Dear Editor:

We thank our reviewers for doing an excellent job to make our paper better. We accept most of the comments, and will introduce corresponding changes to the text. The answers to the particular comments are typed in different color immediately after the comment (as written by the reviewers). The necessary changes and corrections will be added to the revised manuscript.

Regards

Igor Polonsky

Anonymous Referee #2

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Manuscript "Performance of a geostationary mission, geoCARB, to measure CO2, CH4 and CO column-averaged concentrations", submitted by Polonsky et al. for publication in Atmos. Meas. Tech. (AMT) contains new interesting scientific material relevant for AMT and is well written. Referee number 1 gives a nice summary of the paper, which is not repeated here. I recommend publication in AMT after the mostly minor comments listed below have been considered by the authors. General: I have the same question / concern as Referee number 1: Where is the XCO2 accuracy requirement of 0.7% (2.7 ppm) coming from and is this good enough to address the targeted application areas ?

> The accuracy requirement comes from analyzing the geoCARB instrument and comparing its characteristics, such as signal-to-noise ratio and spectral resolution, against OCO and GOSAT instruments. The real-world performance of GOSAT was very important in setting the accuracy requirement. It showed that "algorithm noise", arising principally from poor representation of aerosols, sets a base level for the error. Error predictions from simulations alone tend to underestimate the base level.

Abstract, page 9398, lines 12-13: Please correct "could provide opens the possibility" by deleting the first 2 or the last 3 words.

> Sure.

Introduction:

Page 9400, lines 3-4: Please add a reference / justification for the statement "Nevertheless both are manageable using data from a star tracker and landmarks on the surface of the earth".

> The geoCARB mission requires accurate knowledge of pointing rather than accurate capability of pointing. A star tracker will provide accurate knowledge. In addition, the use of landmarks is a fairly straightforward technique, based on known locations of places where the surface reflectivity experiences an abrupt change (such as sea shore lines). This technique allows one to find and correct problems associated with pointing

Page 9400, line 17: “The study is guided by the excellent work that has been done for :”. It is understood that the authors like papers which they co-authored but please use a more neutral statement here.

This line was written before C. O'Dell was our co-author. It will be corrected.

Section 3.2:

Page 9406, line following: “No attempt was made to impose spatial correlation on the aerosol and cloud fields, because it was thought that maximizing the variability of cloud and aerosol, while still maintaining a link to reality through CALIPSO, would make the tests of the retrieval algorithm more stringent.” I wonder if this is really true. If I understand correctly, spatial correlations are neglected, which means that cloud and aerosol related errors are essentially treated as random errors rather than spatially coherent systematic errors. As systematic errors are likely more critical than random errors, it is not clear for me why the selected approach is more stringent. Please clarify. It is also not clear for me how “Such correlations should assist in correcting for aerosol and cloud when estimating the trace gas concentrations.”. How to distinguish, e.g., erroneous regional CO2 enhancements from a local source?

This is a fair comment, to which there is no easy answer. Indeed the effect of spatially correlated aerosol/cloud upon predictions of sources and sinks of CO2 and CH4 is still an open question. Nevertheless, it is true that “[n]o attempt was made to impose spatial correlations ... because it was thought ...”. Perhaps we were wrong, but intuitively we thought that permitting full variability of aerosol/cloud would revel the full range of errors in retrieved XCO2 and XCH4. That is all the text claimed.

The reason we thought that “[s]uch correlations should assist in correcting for aerosol and cloud when estimating trace gas concentrations also is intuitive, rather than based on hard evidence. We suspect that imposing a spatial correlation on aerosol might assist if CO2 is retrieved over larger regions, rather than column-by-column, as in the present plan for OCO. For example, over a region larger than a single OCO footprint but small enough that the CO2 variation is small, imposing a spatial correlation on aerosol/cloud would reduce the number of degrees of freedom for aerosol, and therefore might improve the CO2 estimate for the region.

