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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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I thank the referee for his careful consideration of my text and his persuasive insistence on the strategic advice already appropriately given by the other referee. I will follow them which probably means that my revised submission is not for tomorrow. New questions are raised by the comments of the referee giving the possibility to clarify additional angles. I choose again to reply quickly, perhaps not completely, with the risk, accepted, of a necessary subsequent correction.

1) I think that chapter 9 is based as supposed by the referee on an investigation of the error propagations. Despite the simplicity of this basis, it seems to me that significant differences appear with the classical theory. I have to be prudent because of my insufficient knowledge and practice of the classical techniques. Firstly, in the equations 59 and 60 of the paper, the covariance matrix for the estimation errors of the source, $\overline{\delta\sigma_{\|f}(\vec{x})\delta\sigma_{\|f}(\vec{y})}$, does not coincide with my background covariance matrix $\overline{\sigma_{\|f}(\vec{x})\sigma_{\|f}(\vec{y})}$

given by the equation 24. Secondly my most probable central source estimate, $\sigma_{\|f}$, would produce measurements $\mu_1, ..., \mu_n$ exactly equal to the observations. In the usual theory the central estimate is obtained by minimising a cost function. This cost function establishes a compromise, if I understand well, between a value of the source propagated by a model from a previous estimation, and the measurements presently available. As a result of this compromise the most probable central source will generally produce measurements slightly different from the observations. It seems so that both approaches correspond to different logics the contours and hence the complementarity of which should be clarified. So far I have not been successful and I would enthusiastically welcome any reinforcement regarding this task between other things.

2) When Dirac detectors are used corresponding to point measurements then the inversion is impossible in the ordinary geometry. The diagonal elements of the matrix **H** diverge (the off-diagonal ones do not). The adjoint concentrations r_i have a singularity describing an infinite 'mass' by the space-time position of the i^{th} detector. Then illumination vanishes everywhere except by the space-time positions of the detectors where it is infinite. This means that the source is rebuilt as a collection of infinitesimal sources inside each detector. This situation is 'smoothed' by the non zero size of the grid meshes. Nevertheless it is generally considered that the result of a numerical calculation is physically relevant if it displays only marginal variations if its resolution is refined. In our case a refinement of the resolution will just degrade the results by approaching the limit of the infinitesimal sources in each detector. I shall try to show this by adding on the figure 2 calculations with the resolution $0,25^\circ \times 0,25^\circ$.

3) On one hand the samples taken by the ordinary detectors are so small compared to the whole atmosphere, much smaller than the meshes of our models, that they must be considered as Dirac. On the other hand satellite measurements are now analysing really big air volumes that could be represented by a smooth detector function π_i. In that case as stressed by the referee no divergence would occur for an inversion in the ordinary geometry. Nevertheless the correction of the divergence is not the primary

aim of the renormalisation. I have developped the case of the Dirac detectors just because it is an extreme case: in that case the ordinary geometry is impossible to use so that its relevance, questionable once, becomes questionable always. And, this is at least what we tried to show, the question behind this is the choice of the best base functions, a choice which I related to considerations of artefacts and entropy. The best base functions are those bearing the least hidden hypotheses. This approach is valid even if the detectors are not Dirac. I think I have optimised the choice of the base functions among those of the form $r_{fi}(\vec{x}) = \frac{r_i(\vec{x})}{f(\vec{x})}$. There are others...

4) I did not take part to the development of the transport model POLAIR3D, I am just a user. It is as far as I know a state of the art transport model with the usual parameterizations. I shall ask my colleagues to provide further references.

5) The known domain is finite... a strange big-bang looking result, like the figures 3, 7, 8. The known domain is the reality as it looks like, not necessarily as it is. The influence on the various detectors of a point remote in the space or in the past from the network will not be very contrasted. All remote (visible) points display detection vectors $\vec{r}(\vec{x})$ almost colinear so that, when associated to sources of various intensity, they are undistinguishable. In fact, the know domain is the whole world or atmosphere, with a finite distribution of weights. Infinitely large space time regions of the standard geometry may receive a little weight thus becoming very little as a part of the 'known domain'. It is an infinite rubber put into a finite volume by an increasing compression of its remote parts.

