Atmos. Chem. Phys. Discuss., 13, C6106–C6109, 2013
www.atmos-chem-phys-discuss.net/13/C6106/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Inverse modeling of Texas NO_x emissions using space-based and ground-based NO_2 observations" *by* W. Tang et al.

Anonymous Referee #2

Received and published: 26 August 2013

Review of "Inverse modeling of Texas NOx emissions using space-based and ground-based NO2 observations" by Tang et al.

General comment: The goal of the article is to test the applicability of different inverse methods to improve NOx surface emissions in Texas and ozone predictions in air quality models. The authors applied different inverse modeling techniques using OMI satellite NO2 column or EPA AQS NO2 ground sites measurements over 2 months to improve NOx emissions from the TCEQ inventory in different regions in Texas that suffer from air quality problems. The authors used CAMx for modeling chemistry, and the MM5 meteorological model for the meteorology. The results from the inversions based on the two set of observations vary by a factor of 3 to 4, with OMI based posteriors increasing the prior emissions by up to 80% while the AQS ground site based inversion

decreases the NOx emission in the posterior by ∼50 to 70%. Each posterior along with the prior are used to simulate ozone concentrations in CAMx. The results are worse when using the posteriors.

Due to the large discrepancies in the posteriors and the results on simulated ozone, the authors conclude that top-down approaches should be seen as a complement to bottom-up approach, rather than a substitute in the region of interest.

Such study should produce interesting results, but the inversions applied in this study still need a lot of improvements before drawing any conclusions on bottom-up and top-down approaches. I recommend major revisions before considering this paper for publication.

Specific comments:

1) The authors restricted their inversions to 2 time periods of 1 month during which high ozone concentrations were measured at either Dallas or Houston regions. Since the authors used OMI satellite data and continuous AQS ground site measurements, I don't see the reason for restricting the analysis to those 2 months specifically. I would extend the analysis to several months to minimize the uncertainty in the posteriors.

2) In section 2.4.2, page 17487, line 14 and 15: the authors say that they assumed an uncertainty of 0.15 for the ground site measurements, 0.3 for the OMI data, and 2.0 for the prior inventory in the covariance matrices. An uncertainty should have a unit. I assume that those values are standard deviations relative to the mean value, basically uncertainties of 15%, 30% and 200% respectively.

In an inversion, the covariance matrices play a key role in the posterior results. The prior uncertainty estimate used in this study, 200%, is in my opinion quite large based on the work done by TCEQ. The authors justified this assumption because of the assumption made by Napelenok et al. (2008) who assumed an uncertainty of 200% for the EPA NEI 1999 inventory. The authors cannot assume the same uncertainty for EPA

ACPD
13, C6106–C6109, 2013

NEI 1999 and TCEQ 2005 inventory without a minimum of justification.

The authors tested the sensitivity of the posterior on the covariance matrix of the prior using a test based on pseudodata in section 3.1. The underlying assumption in this kind of test is basically a perfect (unbiased) model. I am not convinced that this test can be used as a test for the sensitivity of the posterior on the assumptions made on the error in the observations (15 to 30%) and the prior (200%). The authors should use the OMI and AQS ground site observations for testing the sensitivity of the posterior on the assumptions made in the covariance matrices, and not pseudodata.

3) I am not sure to understand why the direct scaling inversion method is used in the paper. Is it to prove that this method shouldn't be used? If so, the authors should state that more clearly in the text and conclusion.

4) The fact that the authors used monthly average measurements from OMI to drive the inversion is probably okay. However, since they restricted their analysis to 2 months, it is as if the inversion was driven by only 2 independent observations in each region, which is not a lot of observations to have good confidence in the posterior results.

However, using 24h-average NO2 measurements from AQS ground sites to minimize the uncertainty from the influence of PBL height on NO2 concentration is not a good idea. At night, power plant plumes are lifted above ground because of the buoyancy of the stack when they are emitted. Since the PBL at night is stable, they don't mix down to the ground. Hence the AQS ground site measurements are not representative of the concentration higher in altitude. If I understand correctly, the power plant are emitted in the model at the surface, without buoyancy. Therefore, one can expect a bias in the model results at the AQS ground site locations at night.

Therefore, using 24h-average AQS NO2 measurements in the inversion will probably make the inversion underestimates the surface NO2 emissions. This is probably the reason why the posterior from the inversion based on AQS ground sites is much lower than the inversion based on OMI data. The authors should use daily average NO2

ACPD

13, C6106–C6109, 2013

measurements instead. PBL uncertainties are not a problem in an inversion as long as the PBL height is not systematically biased.

5) I don't see any validation of the meteorology in the paper. How good is the wind speed, wind direction, PBL height? The meteorology from MM5 must be evaluated. You can use for instance the aircraft measurements from TEXAQS 2006.

6) In the conclusion, the authors say that DISCOVER-AQ flights in fall 2013 will help in reducing the discrepancy in the posteriors with spirals that will be performed over Houston. Why don't the authors use the NOAA flights during TEXAQS 2006 in September/October 2006 to drive the inversion then? At least the NO2 measurements during TEXAQS 2006 are of better quality than the EPA AQS ground sites. They could even use a longer lived species like NOy which will reduce the uncertainty of the inversion.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 17479, 2013.