Atmos. Chem. Phys. Discuss., 4, S1020–S1025, 2004
www.atmos-chem-phys.org/acpd/4/S1020/

Atmospheric Chemistry and Physics Discussions

ACPD

4, S1020–S1025, 2004

Interactive Comment

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

J.-P. Issartel

Received and published: 29 June 2004

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

ACPD

4, S1020–S1025, 2004

Interactive Comment

29 June 2004

I thank the referee S. Houweling for his detailed reading of the paper and for the kind sincerity of his comments. I am very aware of the difficulty of my text with its mathematical formulation. The main reason is probably the way this work was achieved by carefully considering the equations in order to physically interpret their logical constraints with the help of simulations. This approach often led me far from my standard intuition. I do agree that the presentation of this work should now be made more friendly with a focus on the physical angles. I shall just need time to revise the text, possibly a few months, in order to work with the benefit of hindsight. I am sure that the editor can understand this. I can of course right now clarify my text by adressing the points raised by the referee.

In order to foster the discussion that will now rapidly reach its end, I prepared the following reply which perhaps does not go in all the required details. I shall have later more time to improve the present comments.

1) The method is intended to improve the source estimation in general. In fact the question that is behind my work is the following: I have observed at various positions and dates concentration measurements $\mu_1, ..., \mu_n$, I perfectly know the meteorological fields, I have no other piece of information, even indirectly, except perhaps the posi-

tivity of the source. What best can be said about it? When a point source is used to prepare artificial values of the measurements, I hope that my inversion procedure will 'understand' the source is a point by just considering this special value of $\mu_1, ..., \mu_n$.

2) Nevertheless the latter aim is an impossible wish. Retrieving a point source would require an infinite resolution in space and time. This infinite resolution is clearly out of reach with a few tenths (or thousands) of measurements. In general I cannot expect to reconstruct all the details of the source. I shall necessarily retrieve a smooth version of it, and this smoothness is not an aim, it is a physical limitation. As said by the referee, if I understand him well, this limitation may be excessive thus leading to unsatisfactory inversions insufficiently constrained by the available observational evidence.

3) My paper can be seen as an exploration of this smoothness limitation. The source estimate is investigated first as a linear combination of adjoint base functions $r_1, ..., r_n$. I show that these standard adjoint functions are not adequately smooth so that inversion artefacts occur. Then I show that 'optimally smoothed' adjoint concentrations $r_{f,1}, ..., r_{f,n}$ may be obtained by introducing a renormalizing function f. The best f may be characterised unambiguously.

4) The referee says that for a point source at the position of a measurement the inversion might be divergent. Absolutely! I have just made the calculation for a source of 340 kg in the same grid mesh as the station F02 thus obtaining an algebaic estimate with a total of 50 000 - 21 000 kg (and a positive estimate of only 1400 kg). This divergence is not worrying, it is normal and even reassuring. Firstly, when the source is put inside the detector, it is not the inversion which is divergent, it is the value of the measurement. I recall that the source estimate may be written $\sigma_{\|f} = \sum \lambda_i \, r_{f,i} = \sum \mu_i \, g_{f,i}$ with $(r_{f,i}, g_{f,j})_f = \delta_{i,j}$ (Kronecker's symbol). Secondly a point source inside the detector is a highly 'improbable' configuration. When the inversion is adequately smoothed the source is investigated at space and time scales consistent with the relative arrangement of the detectors. This means that if the distance between two detectors is typically 1000 km then any set of measurements will be interpreted in terms of a source

ACPD
4, S1020–S1025, 2004

Interactive Comment

at continental scale with all other details smoothed as irrelevant. Then, if somebody scratches a match just in front of a detector, we shall logically conclude that Amazonia is burning! Indeed the presence of an important point source much closer to a detector than the typical distance between two detectors is a serious disturbance and should be avoided. The figure 9 of the paper shows that a source stretching continuously around, and inside, the detectors does not cause any divergence.

