Atmos. Chem. Phys. Discuss., 4, S1420–S1425, 2004
www.atmos-chem-phys.org/acpd/4/S1420/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Emergence of a linear tracer source from air concentration measurements" *by* J.-P. Issartel

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Received and published: 31 July 2004

The present final reply is intended to complete and clarify the explanations previously given to the referees whom I thank again for their very active involvement into the improvement of the paper.

1) The many symbols in the bottom of the right parts of the figure 1 indicate zero or little valued measurements.

2) The comparison of the technique proposed in the paper with other ones previously advocated is a point raised by both referees. The following points may be noted: a) The reconstruction of ETEX1 is adressed here simultaneously in space and time without the simplifying hypotheses that the date or the position is known. In particular this includes a complete calculation of the background covariance matrix with all space and time correlations. b) The potential of the method is evaluated by means of synthetic mea-

surements obtained for a known distribution of the source. Hence measurement and model noises are cancelled. This strategy might be applied to any inversion method in order to evaluate its potential in these optimal and controlable conditions. c) I tried to show that an inversion strategy defined too rapidly could display an unacceptable dependence on the model resolution. As will be more clearly explained and illustrated in the revised paper this is the case of the non renormalised inversions despite a good aspect on the figure 2a. The non renormalised inversion systematically undersetimates the releases even, as asked by C. Rödenbeck, if the source is spread like a Gaussian law. For this reason I insisted to describe the covariance matrix **B** as well in terms of a continuous kernel $b(\vec{x}, \vec{y})$ the definition of which is model free.

3) The paper is as well intended to introduce concepts and ideas to better handle good and false obviousness. This is not easy and my justifications have perhaps insufficiently been taken care of. Nevertheless the theory often meets the common intuition. a) For instance it is clear that a set of measurements will provide a better description of its close environment, and will be weakly relevant for remote regions; the finite nature of the known domain is just a consequence in my framework. b) The figures 3 and 4 are not totally surprising and, anyway, they could be used as a representation of the background covariance matrix independently of the framework proposed here. c) It is clear as well that a limited number of measurements necessarily result into a fuzzy vision of things, a situation already recognised by quantum statistics. d) In the paper the quality of an inversion, as aked by S. Houweling, is implicitely defined in terms of artefacts related to the idea of hidden hypotheses. This corresponds to the common idea that if inappropriate hypotheses are associated to good measurements the interpretation will be bad (for instance a non renormalised inversion contains the hidden hypothesis that the source lies inside the detectors). I shall try to reach a more explicit definition in the revised paper.

4) The eclairement is a theoretical formalisation of the natural idea that some regions are well and others poorly seen. My opinion is that its validity is not limited to my

theoretical framework, but that any inversion method should give bad results in poorly illuminated regions. Accordingly the quality of the estimation is not the same everywhere. This is still easily acceptable. The consequence is nevertheless that a kilogram of tracer estimatedly released here is not equivalent to a kilogram released there so that it is not relevant to calculate such a total as $\int_{\Omega\times\mathrm{T}} \rho\ \sigma_{\|f}(\vec{x})\ d\vec{x}\ dt$, and this is troublesome. The consideration of which physical quantities are 'allowed' and which are not is no longer intuitive but similar problems were recognised in quantum theory. The positive and negative totals given on the figures 2, 5, 6 should in fact be handled with care. Such totals might be summed up only in regions sufficiently well illuminated, the clear meaning of this 'sufficienlty well' being a question for future investigations.

5) In the paper I supposed that, after the renormalisation, the illumination would become bounded and non singular in the neighbourhood of Dirac detectors (page 2651, line 14). I said the contrary in the point 7) of my response to C. Rödenbeck: close to a Dirac detector, the illumination would diverge like r_i^2. But this is impossible because the illumination must be square summable. Both hypotheses (boundedness and divergence like r_i^2) are certainly wrong because they would be transformed one into the other by the iterative construction of the optimal renormalising function f. So, there must be some intermediate situation with a square summable singularity to be further investigated. I would also like to stress that the renormalisation function used in the discussed paper has nothing more to do with $\max(E, \frac{E_{max}}{1000})$ used in a previous paper.

