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Interactive comment on "Rebuilding sources of linear tracers after atmospheric concentration measurements" by J.-P. Issartel

J.-P. Issartel

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We thank the referees for their detailed reading of our paper, for their kind and accurate comments. Both displayed an encouraging curiosity with respect to our chapters 4 and 5 that should be clarified and made more visible in the revised version of the paper.We were rather surprised that the anonymous referee 1 paid so much attention to our evaluation of the potential of the inversion, just as if this conclusion of our submitted paper was more general ('as it focuses on our poor ability to localise and quantify sources'). On the contrary Alexander Baklanov, referee 2, compares the statistical approach as 'cheaper and more relevant' than a deterministic one for such long term emissions as CO, CO2, CH4. These remarks urged us to compare our proposals for inversion with the currently used statistical techniques. Our conclusion is that our method can be inserted in the statitical approach with no difficulty at all. This comparison is the first and longest part of this response. The end is devoted to

inverse transport questions raised by Alexander Baklanov and to some technical faults mentioned by the referee 1.

As stressed by both referees, the only 'new' thing in our inversion strategy is the smoothing technique, with an illumination function, used to avoid that an excessive importance be paid, when interpreting the concentration measurements in terms of an estimated source flux, to the close environment of the detectors. For lack of smoothing the estimated source will display artefacts that are perfectly foreseeable, not random at all. Accordingly, these artefacts should rather be termed a bias of interpretation.

Our paper is primarily about the choice of the base functions to be used for interpreting the measurements. The statistical approach describes how to use, not to choose, such functions. We propose quantitative arguments, based on the concept of 'illumination', that are fully compatible with the statistical approach for a similar numerical price.

Apart from the illumination and positivity constraint, our inversion is a rather standard one. As usual, an unknown source flux σ is investigated by means of concentration measurements $\mu_1, ..., \mu_n$. The μ_i 's are transformed into the coefficients $\lambda_1, ..., \lambda_n$ of a source $\sigma_{\parallel} = \sum_{i=1}^n \lambda_i b_i$ evaluated as a linear combination of some prescribed base functions $b_1, ..., b_n$. To obtain the λ_i 's one makes the adjoint calculation of the sensitivity function (our retroplumes r_i) for each measurement described by the position and date of the receptor (our sampling functions π_i). Then, to represent the measurement operator, a matrix H of elements $h_{i,j} = (r_i, b_j)$ (i.e. $\int r_i b_j d\vec{x} dt$ or rather $\int \rho r_i b_j d\vec{x} dt$) is calculated and inverted: $\vec{\lambda} = H^{-1}\vec{\mu}$.

The statistical approach (Tarantola, 1987, Rödenbeck et al., ACPD, 19 may 2003) follows these general features. It is generally aimed at estimating the mismatch (background error) $\sigma = \sigma_{real} - \sigma_{pri}$ between an unknown real source and an a priori estimate out of the mismatch $\vec{\mu} = \vec{\mu}_{real}(\sigma_{real}) - \vec{\mu}_{mod}(\sigma_{pri}) = \vec{\mu}_{mod}(\sigma) + (\vec{\mu}_{real}(\sigma_{real}) - \vec{\mu}_{mod}(\sigma_{real}))$ between the measurements really observed and

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the values modelled through the r_i 's for σ_{pri} . The difference $\vec{\mu}_{real}(\sigma_{real}) - \vec{\mu}_{mod}(\sigma_{real})$ gathers measurement and model errors described by a covariance matrix $R = R_{meas} + R_{mod}$. On another hand, due to the quality of the a priori estimate, the residual source estimated as $\sigma_{\parallel} = \sum_{i=1}^{n} \lambda_i b_i$ is expected to be statistically zero, with some covariance matrix $B = [\lambda_i \lambda_j]$. The inversion goes through a gain matrix: $\vec{\lambda} = (B^{-1} + {}^tHR^{-1}H)^{-1} {}^tH R^{-1}\vec{\mu}$. This may be rewritten $\vec{\lambda} = H^{-1}\vec{\mu}_{opt}$ with $\vec{\mu}_{opt}$ defined to be the most probable value for the measurements after the observations have been combined with the statistical constraints of the source. The inversion as such is not statistical so that its relevance could always be studied easily by at least controlling that a plausible artificial source is correctly retrieved based on artificial measurements with no noise at all. The response of the inversion to measurement errors could also be evaluated directly. I personnally think that, when studying such sources as CO, CO2, the best help to inversion techniques against measurement and model errors is the redundancy put in the measurements by the slow space and time variation of the source.

It is now easier to describe our paper in this respect. We supposed no a priori knowledge of the sought source ETEX1 that could compensate the errors in the μ_i 's so that, with $B^{-1} = 0$, the inversion reduces to: $\vec{\lambda} = H^{-1}\vec{\mu}$. Accordingly the errors in the measurements will be filtered only by regularising the inversion of H so that: $\vec{\lambda} = H^{inv}\vec{\mu}$. The effect of measurement and model errors was investigated directly.

We first used synthetic measurements and found that the smoothed inversion, either algebraic or positive, was rather stable under a relative measurement error of 30%, twice as large as the 15% announced by the ETEX team.

