Atmos. Meas. Tech. Discuss., 3, C285–C290, 2010
www.atmos-meas-tech-discuss.net/3/C285/2010/


Atmospheric Measurement Techniques Discussions



# *Interactive comment on* "A remote sensing technique for global monitoring of power plant $CO_2$ emissions from space and related applications" *by* H. Bovensmann et al.

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Received and published: 28 April 2010

Answers to the comments of anonymous Referee number 2 to Bovensmann et al., "A remote sensing technique for global monitoring of power plant CO2 emissions from space and related applications".

First of all we would like to thank the referee for the detailed, helpful and competent comments. Below we present our response to each of these comments.

Answers to GENERAL COMMENTS

The referee is right that CarbonSat builds on the heritage of OCO and GOSAT. Car-



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bonSat however also (in fact primarily !) builds on the heritage of SCIAMACHY.

We will aim at removing redundant information and information which is not of highest relevance. We will also aim at shortening the paper. To what extent this will be possible will depend on the needed additional page space required for the additional results (e.g., extended error analysis) which need to be included in the revised version of the paper to consider the referee's comments. We will also aim at improving the English language.

Answers to SPECIFIC COMMENTS

1) The referee is right that a wind speed of 1 m/s is typically lower than the average wind speed. We will add more results considering larger wind speeds and will also add wind speed statistics for given power plant locations.

2) According to the referee, the OCO concept is supposed to be better suited to monitor CO2 emissions from power plants. We do not agree with this. The main reason for this is that due to the narrow swath of OCO, OCO will rarely cover the emission plume of a given power plant in the nominal measurement mode over land which is the nadir mode. With OCO target mode power plant coverage can be improved. It is however unlikely that the target mode will be used very often. If OCO would be too often in target mode the main goal of OCO, namely to provide the appropriate measurements for the "regional CO2 surface flux" application, would suffer as this application requires measurements which are not to heavily contaminated by strong local sources. CarbonSat, with its much wider swath will provide measurements which are appropriate for both applications (i.e., for regional fluxes and hot spot emissions) in its nominal measurement mode over land which is the nadir mode (which has a much wider swath and therefore much better coverage compared to OCO).

The coverage of CH4 absorption lines by CarbonSat is an important (additional) reason why we think CarbonSat is better suited for power plant CO2 monitoring compared to OCO as this enables to correct for light path related errors. This is mentioned in the



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paper and clearly acknowledged by the referee. The referee is right that the "CH4 proxy method" is mentioned in the paper but not used for a detailed assessment. We will follow the advice of the referee to quantify at least the achievable precison (which will be somewhat worse compared to the reported precisions as the division by the - noisy - CH4 column will somewhat degrade the XCO2 precision but will significantly reduce scattering related errors (we will demonstrate this in the revised version of the paper)).

There are however also areas where OCO is better. These areas include the smaller pixel size of OCO (possible due to the narrower swath) and the better signal-to-noise ratio (due to binning of detector pixels - also possible due to the narrower swath).

3) It is out of our scope to answer the question why OCO does not consider the 1.65 micron CH4 band. We think that covering also this spectral region significantly enhances the usefulness of the measurements as it permits to generate a very useful XCH4 data product (using the "CO2 proxy approach" as demonstrated by SCIAMACHY) and also helps to reduce scattering related error for CO2 power plant and related applications using the "CH4 proxy approach" (which can be applied if the assumption that the methane variability around a given target is much less compared to the CO2 variability). We have not identified any problem related to this approach such as data rate (which will be essentially the same) or optical imaging (a larger spectral region is not supposed to result in any problems). Therefore there seems to be no problem which needs to be overcome. Based on this we conclude that OCO-2 would highly benefit from also covering the 1.65 micron CH4 band.

4) We are working on separate publications where MAMAP is described and results related to the estimation of the CO2 emission of power plants are discussed. The referee is right that the results presented in our manuscript are too limited to state that we have achieved demonstration of concept. We will modify the corresponding section taking the referee's comments into account. The referee is also right that the three conditions do not directly follow from the aircraft results presented. We will consider



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this comment for the revised version of the paper.

