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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Probing ETEX-II data set with inverse modelling" *by* M. Krysta et al.

**M. Krysta et al.**

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**Answer to referee #1**

We would like to thank referee #1 for their review and for pointing us some possible flaws in our reasoning. Some of the issues raised by referee #1 are similar to those addressed to us by referee #3. Hence, the ideas present in both responses are similar.

1. Issues raised by referee#1 (loosely quoting):
*It is not completely clear, what could be the proper extent of generalization of the conclusions from the ETEX-I modelling effort? There could have been some subgrid-scale processes acting in ETEX-II that led tracer plume to diverge; factors absent in ETEX-II could have contributed to ETEX-I simulation success.*

*A good agreement between the modelling results and the experimental data does not constitute a sufficient condition for qualifying the model as good - it may rather be*



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*adequate for a certain set of classes of particular physical situations.*

*Main conclusion rests on the aforementioned assumption.*

Authors' answer:
We agree with the fact that a given class of physical phenomena requires a particular model configuration. Such a configuration may not necessarily be adapted to a simulation of a physical process which is pertinent to another class of problems. Therefore, similarity (or not) of the circulation for both ETEX releases is an important ingredient of the reasoning presented in the paper.

The meteorological situation over Western Europe for the two experiments is similar with respect to the main feature which structures the atmospheric circulation - a low pressure system located over the British Isles.

At the release site, before the passage of a cold front, the surface layer is well mixed-up in the first part of the release and stably stratified later on, according to the figures in (Gryning et al., 1998). This statement refers to both releases, the height of the mixed-layer being smaller in the latter case. In contrast to the first release, the wind direction in the second one is roughly constant, and horizontal wind speed, although decreasing, remains greater than $5\,\mathrm{m\,s^{-1}}$ . Briefly, at the release site, and up to the time of the passage of the cold front, the meteorological situation seems to be fostering a correct model-measurement comparison, even more for the second release than for the first one.

While moving away from the release site, one would expect that the main source of modelling error is due to uncertainties in the advection which would result from meteorological fields of insufficient quality or/and temporal resolution. However, according to (Stohl and Koffi, 1998), where model trajectories based on ECMWF analysed wind fields have been compared to the balloon tracks, "The agreement between the calculated trajectories and the balloon tracks was very good for the first experiment (...),



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and excellent (...) for the second one." And also " ... small errors also indicate that the ECMWF fields of the horizontal wind were of exceptionally good quality in the second experiment". Further on, "Since the horizontal winds are balanced by the vertical winds, this also gives an indication that the grid scale vertical winds were not too bad."

In view of the very good quality of wind fields, the most important contribution to model error seems to be a possible uplift of the tracer due to convective motions accompanying the cold front. Indeed, less than an hour before the end of the release, a cold front crossed the release site. During the two hours preceding the passage of the front, according to (Gryning et al., 1998), "an uninterrupted vertical wind in the layer between the ground and up to 300m (the maximum range of the SODAR during this period) is measured". (However, at a different stage of the release interval, such characteristics were also present for the first release.) As a result, some of the tracer could have been uplifted. Moreover, the front must have crossed the tracer plume before crossing the release site. According to (Stohl and Koffi, 1998), "It is more likely that unresolved small-scale vertical winds in the vicinity of the cold front have lifted the perfluorocarbon tracer into the free troposphere". Elsewhere, "Since the perfluorocarbon plume travelled almost along this front, there was enough time for most of the tracer material to be lifted into the free troposphere by organized rising motion ahead of the front or by convective processes behind the front. These small scale flow features were not resolved by the ECMWF data".

However, indications against such a phenomenon can also be found. Firstly, the front at a release site seems not to be accompanied by a change in heat flux, see Fig.12 in (Gryning et al., 1998), at least not before the following day which reflects typical diurnal/nocturnal variability of the heat flux and is less pronounced than for the first release, Fig.6 in (Gryning et al., 1998). Secondly, a more general statement can be found in (Ryall and Maryon, 1998), "With the approach and passage of a cold front there is the possibility of either frontal ascent or convective updraughts contributing to the removal of the tracer from the boundary layer. The observations of mainly light



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rain and drizzle in the general area do not support the presence of vigorous convective cloud, but the dynamics were indeed vigorous, and it seems likely that, at least in part, frontal uplift may have accounted for the dilution."

