Atmos. Meas. Tech. Discuss., 3, C183–C188, 2010
www.atmos-meas-tech-discuss.net/3/C183/2010/

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.

Anonymous Referee #2

Received and published: 13 April 2010

GENERAL COMMENTS

The paper under review proposes a new space-based instrument that is able to monitor atmospheric total column concentrations of CO_2 and CH_4. The key feature put forward by the authors is the capability to estimate anthropogenic CO_2 emissions by coal fired power plants. Thereby, the proposed satellite mission CarbonSat builds on the heritage of the Orbiting Carbon Observatory (OCO) and the Greenhouse gases Observing SATellite (GOSAT) but CarbonSat provides better geospatial coverage than both of these missions, smaller ground-pixel size than GOSAT, and compared to OCO the capability to simultaneously measure CO_2 and CH_4.

The proposed mission is a straight-forward advancement of the existing satellite missions. In comparison to OCO, I consider an excellent idea to trade better spectral coverage versus coarser spectral resolution in the 1.6 micron band. This modification allows for simultaneously measuring CH_4 (in addition to CO_2) - apparently without significant drawbacks. While I am convinced that the proposed instrument will be a state-of-the-art tool to provide constraints for carbon cycle modeling in general, the study cannot convince me of CarbonSat being able to monitor power plant emissions. The study is suitable for publication in AMT after taking my concerns and comments into account.

My concerns are largely due to the paper lacking clearness and thoroughness. I strongly agree with reviewer 1 who suggests to reduce the number of words by 1/3. The manuscript includes a potpourri of irrelevant and redundant information which drowns the actually important aspects. Discussion an important issues is missing. I will try to go into detail below as far as I am able to follow the rationales. I generally urge the authors to revise the text with a clear focus in mind. I also recommend proof-reading wrt. the English language.

SPECIFIC COMMENTS

1) A large part of the study is based on assumed windspeed 1 m/s. Occasionally, windspeeds up to 4 m/s are considered. Even for a cloudless atmosphere, I would consider 1 m/s wind speed a vary benign (and rarely occurring) scenario for monitoring of CO_2 point sources from space since dispersion of the plume is slow. Probably, 4 m/s is not even the worst case.

Section 2 (p.62, l.1), for example, describes the simulation of a power plant plume and tries to deduce a maximum ground pixel size of 2×2 km^2. The assumed wind speed

1 m/s, one of the most important variables, is only mentioned in the caption of table 1. The paper requires a statistical analysis and a honest discussion of actually occurring wind speeds at the relevant locations.

2) In comparison to OCO, the proposed instrument has larger ground-pixel size and worse or equal signal-to-noise (p.69, l.14). Further, OCO has a small swath (comparable to the dimensions of Fig.1) and features a target mode which could be used to stare at power plants. GOSAT also has a target mode but substantially larger ground-pixels than the proposed CarbonSat. I conclude that at least the OCO concept could actually be better suited than CarbonSat for monitoring CO_2 emissions from power plants.

In my opinion, the one feature that makes CarbonSat better suited for point source monitoring than OCO is its CH_4 measurement capability providing the option to use CH_4 as a lightpath proxy as suggested by the authors. Unfortunately, the paper does not attempt to discuss the accuracy of the lightpath proxy approach although it is prominently advertised. If I understand correctly, the paper does not even use this proxy method for assessing the CO_2 measurement precision (one would need to add the CH_4 and CO_2 noise errors). I recommend to make use of the lightpath proxy method when investigating the achievable precision.

3) Related to comment 2, one might also wonder why OCO did not consider to cover the 1.65 micron CH_4 band and to exploit the imaging capabilities of the OCO instrument. What are the potential problems (data rate, optical imaging) and, most importantly, how are they overcome by CarbonSat?

