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Atmospheric Measurement Techniques Discussions

Interactive comment on "Airborne remote sensing and in-situ measurements of atmospheric CO_2 to quantify point source emissions" *by* Thomas Krings et al.

Anonymous Referee #1

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This paper describes an attempt to estimate CO2 emissions from coal-burning power plants in Germany using either airborne remote sensing measurements or airborne in-situ sampling within the plume. The data analysis and emission estimates use either a mass-balance approach or a Gaussian plume assumption. The uncertainty analysis leads to relative errors on the order of 10-15% and the estimate comparison to reported emissions are consistent with this relative errors. This paper contains a lot of interesting material for an evaluation of the potential and difficulty of such approaches for the estimate of CO2 emissions from point sources. It should eventually be published. On the other hand, I have been very disappointed by the manuscript presentation that looks more like a experiment report than a scientific paper. Also, the paper lacks

conciseness and many figures are not necessary. Although the paper combines the in-situ and the remote-sensing approaches, there is no real discussion of the pros and cons of both. Although the in-situ wind speeds are used for the interpretation of the remote sensing data, the in-situ concentration observations (Figure 5) should have been used, I think, for a discussion on the validity of the Gaussian plume model. It is really not clear why the emission from the Frimmersdorf power plant could not be estimated with the remote sensing technique (P17-18). Indeed, there are many flight tracks downwind of this power plant that, in principle, could be used for that purpose. I assume that the authors have attempted an inversion, with no success, so that they chose to discard this estimate. Their experience on that particular aspect should be clearly stated to help in the design of future similar campaigns. Perhaps only flight tracks within a few kilometers from the emission can be used ?

The paper is strangely organised. The method for the remote sensing approach is mostly described in the "Result" section. Besides, it is rather strange to have in situ measurements, such as Figure 10, presented in the Remote Sensing section rather than the In situ section. There is a need to show early in the manuscript (section 3) the flight track (both in-situ and remote sensing), similar to Figure 12, as well as the location of the "virtual wall" that was chosen for the mass balance estimate

In the "wall" approach (Figure 4), I could not understand why several cells are considered in the along wind direction. Why not assume that the cell dimensions are zres (vertical) x hres (cross wind) x d (along wind). I could not understand the discussion on page 7 lines 27-30. The dimension of the wall is not provided.

Detailed comments: P2L15: Are you suggesting that thermal infrared observations provide valid concentration estimates in the presence of clouds ? P4L12 : Please provide a valid argumentation why the method used in the manuscript is better than krigging P7L16-22. Not clear why there is a need to have the virtual wall oriented precisely crosswind. It seems more important to have the wall aligned with the flight tracks P8L9: Could not understand P9L2-3 : Could not understand P9L6: What about the sampling of the plume and its variability ? What is the variability of the concentration with the wall cells? P9L15: It is rather difficult to understand that there is an uncertainty about the top of the mixing layer, but not on the flux close to the surface. Either one assumes that there is little vertical mixing, in which case only the flight track at a level close to that of the chimney, or there is mixing that transfers CO_2 both high in the mixing layer and towards the surface (see P9L30). P9L20: Instationarity of the source is mentioned. What is the variability of the source according to the power plant management ? P10L5-10. Although it is not stated clearly, I understand that the discussion is for various days. The paper should rather provide the result for the particular day that is analysed in the manuscript, and make a single sentence for the other days. P10L30 "to the top of the well mixed boundary layer". In situ measurements shown in the manuscript (Figures 4 and 5) clearly show that the boundary layer is not "well mixed' P12L1. Although section 5.2 is supposed to show "results", it actually mostly describes the method. P12L24. Justification not clear. 0.9% relative to what ? P12L26: There is no justification for the removal of data "close to saturation". As long as there is no saturation, these data should have a high SNR. Please justify P14L1. It is said that the elevated XCO_2 are well aligned with the wind field from the power plants, but it seems to me that the high value are further North-East that what would be expected P14L6: It is said that the boundary layer depth is important to compute the wind field. However, the in situ measurements clearly show that the boundary layer is not well mixed. P15L1 : Figure 11 shows that there was a significant decrease of the wind speed during the time of the in situ measurements. This should affect the intensity of the plume and I am surprised this was not discussed in the in-situ section. P17L4. Are there realy any significant difference for the modelled wind speed over the 10 km area ? P17L8. Section 5.2.4 is supposed to be a "Result" section. Yet, a large fraction of it describes the method P17L16: It is said that the measurements are P18L4. Description of the wind speed estimate. It is said that a Gaussian profile for the concentration is assumed. Yet, the in situ measurement do not show such Gaussian profile. It would be nice to compare the assumed vertical distribution of CO_2 with the in situ measure-

ments. Also, the fact that there is little vertical gradient in the wind speed makes this discussion somewhat unnecessary. How is done the weighting to derive a mean wind speed ? P18L18. "Very unstable atmospheric conditions". Is that consistent with the observed meteorological conditions on that day ? P23L22: The authors state the error analysis leads to an uncertainty on the order of 10% for the mass balance approach. This is in contradiction, I believe, with the results shown in Figure 15 that show larger variations for the various leg estimates. For instance, three legs can be used to estimate the emissions from Niederaussem. There is a factor of 2 between the largest and the smallest. This appears contradictory with the error analysis, in particular since several of the error sources are biases and cannot explain a difference between the estimates from two nearby legs. I am surprised this is never discusses in the text. P21L1: The whole section 5.2.5 is poorly written. P24L4: "can differ more than 20% for individual power plants". So what are you saying here ? Are you suggesting that the reported emissions shown in the paper (Figure 18) can be off by that much ?

In the following, I make comments on the figures. I strongly believe that several of them are not useful whereas other could bring additional information Figure 1 : Figure 2 : Limited usefulness Figure 4: Provide colour scale Figure 5 : Is this figure supposed to show the same data as in Figure 4 ? I cannot recognize any feature. I strongly recommend to show the value of the measurements within the circles that are used for the interpolations. What is the link between this figure and the "wall" approach shown in Figure 4 ? Figure 6: Definitely not useful. Not clear what is really shown (ie RMS of what, relative to what ?) Figure 7: Definitely not useful Figure 8: Is it really XCH_4 as indicated in the legend, or XCO_2 ? Why no color scale ? Figure 9: Marginaly usefull. The text could simply say that the in situ measurements (potential temperature and aerosol) provide no useful information to determine the top of the boundary layer up to 1100 m Figure 10 : Should definitely be presented in the "in situ" section, together with Figure 5, and not in the remote sensing section. Figure 11: Difficult to read. Values for the X-axis could be simpler (e.g. 5/10/15) Figure 12: Should be shown early on in manuscript. Why is the color scale not adjusted to the data (no observation before 12)

Figure 13: I suggest to reduce the range of the color bar to 0.99-1.02 and have color lines (Gaussian plume model) for 1.005, 1.01, 1.015 and 1.02 Figure 14: I strongly suggest to add, on each of these graphs, a line showing the result of the modeling according to the Gaussian plume approach. Also, add a horizontal line to show the 0. Figure 15 : State explicitly in the legend that each symbol corresponds to a flux estimate derived from a given aircraft leg. Figure 16 : Not useful Figure 17 : Not useful

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