6) In my response to C. Rödenbeck I noted that a source estimated by the usual theory would produce measurements slightly different from the observations due to some correction process; my estimate, devoided of such correction, would produce the same measurements as observed. This is not exactly true. My matrices **H**, $\mathbf{H}_f$ are generally well conditioned. Anyway to avoid an amplification of the numerical errors I impose a conditioning limit of 90 to the inversions. The result is that if two measurements have been preformed too close to one another they will be considered one single measurement with some averaged value and reduced noise. In other words this is an implicit

ACPD

4, S1420–S1425, 2004

hypothesis that the tracer concentration (and source) field may not display brutal variations. When such variations are observed in the measurements, they are corrected. In the classical framework this role is played by the a priori covariance matrix which, for instance in (Roedenbeck et al, 2003) would ensure some spatial uniformity of the estimate. This remark strengthens my opinion that my framework just corresponds to a logic complementary of the classical theory. Then we should perhaps consider more distinctly that the background covariance matrix plays a double role a) a set of a priori hypotheses about the sought source b) additional fuzziness limitations due to the scarcity of the available informations. This distinction resembles the usual distinction between a priori and a posteriori background covariance matrices. My theory would then lead to the complete calculation of an a posteriori covariance matrix based on an a priori matrix gathering all our reasonable hypotheses about the source.

7) I agree that my tentative comparison with the usual framework by means of Gaussian base function was too rapid. It was based furthermore on a misunderstanding of the choice of the base functions in rhgh03 mistaken from the off diagonal terms of the background covariance matrix having an exponential decay for distances of 1275, 1912 or 6375 km. I think now that an extensive comparison of my theory with the usual one should be considered another work.

8) The evocation of quantum mechanics in the paper was not really intended to give a further insight into assimilation. I suppose my algorithm and arguments may be understood and handled independently if properly explained. I considered nevertheless that my paper contains some strange ideas and frustrating results so that the existence of a formal analogy with an existing and validated theory would be reassuring. Indeed I do not find exactly the position, date and intensity of the ETEX1 release. Worse: I argue that, with the measurements I selected, it is not possible to do better, except for introducing artefacts. This is frustrating but not surprising. We all know that we have to add in our inversions many constraints and conditions from indirect observations (and this additional knowledge is not an artefact). I was nevertheless surprised by

ACPD

4, S1420–S1425, 2004

Interactive Comment

an unexpectedly good result when the quantum interpretation led me to evaluate the distribution of energy associated to the estimates. The corresponding localisation of ETEX1 is very good already for the algebraic estimate and even better if we know the source is positive. This means that by just considering the value of the measurements, the algorithm 'understood' the source was a point. Nevertheless this information is not returned in terms of the sought source but of the energy of the sought source which I then defined as an informational energy. Thus the energy seems to be a part of the language of inversions, coming from the use of scalar products and least squares. If so, we ought to know it. This could give a new insight for appropriately designing monitoring networks. I noticed the measurement noises appeared on the figures 5 f, h or 6 f, h in the form of energy puffs around the detectors. These puffs are so narrow that it seems possible to filter them out thus identifying which part of the signal is noise.

9) Information sciences have quite a long history in meteorology so that the comparison with other fields is in the nature of the things. I am just trying to propose a consistent entry to this approach.

10) I agree nevertheless with the referees that the presentation of the text should be simplified with a focus on the operational aspects. I appreciate all the more their help in this respect as I foresee an increasing degree of abstraction for the development of the present work and the very difficulty is exactly to make this acceptable for our colleagues. In particular the comparison with the usual framework should be based primarily on the logical bases. The mere comparison of numerical results would not be sufficiently instructive at this stage, in particular because my point is not to say what is good or bad but to understand what happens with our calculations. Finally, I have recently noticed that the entropic criterion for the renormalising function, $E_f = f$, involves the illumination which, with its quadratic definition $E_f(\vec{x}) = f(\vec{x})\, {}^t\vec{r}_f(\vec{x})\, \mathbf{H}_f^{-1}\, \vec{r}_f(\vec{x})$ may be regarded as an energy, and f defined as a mass. This is an interesting evocation of the general relativity. As such this evocation has no particular meaning. If we want to further support and perhaps understand it, the use of tensor analysis will

ACPD
4, S1420–S1425, 2004
Interactive Comment

become unavoidable.

I am really grateful to the referees who were enough interested into my work to help me find simpler arguments. Their simple thus important questions led me to clearly state a number of implicit hypotheses or conclusions.

References

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Interactive comment on Atmos. Chem. Phys. Discuss., 4, 2615, 2004.

ACPD

4, S1420–S1425, 2004

Interactive Comment