Atmos. Chem. Phys. Discuss., 4, S3760–S3767, 2004
www.atmos-chem-phys.org/acpd/4/S3760/
European Geosciences Union

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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Received and published: 2 March 2005

Final author comments on the discussion paper "Quantification of mesoscale transport across the boundaries of the free troposphere: a new method and applications to ozone" by F.Gheusi et al.

General response to both referees ————————————

Clearly the major criticism from both referees does not focus on the main objective of the paper itself – namely the transport quantification method (that basically consists in a objective selection then a mass integration) – but rather on the method to retrieve the source location of air parcels.

The latter method was previously proposed in the QJRMS paper of Gheusi and Stein, 2002 (hereafter GS02). Although it cannot be considered as a scientific validation but as an encouraging indication, it should be first noted that the method was used succesfully in later published papers from the Meso-NH community in a wide range of real case studies at various scales (eg. Chancibault et al. Monthly Weather Review 2003, vol.131, pp.2290-2311 (convective supercell); Mari et al. JGR 2004, vol.109, art. D15S17 (long-range transport of pollution); Flamant et al. QJRMS 2004, vol.130, pp.1275-1303 (mountain gap flow and foehn)).

On one hand, we acknowledge that a validation study on the method still lacks, and would be of great scientific benefit for our method. As preliminary validation test in the case of the tropopause fold (section 3 of the paper), we computed a reconstructed field of ozone (on 10 February 2002 at 12 UTC) on the basis of the source locations 18h earlier (i.e. in the spirit of the RDF technique but with the source locations given by the GS02 method instead of backtrajectories). As ozone is a passive tracer in the model, ideally the modelled and reconstructed fields of ozone should exactly match. On average over about 1,000,000 model grid-points, the two fields differ of only 7.2% in relative value (relative bias 0.17%). An additional run over the considered 18h without convective transport of both ozone and location tracers actually shows very little difference compared to the control run. The authors can provide upon request (please mail ghef@aero.obs-mip.fr) vertical cross sections showing how well the two fields overlap. Given the complexity of the flow and the distance covered in 18h (about 2200km for a parcel in the fold) this result should prove that the GS02 method gives much better results in complex flows than expected by the referees. In addition we give below some fundamental arguments to support the idea that the GS02 method does the job at least as well (or one should rather say, at most as bad) as commonly used Lagrangian (trajectory) methods.

On the other hand, we consider that the validation of the GS02 method is not among the goals of the present paper, focused rather on transport quantification and potential applications. So we eventually suggest that we could submit an independent note on the GS02 method validation together with a revised version of the present paper (but with less extended changes as suggested by A. Stohl).

As the reliability of the GS02 method is by far the most critical point outlined by the reviewers, in the present response we focus on it and do not answer some other more specific comments of lesser importance (figures, English, etc.). These will be specifically answered in a document accompanying the forthcoming revised manuscript.

Role of mixing ————

This point was criticized in particular by both referees. It is actually a frequently asked question, so we have some level of reflexion on that point. Note that this is discussed to some extent in the scientific documentation published two years ago on the Meso-NH www site (see http://www.aero.obs-mip.fr/mesonh/dir_doc/lag_m45_21mai2004/lagrangian_m45.pdf, p.14-17).

We have the feeling that the common concern of both reviewers, that are obviously familiar with Lagrangian methods, arises from the idea that an air-parcel at a given model grid-point originates from a precise source location at an earlier time. Strictly speaking, this is wrong as soon as the considered volume of air (here a grid-cell) is finite.

A finite air-parcel actually results from the mixing of an infinity of air parcels from different locations in the past. In our opinion the most rigourous (but numerically expensive) approach of this problem is that given by Issartel and Baverel (ACP 2003, vol.3, pp.475-486). By means of an adjoint model they compute backward in time the plume "emitted" by a pinpoint receptor. That plume (a 3D "concentration" field) gives the respective weight with which each point of space will contribute to the mixed air-parcel finally observed by the receptor.

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Ideally the source location given by the GS02 method is the barycentre of this plume.

Clearly in the GS02 method the (numerical of physical) mixing of two air-parcels from source locations X1 and X2 will result in an air-parcel originating from an intermediate location, say X12, between X1 and X2. This could appear as a false result but actually it is not.

Since atmospheric processes are continuous in nature, an infinity of air parcels in a continuous range of intermediate source locations between X1 and X2 will also contribute to that mixing. The result X12 is indeed the barycentre of the source locations of all parcels contributing to the mixing. Therefore it is the correct result although there is an uncertainty on it that is all the greater that mixing is strong. In other words, there is a progressive loss of idendity of the air parcels backward in time.

In this sense, the use of single trajectories as deterministic objects may be misleading in some cases. The considered air parcel is supposed to remain the same at each step of a numerically-computed trajectory. Nevertheless a progressive loss of identity along the trajectory path actually exists, due to integration errors, neglection of physical mixing such as turbulence, etc. This is of course well known, and a more refined information is given by a bunch of trajectories randomly perturbed at each integration step. As result one obtains a probability density field of possible source locations, and the radius of the possible source locations increases backward in time and is all the larger that mixing is strong on the path of the trajectories.

This density field is more or less equivalent to Issartel and Baverel's plume, and its barycentre is equivalent to the source location given by the method of GS02.

We see no argument (except perhaps technical ones on the employed numerical schemes) to claim that one of the two techniques – barycentre of a bunch of trajectories, or GS02 – is better than the other. Fundamentally the dispersion of the possible source locations depends on the flow and not on the retrieval technique used.