5) The results obtained will depend not only on the instrument performance but also on the inversion algorithm used - at least to some extent. Therefore the paper should contain not only a description of the instrument and its performance but also a sufficiently detailed description of the retrieval algorithm. We think that we have implemented an appropriate approach by putting all details related to the retrieval algorithm in one Annex (the reader can read the Annex but it is not mandatory for the understanding of the paper to read the Annex).

The algorithm used is BESD (i.e., DOAS combined with Optimal Estimation). The version of BESD used for this paper is similar but not idential with the version described and used in Reuter et al., AMT, 2010. The implementation as well as the state vector elements are different from the BESD algorithm described and used in Reuter et al., AMT, 2010. In Reuter et al., for example, methane parameters are not part of the state vector. There are also several other differences. Also the application differs. The algorithm in Reuter et al. is applied to SCIAMACHY measurements which are at much lower spectral resolution compared to CarbonSat. Because of all this we cannot simply refer to Reuter et al. but have to describe the algorithm actually used for this manuscript.

For the revised version of the manuscript we will aim at also quantifying systematic errors - at least to some extent - and/or will present results from sensitivity analysis (aerosols, clouds, wind speed, etc.). We will also add results which apply to the "CH4 proxy method". We will however not restrict the paper to the "CH4 proxy method" but will show results for both methods (for selected cases). We consider it a strength of CarbonSat that several partially independent methods can be used to obtain, for example, power plant CO2 emissions. This enables systematic errors to be better quantified and potential problems of the real measurements to be better identified when applied to real data in the future.



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6) In the revised version of the manuscript additional results related to systematic error sources will be presented and discussed (see above). We will modify the abstract as suggested by the referee.

Answers to MINOR COMMENTS

p.60, l.7: This comment will be considered. References to publications where the algorithms for OCO and GOSAT are presented will be added (unfortunately this will not help to make the paper shorter).

p.70, l.15: In the manuscript the inversion is done without iteration (another difference to the Reuter et al., AMT, 2010, paper), i.e., one iteration step in linear approximation is used. The radiative transfer (RT) simulation are however based on solving the full RT equation (including multiple scattering). As mainly random errors are discussed in the paper this is appropriate. For the revised version we will also aim at quantifying systematic errors. Here non-linearities may contribute to the error. This will be (better) explained in the revised version of the paper.

p.74, l.9: This question refers to Table 5 (and not to Table 6). The referee is right that relative emission uncertainties are given for 25 Mt/year although detailed results for this emission are not given in the manuscript. We will change this in the revised version of the paper. The reason why the absolute emission uncertainty in some cases decreases with increasing emission is due to the fact that the physical dimension of the area (20 km by 20 km as shown in Fig. 1) is kept fixed, i.e., only a part of the plume is used for the analysis. For a power plant with a larger emission the fraction of the plume within the analyzed region is larger and therefore the inferred uncertainty is smaller. We will mention this in the revised version of the manuscript and probably also redo the analysis using a larger area.

Answers to TECHNICAL COMMENTS

Typos will be corrected and the English improved.



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Fig. 4 (orbital coverage) and Fig. 6 (simulated spectra) are not redundant but Fig. 3 (spectra) and Fig. 6 (spectra) are to some extent. We will think about removing Fig. 3.

Fig. 5 (spectral albedos): We will think about removing Fig. 5.

Fig. 7 (Jacobians). We think that this Figure is important as it nicely illustrates the BESD algorithm and clearly shows which spectral region provides information on which parameter. For this purpose it is not absolutely mandatory to see every detail (although one can zoom in using the electronic version of the paper) but we will aim at enlarging this figure to consider the referee's comment that is is too small (if this is not possible we will consider to remove this figure or move it to the Annex).

Fig. 9: We will improve this Figure taking the referee comments into account.

Table 4: As the results depend to some extent on the details of the algorithm and its parameters we think that this information should be given. We will consider to move it to the Annex.

Section 4 and table 2: We will improve the notation taking the referee's comments into account.

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Interactive comment on Atmos. Meas. Tech. Discuss., 3, 55, 2010.