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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimating European volatile organic compound emissions using satellite observations of formaldehyde from the Ozone Monitoring Instrument" *by* G. Curci et al.

Anonymous Referee #1

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The article "Estimating European volatile organic compound emissions using satellite observations of formaldehyde from the Ozone Monitoring Instrument" by Curci et al. is potentially scientifically useful to the research community. The objectives of the article and the approach are generally very clear. The study uses HCHO observations from OMI to improve emissions of VOCs via the source inversion that involves CHIMERE model. The authors describe their Bayesian inverse methodology, the data used and ensure robustness of their result by comparing their results with independent measurements and by performing sensitivity studies. Unfortunately, the motivation for the study is not sufficiently strong and the reader can't help but question the results, given how close all the measurements are to detection limit. I thus have a few general and specific comments and suggestions that I list below and that I hope will be useful to future readers.

General comments: 1. It would be great to read more about how this study fits in with previous literature. How is it an improvement or innovation upon what came before. 2. Throughout the paper, it is remarked that OMI-observed HCHO over Europe is close to detection limit, especially in the colder months. This naturally raises the question if it is appropriate to use OMI data for such a study. It would be helpful to read a much stronger justification for the study. One also can't help noticing that the best measurements are over sunny, cloudless regions. The paper does not, however, address the possible bias coming (or not) from that. 3. I found the organization of the paper a bit confusing. For example, in section 2, I expected to see model validation. It was not intuitive to me that it would be in section 3.2. Meanwhile, section 4.2 devoted first three paragraphs to errors and not emissions. Perhaps the order in that section could be reversed? 4. As I am sure the authors do as well, I wish there was more data to compare the model and results to. It seems that EMEP measurements are not at all useful in evaluating the inversion results. Are there any other data? Maybe even a quick comparison with SCIAMACHY could be informative. The authors point out that the Bayesian framework necessarily yields equal or smaller a posteriori errors. Therefore, if there is any other data to include in the study, it would be of great help.

Specific comments: p 19703, l. 1-2: What is the implication of the model not including the latest isoprene chemistry. Does this introduce a bias? Which way? Again, one wonders about all details since data is close to detection limit. p 19703, l. 2-7: I'm confused if all those issues listed are related. p 19704, l. 1: Please define CCDs p 19705, l. 1-4: This is where I start to wonder if there could be a clear-sky bias. p 19705, l. 7: replace "of" with "as" p 19706, l. 20-22: Is upper troposphere feature captured by CHIMERE model, considering that its vertical extent is only 200hPa? p 19707, l. 13-14: This is where I again wonder if there is a clear-sky bias (due to photochemical production of HCHO). p 19708, l. 5-10: It'd be educational to see what's

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"very small bias" and "poor" correlation for HCHO. Please provide more numbers. p 19709, l. 5-6: How do we know that regional biases are due to incorrect emissions? How about chemistry and transport errors? p 19709, l. 8: remove "on" p 19709, l. 18: "column background of 55-85%": What is this a percentage of? I was confused. p 19711, l. 19-20: If it is a linear relationship, it doesn't mean that you can neglect transport (think CO, for example); please clarify your assumptions that allow you to neglect transport. I'm thinking it's a short lifetime. p 19712, l. 9: remove "s" at the end of "two-elements" p 19713, l. 19-20: The sentence "without proper "guidance" the inversion..." sounds quite suspicious. Could you be more explicit about the "guidance"? p 19713, l. 21-23: I don't understand why you would use daily OMI data to constrain monthly emissions in this inversion setup. Isn't it much more computationally expensive without much scientific gain? Perhaps the scientific gain is not transparent to me. Please clarify. Section 4.2: It would be nice to see absolute emission numbers rather than only percentage changes. Future studies that might not use MEGAN might have an easier time comparing results. In fact, it would be great to have a table, like Table 2 or Table S1, with a breakdown by large source countries. Looking at the maps, it's clear there are some inhomogeneities in the inversion results that could be averaged out in the whole-Europe numbers. p 19718, l.3: Although it would be nice to account for nonlinearities in the problem, the adjoint model framework would not be the solution. The adjoint is just a more computationally efficient tool for the same linear approach. Only the Ks could be updated with each iteration. Unless this is a different point and I am missing something. Please clarify or remove. p 19720, l. 4: remove "s" at the end of "depends" p 19720, l. 5: I do not understand the phrase: "uncertainty is introduced by estimation of inversion parameters".

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 19697, 2010.