6) The remote parts with a weak weight are unimportant, and so is the mass of tracer eventually released there, or: a big amount of tracer released in an unimpotant region is unimportant. In fact, in my theory, the natural geometry associated to the estimate is the renormalised geometry of the known domain. The representations of the estimate in the ordinary geometry may give a feeling of unsatisfaction as stated by the referee. I should have given the representaion in the renormalised geometry of the figures 7 and 8. But such figures require a huge amount of work. I shall find the courage to

prepare them anyway. According to my interpretation the total mass of the estimate $\int_{\Omega\times\mathrm{T}} \rho\, \sigma_{\|f}(\vec{x})\, d\vec{x}\, dt$ is not a really relevant physical quantity. The only relevant quantity is $\int_{\Omega\times\mathrm{T}} \rho\ f\ \sigma_{\|f}(\vec{x})^2\ d\vec{x}\ dt$, the total informational energy captured by the measurements.This suggests, and it is not pleasant at all, that $f(\vec{x})\sigma_{\|f}(\vec{x})^2$ is a more physically relevant quantity than $\sigma_{\|f}(\vec{x})$. From my calculations I got the feeling that, for a given amount of the total release $\int_{\Omega\times\mathrm{T}} \rho\, \sigma(\vec{x})\, d\vec{x}\, dt$ the reconstruction is all the better as the informational energy $\int_{\Omega\times\mathrm{T}} \rho\ f\ \sigma(\vec{x})^2\ d\vec{x}\ dt$ is low. I have recently performed inversions based on synthetic satellite images discretised as arrays of smooth measurements. Then, the geometry of the known domain is very close to the ordinary geometry for most of the domain of interest and the algebraic estimate is very satisfactory. This leads to the idea that the detectors should be arranged in such a way that the geometry of the known domain would coincide with the ordinary geometry for the region of interest. Just an idea...

7) The renormalised geometry is not flat in the neighbourhood of the detectors, and indeed, it could happen that there is a detection hole right in the middle of the network. The local behaviour of the illumination by the position of the detectors is not yet clear to me. I think that the renormalised illumination has locally, in the neighbourhood of Dirac detectors, the same behaviour as the empiric $f(\vec{x})$ defined in my previous paper by topping the non renormalised illumination with $\frac{\tilde{E}_{max}}{1000}$. Then, the renormalised illumination will diverge like the r_i^2 so that the renormalised retroplumes $r_{fi} = \frac{r_i}{f}$ vanish at the position of the detectors. Despite this vanishing, the environment of the detectors is still privileged by the inversion, but now finitely privileged. I think this vanishing of all the renormalised retroplumes by the position of all the Dirac detectors will touch only irrelevantly small space and time scales thus spontaneouly limiting the validity of the hypothesis that the measurement is a point. But philosophically it would bear with it the idea that a detector does not see itself.

The problem is that gridded calculations do not give a satisfactory account of the very local behaviour of the illumination, an analytic expression is required there, and I have

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no idea how to obtain it. I have undertaken a collaboration with Abdellatif El Badia, a mathematician of the Universite de Compiegne. Such problems correspond to his skills better than to mine.

8) My evaluation is that the time required for a renormalised inversion is five times larger than that for a standard inversion. Most of the time is spent for the calculation of the symmetric measurement matrices $\mathbf{H}_{f_k}$ by visiting all the meshes of the space time domain. The algorithm for f requires no more than ten iterations and ten symmetric matrices. For a more traditional inversion not using retroplumes as base functions the measurement matrix is not symmetric which doubles its calculation time. As indicated in the paper, on a Pentium IV, 2.8 GHz, 1 Go RAM, the calculation time was 2 mn 30 s for fifty measurements, 20 minutes for 137 measurements and recently 3 hours 30 mn for 400 measurements. The algorithms and the computers may be praobably optimised and the calculation of the various terms $h_{i,j}$ of the matrices may be easily parallelised.

9) In my method all the measurements are assimilated simultaneously because the particular meaning of a measurement depends on all the other ones. If additional measurements become available the meaning of the previous ones is changed so that all the measurements previously and freshly available should, in principle, be gathered for a new assimilation. It is obvious that this principle is waiting for relaxing conditions to be determined. The calculation times should then stay in a reasonable range.

10) I think it would now be worth investigating the use of the method for such sources as CO2, CH4, or CO, probably based on syntheitc measurements and with academic purposes such as the definition of an optimal network. I have not yet undertaken this task for lack of a global model that would be necessarily involved.

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References

[Issartel(2003)] Issartel, J.-P., Rebuilding sources of linear tracers after atmospheric concentration measurements, Atmos. Chem. Phys., 3, 475–486, 2003, previously published in ACPDiscussions, 19 June 2003.

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