We secondly used the real measurements, and, as the inversion was seriously altered, we concluded that model errors prevailed and were bigger than 30% of the signal. This of course may be due to our model. It seems anyway that a statistical approach is indeed necessary to make up for a noise that can be expected to be bigger in

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practice than 30% of the signal. We claim that this statistical approach is completely compatible with the choice of base functions, the r'_i 's, that we recommend.

As announced the main point of our strategy is that the base functions b_i 's are derived from the sensitivity functions r_i 's. The availability of the r_i 's represents no additional cost as they are required in other respect. Instead of calculating and inverting one unsymmetric matrix $H = [(r_i, b_j)]$ we calculate and invert for the same cost two symmetric matrices $H = [(r_i, r_j)]$ and $H' = [(r_i, r'_j)] = [(r'_i, r'_j)']$. Finally the source is estimated as a combination of smoothed r'_i 's calculated with an imperfect inverse model rather than as a combination of arbitrarily chosen b_i 's.

The notion of illumination is more general than the presentation made in the ACPD paper. It could always be used to optimize the choice of the base functions in the non statistical part, $\vec{\lambda} = H^{-1}\vec{\mu}_{opt}$, of the inversion. Suppose we handle some base function b_i chosen independently of the r_i 's. The b_i 's can always be linearly rearranged in such a way that H is the identity matrix. Then, if $\sigma(\vec{x},t)$ is some source of tracer associated to measurements $\mu_i = \int \rho \sigma r_i(\vec{x}, t) d\vec{x} dt$ the inversion will return an estimate $\sigma_{\parallel} = \sum_{i=1}^{n} \mu_i b_i$. This inversion amounts to considering that the measurement $\mu_i = \int \rho \sigma_{\parallel} r_i(\vec{x}, t) d\vec{x} dt = \int \rho \mu_i b_i(\vec{x}, t) r_i(\vec{x}, t) d\vec{x} dt$ is due to the point (\vec{x}, t) for a weighted fraction $r_i(\vec{x},t)b_i(\vec{x},t)$. Then, generalizing ACPD formula 18, we may regard the quantity $E^b(\vec{x},t) = {}^t \vec{r}(\vec{x},t) \vec{b}(\vec{x},t)$ as the total weight attributed to a point (\vec{x}, t) by the inversion of H. We think that, in order to have a good interpretation of the measurements, this effective illumination should be as homogeneous as possible otherwise aberrations are expected. For instance, if $E^b(\vec{x}_0, t_0) = 0$ then one can see that the values at (\vec{x}_0, t_0) of the real and estimated source, $\sigma(\vec{x}_0, t_0)$ and $\sigma_{\parallel}(\vec{x}_0, t_0)$ are totally independent of one another. Of course we can decide to use an inhomogeneous effective illumination in order to attenuate the importance of regions that are so far from the detectors that the transport processes cannot be modelled reasonnably well. The inversion will be based on the influence of the best illuminated regions, and this

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influence will be propagated to less privileged regions by the shape of the b_i 's. The b_i 's should be chosen carefully to obtain a relevant propagation of the information.

Any inversion, with arbitrary b_i 's, can be formally put in the non smoothed shape of our paper by substituting for the r_j 's their projections r_j^b 's on the b_i 's. The r_j^b 's are linear combinations of the b_i 's in such a way that $(b_i, r_j) = (b_i, r_j^b)$. Nothing is changed in the calculation and use of H or H^{inv} . Exactly in the same way as the r_i 's are the retroplumes associated to the sampling functions π_i 's, the new functions r_i^b 's may be regarded as the retroplumes associated to effective sampling functions $\pi_i^b = -\frac{\partial r_i^b}{\partial t} - \vec{v} \cdot \vec{\nabla} r_i^b + \zeta(r_i^b)$. Hence, using b_i 's linearly independent of the r_i 's amounts to playing with the definition of the samples. Again this may be justified if we consider that a sample taken at some given place and date is representative of a wider area.

We think that the above ideas may be used to discuss the efficiency of a monitoring network and the adequacy of an inversion strategy. The following arguments are based on a significant discussion about inversion techniques in the aforementioned paper by Rödenbeck et al. devoted to the evolution of the global source of CO2 between years 1982-2000. These authors renounced using data from stations not operated during the whole period in order to avoid that variations in the geometry of the monitoring network might have any influence on their inversion. This influence could be described and controlled in terms of the variation of the effective illumination. It could also be reduced by smoothing the base functions so that the effective illumination would be maintained as homogeneous as possible independently of any evolution in the geometry of the monitoring network. Also, we think it could be interesting to investigate their supposed inversion artefacts, year 1989 in Africa, in terms of effective illumination E^b or effective sampling functions π_i^b 's. In particular, an anomaly of the atmospheric transport that year would touch E^b thus leading to inversion artefacts.