For instance in the case of the "extreme situation" imagined by A. Stohl (S2966, "Major points", first pragraph), we do not deny that there is a difficulty with the method of GS02. Clearly we would not be in good conditions to use it (and besides to use a limited area model at all). But we also emphasize that it is very likely that a bunch of backtrajectories will also show a dispersion that rapidly (in the past) covers a large part of the model domain. In such a case a single backtrajectory, and the source location retrieved with it, would be of little significance.

Both single-trajectory (as opposed to statistical trajectory methods) and GS02 approaches actually have a common lack when used to retrieve the source location of an air-parcel: a quantity measuring the uncertainty of the result – ie the other possible locations around the result. Of course this uncertainty is all the greater that the parcel has passed in turbulent or convective areas, strongly convergent flows, etc., or that the advection scheme is diffusive. To build such a quantity still belongs to future works regarding the GS02 method and we remain concerned that it is of major importance.

Regarding the opinion of the anonymous referee and the additional comment from A. Stohl that echoes it, we think that it is not completely true, and at least exaggerated, to consider that "the physical parametrizations for mixing (...) will worsen the problem, probably to the point that the method gives entirely wrong results" (A. Stohl). We tried above to demonstrate two points: (i) if mixing is so strong that the GS02 gives entirely wrong result, so does a deterministic trajectory method. (ii) mixing does not worsen the result but simply makes it less certain.

Regarding point (i) the presented case-studies were of course designed to work in satisfactory conditions. Even if it is mentionned neither in GS02 nor in the present paper (probably it should be), we must further emphasize that the GS02 method includes the possibility to refresh periodically the location tracer fields to obtain path fragments that are sufficiently short to avoid excessive mixing. Then a post-processing enables to reconstruct the whole path from the fragments (a detailed explanation can be found from the web link given above). In the case of the tropopause fold, the refresh occurs every 6h. In the case of the PBL plume, hourly.

It must also be said that taking the convective mixing into account made possible, in some recent studies with Meso-NH at large-scale over the tropics, to evidence vertical convective motions otherwise not, or poorly, captured by the model-resolved vertical velocity. Examples of vertical displacement fields over Brazil can be found on-line on http://www.aero.obs-mip.fr/mesonh/troccinox/.

Response to specific major comments from A. Stohl ——————————————————-

Performance of the method of source-location retrieval (GS02) in complex flow situation: The above result and discussion should have answered A. Stohl's concern to a large extent. Yet we acknowledge that testing the GS02 method in the case of an analytic flow, and against trajectories, would be a strong gain in reliability, so we propose to write a specific note on it.

Regarding the misfit between the ozone curves of Figure 3b, the underestimation by the model in the troposphere is due to a known bias in the chemical analysis that we used to initialize our model (ozone is then a passive tracer in this simulation). However the only structure we are interested in in this study, is the tropopause fold (1) which is modelled at the right place at the right moment. Given the quite rapid eastward motion of the trough in this situation, it is an indication that the model pretty well captures the flow.

Mass transfer from the boundary layer: The diurnal "breath" of the boundary layer over land of course well known. The interesting thing here is that the developed PBL over land is in the chosen case especially deep due to the convergence of the sea- and mountain-breezes with the synoptic flow. Then this ozone-rich air is released over sea where the boundary layer does not develop such deeply the day after, so that the ozone experiences medium-range transport up to the area of Marseille where it mixes to local pollution. This could perhaps be shown in the revised paper.

Response to specific major comments from anonymous referee 2

The anonymous referee is not mis-interpreting the method. We already have to some extent answered the question of mixing. However we disagree with some of his arguments.

First we have the feeling that the referee somewhat mistakes advection and conservation. Whereas it is perfectly true that advection alone ensures conservation in a continuous-space representation (eq. 1 in the manuscript), it is no longer true as soon as space is discretized. In this case the tendencies from the parametrizations (turbulence and convection) must be included in the conservation equation – even if their form is not conservative – because they represent sub-grid transport that is conservative in nature. There is no fundamental reason that what is true for a chemical passive tracer is untrue for another passive tracers.

The general interpretation of mixing for the location tracers was already discussed above. However let us give a specific comment on the example given by the referee (convective plume). It is perfectly true that the air contained in a model grid cell is a mix between PBL air and air from levels above (and hence the result z0 will be a weighted average). Consequently identifying the air of this grid-cell as originating from the PBL, as expected from the referee, is false since the air actually originates from a certain range of altitudes. In practice the contribution of PBL air through the parametrization will lower the resulting z0. This means that the air seems to come from a lower level than expected and thus ascendent motion can be diagnosed even if the resolved vertical velocity is zero.

The same advection scheme was used for the dynamical fields and passive tracer fields in our simulations.

Regarding the mismatch between ozone and the z0=4500m contour in Fig.4, the offset is in most part due to the fact that isopleth of ozone do not concide with iso-z surfaces

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at the initial time – as noted by the referee. The vertical cross-section of passive ozone vs. reconstructed ozone shows a good coincidence (an offset of few kilometres still remains but this should to be compared to a distance of 2200km traveled by the air-parcels).

Regarding the RDF technique, we warned above on the use of single trajectories in case of mixing. If a field, say PV, is reconstructed not from a single trajectory (giving a single possible source location and a single value of PV) but from a bunch of stochastically perturbed trajectories (giving an ensemble of possible source locations over which an averaged value of PV can be calculated), it is likely that the obtained reconstructed PV field will be smoother. In this sense we have the feeling that the classical (single trajectories) RDF method is under-diffusive and probably as wrong as using the GS02 location tracers.

Minor point (3): We used TKE=0.2 m2/s2 as upper boundary of the PBL specifically in our study. In the model itself, a unique parametrization of turbulence is used at all levels so distinguishing the PBL from the free tropopshere makes little sense.

On behalf of the authors, François Gheusi

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