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Atmospheric Chemistry and Physics Discussions



# *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.

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## 1. General recommendation

This paper presents a methodology that is, in principle, very interesting and could see many applications. Therefore, I would like to see this paper published. However, while the underlying method is certainly worth publication, the presentation quality is poor and the testing of the method's numerical implementation is clearly insufficient. Instead of the case studies presented, in my opinion it would be much more important



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to present a thorough analytical testing of the method. To judge the significance of the results in complex situations such as those presented, the numerical accuracy (and possible numerical problems) must be known beforehand. Therefore, I recommend very significant major revisions before publication. My reasons for this recommendation are given in detail below.

## 2. Major points

I am wondering how well the method works in complex flow situations. For instance, what happens if advection brings closely together air masses from vastly different origins. Imagine the extreme situation that air masses from both ends of the model domain are folded together to the point where the model resolution is insufficient to distinguish between the two origins. I suppose the advection scheme would "mix" the two positions and one would erroneously think that the two air masses have originated from the middle of the domain (averaging of the two original positions) instead of from the two ends of the domain. Such situations are not uncommon in the atmosphere (the complexity of such flows is nicely demonstrated in the results of contour advection). I would expect that the numerical diffusion would likely act to "mix" original positions already long before the air masses are folded together below the grid scale (i.e., even when they should still be resolved, theoretically). Whenever this happens, the method would yield strongly misleading results. Therefore, and as a strict testing of such flows was also not reported in Gheusi and Stein (2002), I would like to see a stringent test of how well the method works under such situations. You could test this by prescribing an analytical convergent flow field for which the solution is known.

Most of the figures are rather small, but the quality of Figure 3b is especially poor. I can't read anything in this figure. What I can see is that the model and the measurements do not fit together very well and I am not sure how well the simulation actually captures the flow situation. In Figures 6-8, the geographical location of the box shown is not clear if one is not very familiar with the topography and the coastlines of Spain and southern France. At the very least, the domain shown in Fig. 6a should be indicated more clearly (for instance, by including country boundaries and coastlines).

The level of the use of the English language must be improved before the paper can be accepted for ACP. As a non-native speaker myself I know how hard it is to achieve a good written English, but ACP standards are high and a paper with improper level of English would also not see the impact it otherwise deserves. The authors may either ask a native English speaker to have a look over the text, or use one of EGU's professional translators to correct the language. Otherwise the text is very difficult to understand in many places. One example is the use of "dispersion" on page 8105, line 6, where the authors obviously mean disagreement or similar. There are just too many instances of improper use of English for me to make suggestions for corrections.

The equations on pages 8119 and 8120 are written in a too sloppy way (the same is true for the equation on page 8115). Please specify precisely the boundaries for the integration. There are 3 integral signs, but only one integrand, please make coherent. Also, it would be more logical to present the equation for air mass first and then the equation for ozone.

For the ozone and mass transferred out of the boundary layer, the time selected just a few hours after the collapse of the boundary layer is unfortunate. The result is rather trivial as it is well known that the boundary layer height over land is much more shallow during nighttime than during daytime. The mass could have been determined approximately simply by the known difference between the typical boundary layer heights. It would be much more interesting to know how much of the air/ozone remains outside of the boundary layer over the next 24 hours when a convective boundary layer develops again over land - even if much of the air seems to be advected towards the Mediterranean in your case.

The calibration of the $\alpha$ factor is very arbitrary. Given the importance of this parameter, the justification for it's value is not good enough. I assume it would depend on the meteorological situation, etc. This brings me back to suggesting a thorough analytical testing of the method.

## 3. Minor points

Page 8105, line 28: It would be convenient for the reader if you could define the spatial scales (in km) of meso-$\alpha$ and meso-$\beta$.

Page 8106, line 19: Trajectory models may lack certain parameterizations, but that they are representative of a certain mass (and not a volume) per parcel is not an assumption but a fact. The mass they are representative for depends only on the resolution of the grid they are started from. Concerning the parameterizations, Lagrangian methods can include physical parameterizations (see James et al. 2003), but an important disadvantage is certainly that they are all off-line and have to diagnose physical processes from the Eulerian model output and must also interpolate in time for the advection step.

Page 8107, line 15: The contour advection approach also suffers from a rather subjective threshold for the surgery.

Page 8112, line 18: stRereographic

Page 8117, lines 25-27: It is unlikely that the ozone was formed at lower levels and then uplifted. Instead, in a turbulent mixed layer, also the precursors (which have lifetimes exceeding the turbulent time scale) are mixed over the entire layer and the ozone is formed in the entire boundary layer.



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## 4. References

James, P., A. Stohl, C. Forster, S. Eckhardt, P. Seibert and A. Frank (2003): A 15-year climatology of stratosphere-troposphere exchange with a Lagrangian particle dispersion model: 1. Methodology and validation. J. Geophys. Res. 108, 8519, doi: 10.1029/2002JD002637.

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

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