We now come to the discussion of Alexander Baklanov about inverse transport S1197

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and 'retroplumes'. We did not really understand why the referee considers this interpretation is not correct. The idea of 'retroplumes' was proposed in (Hourdin and Issartel, GRL, August 2000). The retroplume represents the concentration of the air sampled for a measurement in the background, before the air was sampled. This definition just states that the air was in existence before being sampled. The concentration of the retroplume obeys a retrograde transport equation which was shown in (Issartel and Baverel, ACP, 19 may 2003) to coincide with an adjoint equation provided some technical precautions are taken. The interpretation of linear adjoint transport as an inverse transport is usual in the theory of nuclear reactors. The following quotations were found in the corresponding litterature:

Paul Reuss, Eléments de Neutronique, CEA editor, 1986: 'Physiquement, on déduit H^+ (the adjoint operator) de H en inversant le sens de l'histoire des neutrons...' (page 126)

Iván Lux, László Koblinger, Monte Carlo particle transport methods: neutron and photon calculations, CRC Press, 1991: 'Based on the similarity between the collision density and value (i.e. adjoint) equations one can imagine that the latter equations also describe collision densities for some kind of imaginary particles: pseudo-particles. These curious particles start their history from the receptor, since the source term in equations 4-99 and 4-100 are the pay-off functions of the original physical problem.' (page 129)

In fact, Jeffery Lewins (Importance, the adjoint function, 1965) argues that the inverse interpretation of the adjoint function was introduced in reactor physics by E.P. Wigner (Effects of small perturbations on pile periods, 1945), H. Soodak (The science and engineering of nuclear power, 1948), H.J. Hurwitz (A note on the theory of danger coefficient, KAPL-48, 1948).

It seems that our sentence on page 3182, lines 20, 21, has raised some confusion: 'We recall that the linear decay processes should be considered a transport term

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in the l.h.s. of equation 3 rather than a negative contribution to the r.h.s. source σ .' We do agree with Dr. Baklanov that 'decay or transformation processes should be considered separately in the equations, but not in the source term.' We suggest to use his formulation to replace ours because this is exactly what we mean.

Alexander Baklanov raises an interesting discussion about time symmetric turbulent motions and isentropic turbulence. We would be considerably interested in investigating this point if the referee would be so kind as to indicate us some pedagogical reference. The turbulent motions are not necessarily time symmetric as signaled in our paper with the case of convection page 3176, lines 11-16. We tend to use the word 'diffusion' to mean time-symmetric turbulence so that 'convection' with rapid concentrated updrafts and slower sinking is not a case of 'diffusion'. We do not think that this use of the words is standard, perhaps it would be enough to mention it in the text. Is there some standard wording? Nevertheless the atmosphere as a whole is, globally and locally, an isentropic system with slight daily and yearly rythms. Accordingly, atmospheric turbulence, no matter the motions are time-symmetric or not, is essentially isentropic, isn't it? The atmosphere creates entropy but does not store it and the full price of the atmospheric activity is paid by the radiation.

The technical faults n° 4 and 5 mentioned by the referee 1 require the following explanations.

Technical fault n° 4: In order to simulate a measurement noise in our sets of 48 or 130 synthetic measurements we used two sets of independent random variables α_i , i=1 to 48 or i=1 to 130. The α_i 's were drawn from a computer embedded gaussian law of average value 0 and standard deviation 0.3. The noiseless synthetic μ_i^s was transformed into a non-negative noisy measurement $\mu_i^n = \max(0, \mu_i^s(1 + \alpha_i))$. More exactly each α_i was built by means of two random variables β_{2i} and β_{2i+1} equally

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distributed between 0 and 1: $\alpha_i = 0.3 \cos(2\pi\beta_{2i})\sqrt{-2\log\beta_{2i+1}}$. The β_i 's were obtained from a system supplied law having the announced statistical properties. The system works with a 'random seed' which means that, if we give the same seed, then we obtain the same random sequence $\beta_1, \beta_2, \beta_3, \beta_4, \dots$; if the seed is changed, then another independent sequence is drawn.

In order to investigate the stability of the inversion with respect to the noise the calculations should have been repeated with several seeds. This has been neglected. Firstly we considered that, as our random sequence $\alpha_1, \alpha_2, ...$ was not privileged, another random sequence, produced with another seed, would compare qualitatively the same way with the noiseless inversion. Secondly the figures would have been bigger and more difficult to explain.

Technical fault n° 5: The sources obtained from the inversion are ground or sea level fluxes $\sigma(\vec{x}, t)$ varying with the 2D position \vec{x} and time t. We did not report the time variations of our solutions, just for the sake of the simplicity. We can say here that the time behaviour of the inversion is just as good, or bad, as the space behaviour. In the figures we reported only the total contribution of each position \vec{x} . To this end we ran a summation over the period from 15 to 27 October 1994. This is what is meant by the formula $\Sigma(\vec{x}) = \int \sigma(\vec{x}, t) dt$.

The referee 1 also recommended a thorough descriptive revision of sections 4 and 5. Is it possible to help me a bit more: should I give more accurate mathematical definitions, physical interpretations, should I change totally the angle? Does he mean that the length of these sections should be increased in the revised version?

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Interactive comment on Atmos. Chem. Phys. Discuss., 3, 3171, 2003.