To our opinion this frontal uplift is, of course, possible but unlikely. Normally, when a difficult atmospheric situation occurs (like a frontal passage), the results from a large number of models would be very different and most likely the many model results would cover most of the possible solution. In the ETEX-II case the results from the various models were surprisingly similar. To our opinion, it is unlikely that for a very large number of atmospheric tracer models all of them agree on an overestimation of around a factor of ten. Furthermore, while modelling the atmosphere over long periods of time (decades) and validating a Chemistry Transport Model against all possible measurements (as e.g. the EMEP data in Europe), a large number of frontal passages are present in the simulations. This implies that the frontal uplift situation should influence the model results frequently and that very often the model results should be biased by a factor of up to ten. This is, however, not the case.

2. Issues raised by referee#1 (loosely quoting):
*Not clear what is the advantage of the inverse modelling. Not clear to the reader what particular gains are achieved by using* $\boldsymbol{\sigma} = \boldsymbol{H}^{-1}\boldsymbol{\mu}$ *[notation as in the manuscript]. If* $\boldsymbol{H}$ *is inadequate then* $\boldsymbol{H}^{-1}$ *would be inadequate too. Does the inversion circumvent this difficulty? Explore the fact that coarsening the model grid results in an increase of the reconstructed mass, while refining the grid causes the model to break* $\Rightarrow$ *contrast with the ETEX-I results.*

Authors' answer:
We agree that the model flaws are fully present in the inverse modelling methodology. The point is, however, that inverse modelling, as presented here, does more than a



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model alone. The observation equation is

$$\boldsymbol{\mu} = \boldsymbol{H}\boldsymbol{\sigma} + \boldsymbol{\varepsilon}\,,$$

where $\boldsymbol{\varepsilon}$ accounts for errors of any nature (model, observational,representativity). And the solution (even in a simple case of errors modelled with a Gaussian distribution) is *more* than $\boldsymbol{H}^{-1}\boldsymbol{\mu}$. It is

$$\boldsymbol{\sigma} = \boldsymbol{B}\boldsymbol{H}^T(\boldsymbol{R} + \boldsymbol{H}\boldsymbol{B}\boldsymbol{H}^T)^{-1}\boldsymbol{\mu}\,,$$

where $\boldsymbol{R}$ is the observation error covariance matrix, $\boldsymbol{R} = \mathrm{E}(\boldsymbol{\varepsilon}\boldsymbol{\varepsilon}^T)$, and $\boldsymbol{B}$ the background error covariance matrix, $\boldsymbol{B} = \mathrm{E}(\boldsymbol{\sigma}\boldsymbol{\sigma}^T)$. In essence, our inverse modelling approach mimics data assimilation: the source $\boldsymbol{\sigma}$ is estimated along with errors $\boldsymbol{\varepsilon}$. In particular, this allows to partly correct the errors present in the observations and the model.

In the manuscript, the errors are modelled with a Gaussian distribution and are independent one from another. Hence, the retrieval is limited to local uncorrelated errors of a reasonable magnitude. It is thus adapted to tackle rising motions, provided they are localised and each of them affects at most a few measurements. In contrast, a large-scale motion that is not accounted for by the model, for example a frontal uplift at the release point, would presumably evade the inverse modelling analysis. There is obviously some room for improvement here that might come from a finer (non-Gaussian) error modelling.

Briefly, inverse modelling does more than a dispersion model alone, since the errors are modelled too. Should the true errors be independent one from another (like supposed in the inversion procedure) then inverse modelling would bring some improvement to the consistency of the measurements. Since it is not the case, there is an indication of the presence of large scale correlated errors. In view of the tracer quantity that is missing in the measurements, these errors cannot be ascribed solely to the frontal passage at the release site and leave the question of the quality of the measurements open.



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Please note, that the link between grid resolution and inversion results has been thoroughly analysed in (Bocquet, 2005).

3. Issues raised by referee#1 (loosely quoting):
*Little support to the conclusion. The conclusion would be easier to accept if the authors (1) brought substantially more attention to the meteorological conditions, especially to the factors that might have given rise to diverging results - keeping in mind the chaotic nature of the transport phenomena; (2) explained, what is the particular advantage of the inverse modelling for supporting the conclusions.*

Authors' answer:
An appropriate description of the ETEX-II meteorological situation, as presented in this note, has been added to the manuscript.

The issue of the added value of inverse modelling in data interpretation has already been raised in the last section of the manuscript. In the revised version the discussion has been extended to inverse modelling capacity to deal with uncorrelated/correlated errors and forms a separate section now. In particular, the final conclusion has been based on the inconsistency of the hypothesis of independent errors, i.e. the local nature of venting motions blowing the tracer up in the atmosphere versus lack of improvement of measurement consistency in the inverse modelling procedure based on the hypothesis of uncorrelated errors.

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