5) The comparison with other methods is a delicate matter. I can just give a few elements. Traditionally the measurements effectively observed $\mu_1^{obs}, ..., \mu_n^{obs}$ are decomposed into $\mu_i^{obs} = \mu_i^{pri} + \delta\mu_i^{err}$ where μ_i^{pri} is the value of the measurement expected from an a priori value of the source, $\delta\mu_i^{err}$ is an error due to the limited quality of the model and of the observations. It seems that the decomposition I propose is very different : $\mu_i^{obs} = \mu_i^{pri} + \delta\mu_i^{new} + \delta\mu_i^{ter}$. The difference between the a priori value (taken to be zero in the paper) and the observations is decomposed into two terms. The term $\delta\mu_i^{ter}$ is the technical error corresponding to the imperfection of the detectors (and of the model: a point to further investigate). The statistics of $\delta\mu_i^{ter}$ are known technical properties of the detectors. I also considered that, even if the model and detectors were perfect, it would not be possible to know the source completely because it is an infinite dimensional object. Due to our incomplete knowledge of the source the observations would still drift from the a priori values. This is the meaning of $\delta\mu_i^{new}$ (corresponding simply to μ_i in the paper). This additional term is not an error. Before the values of the measurements are known $\delta\mu_i^{new}$ may be considered a random variable; as such I called it the 'anticipation'. Then $\delta\mu_i^{new}$ and $\delta\mu_i^{ter}$ are statistically independent due to their fundamentally different nature.

6) The statistics of the anticipation is fundamentally not a technical problem. It is, at least we tried to show, a geometric problem. The word 'geometric' means here that the statistical law of the anticipation depends only on the arrangement of the detectors in space and time together with the meteorological fields. The statistics of the anticipations would not be altered if the quality of the detectors was changed.

ACPD

4, S1020–S1025, 2004

Interactive Comment

7) The referee asks me to clarify on the page 5 some equation which is not clearly indicated. There are so many equations in my text that I do not clearly understand what should be clarified. Perhaps I should say more clearly that the statistics for the anticipation and those for the source (the background covariance matrix) may be deduced from one another by means of the equations 10 and 11. But both statistics have to be admitted as an assumption. I prefer the word ’assumption’ to the word ’definition’ in this respect because I feel reluctant considering that the reality behaves according to my definitions. The statistics of the sought source are described, in my procedure by a ’background covariance matrix’ **B** which is neither a priori nor a posteriori. The value of **B** may be calculated completely, once and for all, (equation 24) before the effective value of the measurement is known. This is intended to answer the remark of the referee about the page 4. In my procedure there are no indirect observations. The background covariance matrix becomes a geometric property of the measurements to be inverted : a property that depends on their relative arrangement in space and time, not on their values neither on their technical quality in terms of noise.

8) The work of Bennett and McIntoch has been indicated to me a few weeks ago by Abdellatif Ouahsine of the Universite de Compiegne. These authors have clearly stated the requirement for some ’renormalisation’ in order to remove the singularities of the adjoint functions by the positions of the detectors. I extend the requirement for such a renormalisation even in a situation where no singularity is to deplore. The renormalisation is in fact an entropic requirement. Due to the observation the entropy of the observed system decreases. The renormalised inversion is the one that lowers less the entropy of the system (so that the entropy after the observations is maximum, though less than the entropy before the observations).

Another difference with Mc Intoch and Benett is the decomposition of the observations described in the point 5.

9) The comparison with quantum theory is important and must be indicated. Quantum theory is a theory of the measurement, the link with data assimilation is not so

ACPD

4, S1020–S1025, 2004

Interactive Comment

surprising and I think it may be more than an analogy. The parallel with this theory is also a beginning of interpretation of the strange results obtained with the focus of the 'informational energy' at the right position. So this parallel is both an interpretation and a question for further investigations. I think raising new questions is also in the scope of the journal ACP. Nevertheless the presentation of this interpretation may certainly be reduced.

10) I do not see any problem with figure 2c3. Is it possible to the referee to give me a clearer indication of what is wrong?

References

[Bennett and McIntosh(19)] Bennett, A.F., McIntosh, P.C., 'Open ocean modeling as an inverse problem: tidal theory', Journal of Physical Oceanography 12, 1004–1018, 1982.

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

ACPD

4, S1020–S1025, 2004

Interactive Comment