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Interactive comment on "Quantification of mesoscale transport across the boundaries of the free troposphere: a new method and applications to ozone" *by* F. Gheusi et al.

Anonymous Referee #2

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General recommendation

The paper is in general well-written and the method innovative and interesting. However, unless I am seriously mis-interpreting the method used it seems to have a major flaw which cannot be overcome. Thus, in my opinion there seems no option but to reject the paper.

Major points

p8110, Lagrangian technique: It is stated that the 'conservation of the initial co-

ordinates is assumed by definition'. This is certainly true according to equation (1) in which the tracer field only evolves by advection. However, the authors also state that the tracer fields experience not only advection but also the sub-grid turbulent and convective transports through the turbulence and convective parameterizations (p8111,l1). Hence it seems to me that in fact the tracers are not conserved. Consider for example a convective plume. If the tracer is transported from the boundary layer to the upper troposphere via this convective plume where it mixes with the surrounding air then the resulting tracer values will be a weighted average of those of the boundary layer (low for the z tracer for example) and those of the upper troposphere (high for the z tracer). Hence the air can not be identified as having come from the boundary layer via this method. On a related note, all advection schemes have some implicit and/or explicit diffusion - is the advection scheme used to advect the tracers really conservative. So, while the representation of (physical as opposed perhaps to numerical) mixing processes is important for realistic modelling of the evolution of chemical tracers, these same mixing processes seem to mean that this method cannot be used to evolve tracers representing the initial co-ordinates of air parcels.

On a similar note, even assuming that the tracer advection is conservative, errors will occur if the tracer advection scheme is not the same as that used to advect the dynamics in Meso-NH. For example there could be some displacement error between the advected position of a tropopause fold (diagnosed from PV which although often not completely conserved by models is a consequence of the evolution of the wind and thermodynamics in the model) and the location of stratospheric air as diagnosed from the tracers even if no irreversible PV modifying processes have occurred.

Do the above concerns perhaps account for the mismatch between the ozone field and the evolved position of the 4500 m surface in fig 4 ? Although you would not expect an exact match between ozone and this isosurface unless the tropopause laid along this isosurface at the initial time, the obvious horizontal offset is surprising and concerning.

In the footnote on page 8111 the authors make the comparison with the reverse domain

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filling technique. The RDF method of course avoids the difficulties inherent in the method presented in this paper by using advected back trajectories to determine the source location of air parcels. Hence although if mixing has occurred then the inferred source location might be in error, it is at least a possible source location given the evolution of the model dynamics. In the method proposed by these authors the inferred source location could be seriously in error if mixing between air from different source locations has occurred.

Minor points

1) Still many English language errors and some spelling errors.

2) p8104,l7: 'That dispersion appears to be "sponged" by the modelled chemical cycles.' Please elaborate this sentence - I don't understand what you mean here.

3) p8116, l15: How does the model define it's boundary layer - does it use TKE ?

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 8103, 2004.