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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

Anonymous Referee #2

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Review of the paper:

Turbulent vertical diffusivity in the sub-tropical stratosphere
written by I. Pisso and B. Legras

General: This study complements the investigations in Legras 2005 where turbulent vertical diffusivity D was determined for polar and extratropical stratosphere. In the presented manuscript, Pisso and Legras apply the method of the diffusive reconstruction for 2 tropical balloon profiles in order to estimate the vertical diffusivity in the tropical lower stratosphere. They also discuss the relation between the Lyapunov exponents and mixing. This case study, after addressing a considerable amount of minor points discussed below, can be published by ACP.

Specific comments:

- P. 6605, L. 10-15
  I do not understand what is the intention of the sentence starting with "It should be noted that this overworld barrier....". The tropical pipe extends upwards of $\approx$420 K and separates the tropics from the extratropics. The subtropical jets are below 380 K, so there are no significant transport barriers, with exception of the equator, between 380 and 420 K. You should reformulate this sentence.

- P. 6605, L. 20-25
  "not only transport but also mixing". In my understanding transport means advection + mixing. Advection is the reversible and mixing the irreversible part of transport. Please clarify.

- P. 6605, L. 25-
  "this has to be too large" - you mean "this is too large in the most numerical models due to limited resolution"

- P. 6605, L. 25-
  "the natural value" - even in your paper D mean a mean (effective) diffusivity that still differs from the physical one

- P.6606, L. 5-10
  please explain "local equivalent diffusivity"

- P. 6606, L. 20-25
  What is the meaning of the abbreviation "SF". Why do you denote the first flight SF2 and the second one SF1

- P. 6607, L. 15-20
  Maybe some additional sentences are necessary to explain that extratropical in-

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trusions around 400 K, in particular in summer, have higher ozone values than corresponding air masses in the tropics.

- P. 6608, end of subsection 2.1
  Figure 4 is weak and does not really support the text. You can skip this figure.

- P. 6608, end of subsection 2.1
  "the by Fig. 4" is not correct

- P. 6608, L. 10-15
  What is the numerical value of δt ? Later on you give the value 900 s for the trajectory integration time. I am not sure that this is the same δt. If it is the same value, you should mention it earlier. It is important to know what is the frequency of the applied perturbations.

- P. 6609, discussion of the Green's function
  I think, the diffusive reconstruction does not use the Green's functions. In my understanding, the diffusive reconstruction uses only backward trajectories overlaid with the random perturbations applied every time step δt. If you do not use the Green's function, please do not introduce the equations (3) to (5).

- P. 6614, L. 10-20
  Fig 8 gives no insights into the argumentation. I would recommend to skip this figure. Figure 9 is enough. I would recommend to denote the shifted profiles by using relative coordinates (Δlon or Δlat with respect to the undisturbed profile)

- P. 6615, L. 0-10
  In the description of the calculations of the Lyapunov exponents it is not clear if you calculate these exponents every 24 hours (i.e. deformations along $\Delta t = 24$ hours trajectories are analyzed and this is done every 24 hours). Please clarify.

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- P. 6616, L. 20-25
  "This suggests that D cannot by solely related to local strain"
  I do not understand this conclusion. What I learned from your investigation of the Lyapunov exponents is the fact the highest values of λ define the dynamical boundaries, i.e. region where strongest vertical ozone gradient can be found in pure trajectory calculations (with $D = 0$). That means that vertical diffusion is really necessary at these dynamical boundaries in order to reconstruct the observed ozone profiles which are much smoother than the the profiles reconstructed with $D = 0$. In other words, mixing, i.e. high values of D are "most important" in region with enhanced Lyapunov exponents, so one expect that mixing and enhanced Lyaponov exponents are positively correlated ? This important point should be discussed more carefully.

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