4) Despite its title ("Simulation of power plant CO_2 emission plumes"), section 2 (p.62, l.1) mostly features the description of an airborne demonstration model for CarbonSat. Such an airborne demonstrator could make a very strong point if discussed

properly. However, section 2 is only a description of the airborne instrument and its first deployments. A quantitative validation of the retrieved CO_2 and CH_4 concentrations and the deduced CO_2 emissions is lacking. In my opinion, this is not a demonstration of the concept but rather serves as an advertisement of the airborne instrument. I suggest to either perform a quantitative validation exercise or to remove the discussion on the airborne demonstrator from the manuscript.

Further, I cannot follow how the conditions i) through iii) (p.64, l.8) are deduced from the aircraft measurements given that there is no quantitative validation undertaken.

5) If I understand correctly, the BESD algorithm is used to theoretically assess the CO_2 measurement precision. This algorithm has been extensively described by Reuter et al., AMT, 2009. Optimal estimation theory has been discussed in various manuscripts. Thus, a large part of section 5 and the whole appendix B can be removed from the manuscript.

Why is BESD used at all? The peculiarity of BESD is its capability to retrieve atmospheric scattering properties and thus, to accurately model the lightpath. The manuscript, however, highlights that lightpath issues are dealt with by using CH_4 as a lightpath proxy.

As far as I can trace the manuscript, the CO_2 uncertainty estimate (p.73, l.15) is found by applying the BESD algorithm to a simulation that is calculated by the BESD forward model, i.e. retrieval and simulation algorithms are consistent and thus, the CO_2 uncertainty estimate does not include forward model errors. I believe that the latter are actually the dominating error contribution (see also comment 2) by reviewer 1). The manuscript must make clear at a prominent place (abstract, conclusion) that only precision, not accuracy is investigated.

In the view of a more consistent manuscript, one could consider to remove the BESD algorithm from the study and to base the discussion on the lightpath proxy method (see also comment 2) above).

6) Summarizing my above concerns (benign wind speeds, lightpath proxy method requires consideration of CH_4 and CO_2 noise error, no systematic errors considered), I conclude that the estimated CO_2 emission uncertainty of 0.5 - 5 Mt/year achievable by CarbonSat is overly optimistic. The manuscript contains several hints to the aforementioned error sources but none of them is investigated or makes it to the conclusions or to the abstract.

MINOR COMMENTS

p.60, l.7: While the introduction features a lengthy discussion on SCIAMACHY and thermal infrared sounders, it fails to even mention algorithm developments dedicated to OCO and GOSAT which are of immediate relevance to CarbonSat eg. Connor et al., JGR, 2008, Oshchepkov et al., JGR, 2008, Bril et al., Appl. Opt., 2009, Butz et al., Appl. Opt., 2009, Kuze et al., Appl. Opt., 2010.

p.70, l.15: "the radiative transfer (RT) is sufficiently linear". Do you mean that the inverse problem can be solved iteratively in linear approximation? Or do you really mean that one iteration step in linear approximation is sufficient? I would doubt the latter unless the initial guess is close to the true state. Please clarify the manuscript.

p.74, l.9: Emission uncertainty is estimated for 6.5 and 13 Mt/year power plant emission (table 6). Relative uncertainties are given wrt. 25 Mt/year although this calculation is not carried out. This seems daring. Is there a good reason why in some cases the absolute (!) emission uncertainty decreases with increasing emissions in

table 6.

TECHNICAL COMMENTS

I will not list typos and all instances where I have doubts about use of the English language. I trust in the authors to thoroughly proof-read the manuscript. Please consider the following in particular.

Fig. 4 and Fig. 6 are somewhat redundant.

Fig. 5 is not urgently required.

Fig. 7 is not required and much too small.

Fig. 9 upper panels are redundant to Fig. 4 and Fig. 6. Fig. 9 middle panels are much too small. Fig. 9 lower panels are too small.

Table 4 is not urgently required.

Section 4 and table 2: Throughput "τ" on p.67, l.13 vs. "T" in table 2. S in electrons on p. 68, l.5/l.8 vs. S in Coulomb in equation 4/5.

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