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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimates of European emissions of methyl chloroform using a Bayesian inversion method" *by* M. Maione et al.

Anonymous Referee #2

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The manuscript by Maione et al. uses measurements of methyl chloroform at several monitoring sites in Europe to estimate the source regions and magnitude of emissions of this chemical. Such estimates are important for evaluating trends and for comparing reported emissions to those derived from atmospheric measurements. The emissions are estimated using a Bayesian inversion method, which uses meteorological analyses and initial estimates (and uncertainties) of emissions to find a distribution of sources of methyl chloroform that best describes the observations. The authors find a significant source of methyl chloroform from the southeast region of France, which is attributed primarily to chemical production facilities and hazardous waste processing. Further, the analysis identifies additional sources not previously identified, such as in Northern Italy. I expect that the SEF source could (or was) already identified from emission

data and simple trajectory analysis, but this manuscript provides a more sophisticated approach that allows emission estimates to be made.

The paper is well organized, the measurements and the modeling are sound, and the conclusions are interesting. I would recommend publication of the manuscript. I have only minor questions and comments, most of which reflect my own unfamiliarity with the inversion procedure and assumptions.

Questions/comments:

p.8215, line 23. Since there is some detail given about the analytical procedure, I was curious about the statement about the standard humidification and linearity. Multi-level calibration and linearity are not discussed in the method description. How is this done and how often is linearity checked? Is there an issue with non-linearity in the measurement of the higher level signals? Also, it's not clear to me how a uniformly humidified standard ensures "a close similarity in composition" to air at a coastal station and also a mountain site. Probably not a big deal, but if not then just leave out all the boiler plate that is not supported nor particularly relevant to the current manuscript.

p.8220, line 1. The baseline attribution seems reasonable to me, but I wonder if there are any comments about the baseline irregularities, such as the concentration dip at CMN in Jan 2006, or the longer term cycle at MHD throughout 2002-2003.

p. 8220, line 19. Obviously it is not just the location of the MCF source nearer to CMN, but also the wind direction and transport.

p. 8217, line 25. Please add "emission estimate" or similar after "a priori". Try to avoid modeling jargon.

p. 8223, section 3.2.3. I was struck by the distinction of the influence of SEF on the observed mixing ratios. Visually, all of the excursions, with the exception of a period in early 2009 at CMN, are due to the influence of SEF. This would seem to indicate that all of the other information on other emission sources are contained in mixing ratio

excursions above background that are in the 0.1 – 0.3 pptv range, which is close to the uncertainties in the background itself. Is this true? It seems remarkable to me. Can this aspect be discussed a bit further?

p.8221, line 1. It is noted that the MCF emissions to soil and water are not included in the a priori emission estimates (though it is suggested that these sources are revealed in the analysis). It would be useful to understand the relative magnitude of these sources, since I assume they are just a slower release to the atmosphere compared to direct emission.

p. 8221, Figure 4. Though I could see the location of the sites on other Figures, the black dots that designate the measurement sites tend to disappear in the dark purple.

General comment: The appendices provided little information that I thought was particularly important for the analyses. I would expect that the basic findings of the Appendices could be incorporated into the main text and reduce the overall length of the manuscript. Some parts of the appendix discussion, too, e.g. A1.4/Table A.2, were rather opaque and unintelligible to someone (me) with no expertise in evaluating model statistical tests.

General comment 2: It would interest me to know what other of the suite of trace gases measured at CMN (especially) can also be traced back to the SEF sources?

General comment 3: The emission estimates should be reported with some clear statement of the associated error. If stated in the text, I missed it.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 8209, 2014.