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Atmospheric Measurement Techniques Discussions

Interactive comment on "MAMAP – a new spectrometer system for column-averaged methane and carbon dioxide observations from aircraft: retrieval algorithm and first inversions for point source emission rates" *by* T. Krings et al.

F.-M. Bréon (Referee)

Francois-Marie.breon@lsce.ipsl.fr

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MAMAP- a new spectrometer system for column averaged methane and Carbone dioxide observations from aircraft: retrieval algorithm and first inversions for point source emission rates.

This paper presents an experiment where airborne measurements of total column CO2 and CH4 are used to estimate the emissions from two large coal power plants in Germany. This experiments, in addition to its intrinsic interest, is of great value for the eval-

uation of the Carbonsat Concept, which has been pre-selected by ESA and currently under phase A study. The manuscript briefly describes the CO2 column retrieval algorithm, it details several method for emission estimate from the column measurements, it compares the estimates to the reported emissions from the company operating the power plant, and discusses the uncertainty estimates. There is no doubt that this paper provides new results in a rather clear and very comprehensive way. It should therefore be published. I do have a few criticisms that should be accounted for by the authors:

One of the approach used to invert the emission fits the column measurements to a Gaussian shape as described in eq 13. The inversion has therefore one free parameter (the emission rate) but the width of the plume has to be fixed according to strong hypothesis on the atmosphere stability. I wonder why the authors did not rather made an inversion with 2 free parameters, i.e. the "a" parameter of eq 15 in addition to the emission. There is certainly enough independent observations to invert two free parameters, and this would avoid a strong hypothesis in the retrieval.

I am rather surprised by the modeled vertical distribution of the CO2 plume (eq 26). There are two terms. One with a maximum concentration at stack height (OK, fair enough) and another below the ground but another with a maximum below the surface. What is the purpose of the second term ? In addition, is there any evidence that the plume vertical distribution has such shape. Because the effective wind speed is directly affected by the vertical distribution, this has some consequences.

I already posted a comment on the computation of the averaged wind speed. I give it again here: The wind speed is an essential parameter to infer the emission from the column concentration. Indeed, the column concentration is inversely proportional to the wind speed (see eq 13). From the wind speed vertical profile (in fact, two layers with different wind speeds), the authors compute an averaged value Ua, weighted by the fraction of the emission in each of two layers (w1 and w2). Ua = w1 U1 + w2 U2 I argue that, as the vertical column is proportional to the inverse of the wind speed, the averaged wind speed should be computed as 1/Ua = w1/U1 + w2/U2 This has large

consequences; In the case of Janschwalde, the values are w1=56%, w2=44%, U1=3.6; U2=6.5 Which leads to averaged wind speeds of either 4.88 (authors method), or 4.48 (present) In the case of Schwarze, w1=55%, w2=45%, U1=2.5; U2=5.6 Which leads to averaged wind speeds of either 3.9 (authors method), or 3.33 (present) Thus, it seems that the effective wind speed is overestimated by about 10%, with an equivalent impact on the power plant emission estimate.

Atmospheric stability : There is a discussion at page 2232 that leads the authors to assume that the atmosphere can be classified as "very unstable". I disagree with the authors has, during early mornings, the night inversion is usually still presents and provides some stability to the atmosphere. In addition, the sun energy inputs is much smaller than at midday which therefore limits the surface heating. I believe that the authors try to justify here their choice of a rather wide plume (observations) which can only be reproduced by the Gaussian model with a "very unstable" parameter. This is, I believe, another argument to keep the plume width (i.e. a) as a free parameter in the inversion.

In addition to Figure 3, it would be most useful to show a cross-section (along black lines) of the measurements and models. I understand the model would be a Gaussian shape along the flight track and would like to see how the measurements get distributed. Please add a figure.

I am rather surprised that the authors estimate the uncertainty on the wind speed based on its reported bias. I am aware of many variables which are known with essentialy no biases but rather large uncertainties (ie uncertainties much larger than biases). Please justify the choice.

The section on aerosol impact on the measurement (p 2237-2238) is rather long and could be very much reduced.

Other minor comments (mostly typos) are given below. I strongly recommend that the author use a spelling/grammar corrector. In fact, they should have done so before

submission. . . Abstract : "reliable estimates" should be more quantitative P2211, L28: sampling, not samling P2213,L18: Description of instrument FOV is not clear P2218, L11: topographic P2220, L8 to the fact that. . . (no comma) P2224, L17: "decent" does not seem appropriate here P2229, L4: inhomogeneity P2236, L22: sophisticated

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 2207, 2011.