molecular viscosity.

5. "Local" has been removed as the estimated diffusivity results in fact from a Lagrangian average. It is local in the sense that it varies from case to case and according to the altitude and latitude range but this should be clear from the rest of the discussion.

6. SF means short flight. The ordering of the flight was the responsability of the campaign coordinator. See the overview article of Pommereau et al. (Atmos. Chem. Phys. Discuss., 7, 2389–2475, 2007) in this issue.

7. There is an equator to pole gradient of ozone at 400K in both hemisphere during summer although it is much weaker than during winter. Figure 6 has been added to support this statement, based on ECMWF analysis and ozone climatology.

8. Figure 4 allows us to determine that the ozone layer of SF2 is a well-defined structure while the ozone layer of SF1 is likely to be more fugitive, that is less easy to reconstruct.

9. Correction done.

10. The random motion is actually applied by subdivising the interval δt into 50 subintervals. This is now explained in the text. Notice that the algorithm is provided for a simple Euler integration scheme. It has to be adapted to other schemes.

11. The diffusive reconstruction is a numerical solution to Eq.(5) which demonstrates the validity of our calculations.

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12. We think it is useful to show the location of the shifted profiles used in Fig.9 with respect to the three dimensional distribution of the reconstructed tracer in order to appreciate the orientation and the intensity of the gradients.

13. The triads are renormalized every day for better accuracy but they are calculated over a longer interval. In practice they stabilize after 3 to 5 days. A sentence has been added.

14. The relation between Lyapunov exponent and diffusion has been discussed with more details in our previous paper (Legras et al., 2005, Atmos. Chem. Phys., 5, 1605–1622) and is a topic of work in progress. Assuming for a relation between Lyapunov exponent and diffusion appears as a natural extension of the mixing length hypothesis or parameterization of diffusion based on wind shear. The CLAMS model (Konopka et al., 2004, J. Geophys. Res., 109, 2315, doi:10.1029/2003JD003792) is based on this assumption. There are however several reasons for which this relation might not always be satisfied. The diffusion can be due to vertical propagation and breaking of gravity waves which cannot be simply related to the local or the Lagrangian value of the shear. Large horizontal or vertical shear does contribute a lot to the Lyapunov exponent but does not necessarily lead to instabilities and contribution to mixing. Actually, a dynamical boundary detected by large Lyapunov exponent and where large tracer gradient is observed is also a region of weak mixing that allows the generation and maintenance of the gradient.

3 Answer to referee 3

1. We have performed an additional calculation with horizontal diffusion replacing the vertical diffusion. It has been found that the horizontal diffusion must be

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$D_H \approx 250\,000$ m^2 s^{-1} is order to match the reconstructed profile obtained with vertical diffusivity $D = 0.5$ m^2 s^{-1}. The large ratio, of the order 500 000 between D_H and D is consistent with the examination of the dispersion of close trajectories. Parcels initially distant of 10 m in the vertical along the SF2 profile are separated by an average distance of 9 km after 30h of backward integration without diffusion. This means a vertical shear 0.008 s^{-1}. Parcels initially separated by 2.5 km in the horizontal hardly separate by more than 5 km over the same period, which provides an upper bound for horizontal deformation or shear as 4.10^{-6} s^{-1}. It thus appears that the purely horizontal dispersion is very weak in the considered case, which is a summer situation, at least based on the ECMWF analysis. We have checked our calculations by using the same code on high latitude winter ozone profiles for which the diffusivity ratio is found to be 60 000, compatible with a vertical to horizontal shear ratio of 250 as estimated by Haynes and Anglade (1997, J. Atmos. Sci., 54(9), 1112–1136) in the lower stratosphere. As 3D turbulence can be broadly considered as isotropic - even if is localized within flat pancake structures - we can certainly neglect the direct contribution of turbulent diffusion in the horizontal direction. We have added a curve to Fig. 8 (previously Fig. 7) and a paragraph in the text.

2. Correction done.

3. See answer 11 to referee 2. ρ has been defined.

4. We use a standard running centered average over 5s which is equivalent to convolute the data with a uniform kernel of length 5s as indicated in the text. The result is weakley sensitive to the exact length of the kernel and we have tried more elaborate kernels without significant impact.

5. Correction done.

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4 Answer to referee 4

1. The sentence has been modified to meet also a request of referee 1 and the two references have been added.

2. We are actually using two profiles, not one, but we agree that it would be useful to extend this study to more cases from other campaigns in the subtropics during other intrusions but there are very few data of this sort which are available. We have made the suggested modifications.

3. We have studied the impact of replacing the vertical diffusion by a horizontal diffusion. See the answer 1 to referee 3. Both vertical and horizontal diffusions match the values found by Hegglin et al. (Geophys. Res. Lett., 32, L13812, doi:10.1029/2005GL022495, 2005) although we believe that it is somewhat coincidental for the horizontal diffusion as the scaling f/N mentioned by the referee is likely to apply in other regions. The estimate of horizontal diffusion by Hegglin et al. includes the mean effect of synoptic perturbations and should thus be much larger, in general, than our subgrid-scale estimate.

4. February is actually during summer in the southern hemisphere but the referee is right in pointing out the inconsistency of our sentence. We agree with his interpretation and the text has been modified accordingly.

5. The slow diabatic motion of the lower stratosphere is descending in the extra tropics and at high latitude, but the fast isentropic motion is rising for a parcel moving from the high latitude to the tropics because the isentropic surfaces are sloping upward to the tropics near 400K with respect to pressure and altitude. It is this fast motion and not the slow motion which dominates over the 9 days of the reconstruction. See also the new Fig.6 and answer 7 to referee 2.

6. Correction done.

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7. The unique occurrence of LMS has been replacedby ETLS. We agree that ETLS is somewhat redondant as pointed out by the referee but it is used here as a self-descriptive notion. Such expression are not uncommon in climate science, such as "incoming solar radiation" or "polar sea ice".

8. Yes, we mean "as shown by climate simulations". The sentence has been modified.

9. Correction done.

10. SF stands for short flight. This was not defined and it has been corrected. DMI is Danish Meteorological Institute. It was defined but obviously it was not clear. We have made a correction.

11. This awkward sentence has been replaced by the suggestion of the referee.

12. to 19: Corrections done.

5 Answer to referee 5

1. The Castillo-Negrete and Morrison paper explains that a jet is an efficient barrier because of the so-called non-twist condition which means that the width of broken tori vanishes near the center of the jet. This is, to our opinion, the right explanation of why jet cores are such efficient transport barriers and we consider this paper as a generic reference to transport barriers associated with jets. A detailed discussion of such matter is, however, outside the scope of this paper.

2. This paper is part of a special issue devoted to Hibiscus and the campaign is fully described in the overview paper by Pommereau et al. in the same issue.

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HIBISCUS is actually an acronym, although the meaning has probably been lost, and should be capitalized. This has been corrected.

3. SF means short flight.

4. See answer 6 to referee 2.

5. See answer 11 to referee 2.

6. The REPROBUS model simulation used in our study has been initialised on 1st April 2002, using ozone field from ECMWF analysis, and climatologies of other chemical compounds. The elapsed time between this initial state and February 2004 ensures that all the transients have passed and that the model state is independent from this initialisation, at least for ozone.

7. Basically, the isentropic surfaces above 350K are sloping down towards the pole with respect to pressure levels or altitude. See the new Fig.6 and also answer 7 to referee 2 and answer 5 to referee 4.

8. It is only the first panel of Fig.9 which is referred before Fig.8 while the other panels need Fig.8 to be geographically located. We do not see a better way to handle this.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 6603, 2007.