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Interactive Comment

Interactive comment on “Optimizing CO_2 observing networks in the presence of model error: results from TransCom 3” *by* P. J. Rayner

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Most of the comments from the two referees ask for some further explanation. Some of these, I believe, have already been addressed in response to the “prereview” technical changes requested by the referees. I will point out the relevant sections of text that I think already address these. In the sections below, questions and comments from reviewers are indented.

1. Specific comments from C. Rödenbeck

> I fully accept that the ‘between uncertainty’ (defined in Eq (2) as the empirical standard deviation of flux estimates across the ensemble of transport models) is an appropriate measure of the model spread for the given pur-

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> pose. It should be kept in mind, however, that the ensemble of models is by far not a stochastic sample (in particular, not a Gaussian around some truth). As a practical consequence, some different weighting of ‘within’ and ‘between’ uncertainties in the ‘total’ uncertainty seems to be as justified as the equal weighting chosen here. It might be interesting to discuss this a bit, as this weighting would probably shift the threshold in the balance between selecting a site in order to constrain fluxes and rejecting it in order to reduce model spread. Moreover, a remark should be made that errors common to all models will not be taken into account by the optimized network. Such common errors are very likely to exist and to be significant.

This raises a series of good points although I don’t agree with the “practical consequence”. It is worth expanding on what the “total uncertainty” metric is meant to describe. It seeks to answer the question “if we choose one model and one realization of the observations, both at random, what confidence interval on estimated fluxes can we expect?” We assume, completely arbitrarily I admit, that the two contributions to the probability density function (PDF) for the estimated fluxes are independent, namely that part caused by our random choice of model and that part caused by our random choice of data. Given these assumptions, I do not see a justification for choosing an unequally weighted combination of the 2 PDFs.

We derive the PDFs for the two components in different ways. The “within” part we get by propagating the data uncertainties through the inversion formulae, the “between” part we must estimate from the sample distribution we have available. We have used just the mean and variance to describe this distribution; a description that is only accurate if the PDF is Gaussian. In preparing the Transcom-3 control cases we had a discussion about this same question. The variation of estimated flux results with model is shown in Figs 7 and 8 of Gurney et al. (Tellus, 55b P555, 2003). These are not histograms so it’s a little harder to check but we did plot them as histograms and found the sample distribution, at least, surprisingly Gaussian. Of course it isn’t exact and

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even if it were it would not mean the population had the same distribution but I doubt the assumption is a serious problem for the calculations in the paper. Some of the above explanation does help to clarify the assumptions we have made so I have added a paragraph to the paper.

> The three cases of optimization, termed ‘within’ etc., should be explicitly defined, e.g. by explicitly stating the link between the different measures of uncertainty on the one hand, and the metric (and the score) used in the genetic algorithm on the other hand.

I have referred back to the relevant equations in the description of the experiments and also added an equation for the “total uncertainty” to make this clearer.

> The members of the population are said to be defined by a set of integers. It should be explained what the values of these integers mean.

I have now made this more explicit in the method section.

> It seems that the effect of taking an observing site n times means an enhanced quality requirement for that site (reduction of required data uncertainty by 1/ n). This should be explained in the methods section.

In fact the opposite is true. Using a station with data uncertainty σ n times is like using one station with a data uncertainty $\frac{\sigma}{\sqrt{n}}$. This assumes implicitly that the data uncertainties on each individual measurement at the same point are independent which is probably optimistic. See the discussion of data uncertainty in the supplementary material of Gurney et al. Nature, 2002, for a more detailed explanation.

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The given discussion of robustness of the TransCom3 annual mean estimates specifically refers to the choice of the optimal observing network, not however to the various other aspects of an inverse calculation. To avoid confusion to readers outside the inversion community, this qualification should also be added in the abstract, as any present-day CO2 inversion results (or at least certain features thereof) are of limited robustness with respect to many of the choices that have to be made.

This is a very good point. In fact, robustness was a bad choice of word and I have changed the abstract accordingly.

In the captions to Figs. 1 and 2, top and bottom panels are referred to by (a) and (b) which is however not defined in the graphics.

Fixed.

2. Specific comments from P. Kasibhatla

How many parameter values (index of possible observing sites) are in each of the 200 members when the algorithm is initialized? Is it 110 (one for each site), and the starting members simply differ in the order in which these parameters are arranged? Or is it something else?

This is part of the concern raised by Dr. Rödenbeck and I believe the modifications addressing his concern also address this one.

At the end of the 500 iterations, one ends up with a population of 200 members, each made up of the same number of parameter values as in the initial

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set-up. Is that correct? If so, it is not clear as to what is being presented in Table 2 and Figs 1 and 2. I assume these are some summaries of this ending population - please clarify exactly what these are.

How is convergence tested for - I assume this is meant to be addressed in Section 2.2. But I find the explanation lacking in that it is not clear to me why an acceptable configuration from the test described translates into an acceptable configuration for the real experiments.

I will deal with these comments as a group since addressing them required a series of coupled changes. I have added another item to the list in 2.2 describing convergence and a sentence later pointing out that the final population consists largely of clones of the fittest member.

To what extent are the optimaization results sensitive to the parameters used in the GA algorithm, and to what extent are the results dependent the fact that the GA algorithm itself is used in the first place?

Operationally this question boils down to whether the genetic algorithm is succeeding in finding an optimum. If not then the algorithm or its implementation are failing. I don't think this is the case, supported by the test case but I can think of no other way of verifying this. Certainly the set of available solutions (110^{76} for a 76 station network) is far too large for direct inspection and the results are counter-intuitive enough that a more heuristic method such as the incremental optimization approach of Patra et al. (2003) doesn't seem likely to succeed.

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3. Other editorial changes

"Sampling is without replacement" should have read "Sampling is with replacement" and I have corrected this.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 5185, 2003.

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