**Atmospheric
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Discussions**

Interactive
Comment

Interactive comment on "Inverse modelling of the spatial distribution of NO$_x$ emissions on a continental scale using satellite data" *by* I. B. Konovalov et al.

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This study presents a new technique for estimating sources and sinks of NOx from satellite observed total column NO2. Contributions in this field are welcome because valuable satellite products are available, but it is not yet clear how to take optimal advantage of the data. This study presents some novel ideas on the use of additional ground based data and reduction of transport model calculations that are worthwhile. The manuscript is rather thorough on mathematical aspects. However, the link between mathematical concepts and interpretation in terms of 'real world' emissions

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needs some more thought. Besides the method as such, it remains unclear what can be learned from the actual emission estimates. In my opinion, important revisions are needed to make this article suitable for publication on the subjects listed below.

GENERAL COMMENTS

Uncertainty versus variability

This manuscript confuses uncertainty and variability of signal. Generally, the overall observed variation is the sum of variation in signal and variation due to measurement errors. In principle, for both components a standard deviation can be calculated, but the resulting sigma's nevertheless mean something different. For example, in equation 5 sigma_c and sigma_e refer to uncertainties. However, in equation 16 the ratio is taken of sigma_e (=error) and the standard deviation of the difference between two estimates (=error + signal). Another example is equation 18 where sigma_s (=error) is made equal to the variance of the difference between observed and modelled concentrations (=error + signal). The authors claim that the uncertainty of the various sources of input data can be estimated from the data using their method, which in my opinion is not the case because the method does not allow separating the contributions of signal and error to the overall variance. This issue should be resolved.

Use of bias

The authors rightfully interpret the bias term delta_c as the difference in the bias of the model and the observations. By removing this bias they guarantee that the inversion approach satisfies the assumption of unbiased random variables and produces statistically consistent estimates. However, the meaning of these estimates remains unclear as long as it remains unknown which part of the bias comes from the model and which part from the measurements. This point is actually acknowledged by the authors on page 25 where they mention that: 'It is useful to note that the improvement in agreement between the simulated and measured NO columns does not necessarily mean that the a posteriori emissions are actually better than the a priori ones.' This seems

to suggest that the inversion is actually not meant to improve emission estimates. It raises the question what else is really the aim of the inversion? In the next sentence the authors mention that 'However, it is important that the residual uncertainty of the NO2 columns is in perfect agreement with its estimation (sigma_c) obtained using the linear statistical models'. Statistically consistent NO2 columns are a useful product, however, by removing the bias between model and measurements does not at all guarantee that obtained NO2 columns are bias free. This would also require knowledge of the part of the bias that comes from the model and the part that comes from the measurements; however, no attempt is made to address that issue. The authors should clarify what the purpose is of the bias correction. Currently it is also unclear how large the bias correction is and if the relation between bias and observed NO2 column might explain the origin of the bias.

Posterior NOx emissions

The authors claim to have reduced the uncertainty in the NOx emissions over Europe by 40%. Yet hardly any quantitative information on these fluxes is provided that would allow comparison with other studies. Only Figure 7b provides the logarithm of emissions without a unit, which, obviously can hardly be used for the purpose of inter-comparison. At least a table is needed with prior and posterior emissions per country with corresponding uncertainties with some discussion of the significance of the numbers.

Deconvolution of low-resolution columns

It is unclear why information is added to the measurements to increase its spatial resolution (page 9). Probably the high-resolution information is already present in the a priori estimate, in which case it doesn't introduce any new information but rather counts the same information twice. Indeed the authors mention that when using the CHIMERE patterns measurements and model loose independence. The use of SCIA-MACHY data from 2004 seems problematic also since, judging figure 9, SCIAMACHY

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measurements yield quite different results.

Units

Somehow the manuscript should be clearer in the units of the reported emissions and uncertainties. The origin of confusion is probably the use of lognormal distributions. In a number of instances it is not clear whether numbers refer to emissions or logarithms of emissions and uncertainties or relative uncertainties. Some examples are:

page 21: 'It is found to be about 0.01.' page 25: 'In particular, the uncertainty of the a priori emissions is estimated to be about 0.6' page 25: 'the corresponding RMSE has been reduced from 0.43 to 0.30' table 1: sigma_e & sigma_c figure 9: 'The ratio of the difference between the natural logarithms of the a posteriori and a priori emissions to the $>>$ log? $<<$ uncertainty of the a posteriori estimates'

SPECIFIC COMMENTS

page 6. SNAP sectors should be explained.

page 11. What could be the reason why measurements at Sniezka are so much higher than the model?

page 26. Last paragraph of 4.2: If I understand well the RMSE improves from 0.3 to 0.25. This doesn't seem to justify the next sentence ('inversion corrects the major part of the discrepancy')

Figure 6b. I would have expected that with perfect data the estimated ratio of observed and prior sigma would be at the lower limit. Yet the minimum of the blue curve is at a ratio of 0.2. Please clarify.

TECHNICAL CORRECTIONS

Page 18: 'The idea of the tests is to perform inversions for synthetic data and to compare the results with the exact solutions known a priori.'

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The use of 'a priori' here (and in some other cases) confuses the reader, since this is not part of a prespecified first guess (whereas elsewhere 'a priori' is reserved for that)

page 19: squares are missing in equation 14

page 19: 'emisions' = emissions

page 19: 'The RRMSE expressed in terms of absolute emissions' = RMSE

page 21: 'since the parameters of the distributions (2)' I guess (2) doesn't mean equation (2) here, but 'mean and sigma'. Please avoid confusion.

Figure 4 is not referred to in the text.

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