As a compromise, in revising the text we will soften the statements so that it is clearer that they are speculative. For example, for “would make the tests of the retrieval algorithm more stringent” we suggest “should reveal the range of errors that aerosol and clouds may cause”. Similarly, for “Such correlations should assist ...” we suggest “Such correlations might assist in correcting for aerosol and cloud if trace gas concentrations are estimated over regions smaller than the correlation lengths of the trace gases but larger than single satellite footprints”.

In connection with estimating the strengths of emissions from power plants, we understand that spatially correlated aerosol causes systematic errors (biases) in the XCO2 estimations, whilst uncorrelated aerosol increase the standard deviation. Later in the manuscript we show that a constant overall XCO2bias has a smaller effect on the estimated source strength than random errors.

Section 4.1:

Page 9410, lines 20-22: Concerning the two types of aerosols: To avoid urging the reader to read the other publications: Please add details on how this is achieved / dealt with in the retrieval algorithm and add a short description of the two aerosol types 2b and 3b.

> I am not sure what the reviewer would like us to say here. We use a fairly standard retrieval algorithm, described many times recently. Our choice of the cloud/aerosol models is quite arbitrary; it should include basic cloud types (water and ice) and a few background aerosol types to model the effect of aerosol. Because real aerosol is highly variable, it is understood that this modeling is very approximate, and does not result in retrieval of real aerosol properties. We will add brief descriptions of Kahn's type 2b and type 3a aerosol.

Section 5.1.2:

Page 9416, line 12 (item 3): In contrast to the earlier used scaling factors, esp. the use of a scaling factor of 1000, why is 100 the largest scaling factor used here?

> In the experiments (as pointed out earlier in the text (page 9414) scaling factors up to 10^8 were used. After the scaling factor reached about 100, the differences were insignificant, so the corresponding results were omitted.

Section 5.1.3:

Page 9417, line 11 following: An overall bias can have many causes, incl. the selected a priori information, and may not be critical for inverse modelling of surface fluxes (if it is constant). Therefore it is not clear why "On the negative side, it causes the bias in retrieved XCO2 to shift" is a major issue. It would only be an issue if the bias is not constant.

> We made a limited study of the effect of eliminating the strong CO2 band on the retrieval, and found that generally it resulted in a bias. However, the bias might not be constant, and might depend on other factors (presently unknown). Therefore, here we just describe our findings, saying that further research is required to understand this particular effect.

Sections 5.2-5.4:

Please add a table to provide an overview about the results obtained for the different experiments.

> We do not believe that a table is necessary.

Section 5.5:

Page 9422, line 11 following (and corresponding assessments later in the paper): Analysis approach: ": : : and illustrates the advantage of fine temporal sampling with subsequent averaging of multiple snap-shots". I don't think that, except under very rare circumstances, individual snap-shots can/should be averaged and successfully analyzed in terms of emissions. A changing meteorology (wind) will change the (direction of the) emission plume (e.g., within one hour)! How to deal with this?

Here we assumed that all snapshots are processed independently, and the results of the processing are being averaged. We agree that averaging the snapshots before analysis would be inadvisable.

Section 5.5.1:

Page 9422, bottom: Please mention that more details on s(x) will be given in Sect. 5.5.2.

A particular form used in our simulation is discussed on page 9425.

Page 9424, line 10 following: Not only number of snap-shots matter but also SNR etc. Furthermore, I don't think that adding of snap-shots is possible (see above). Therefore, it is not clear that GEO is better than LEO under all circumstances as suggested by the text. Instead, there are pros and cons for each observation mode. I recommend modifying the text to reflect his.

As explained above, we propose to average the results from snap-shots after each has been processed independently. The only assumption required is that the emission strength is constant over the period of observation, and that is quite reasonable if the period is short, such as one hour. We do not intend to average spectra over the observation period, and the reviewer's comment appears to be based on a misunderstanding of our intent. A LEO satellite does have numerous advantages, including for example global coverage, but it is undeniable that the ability of a GEO to observe a target area frequently is an advantage. In this paper we have explored that advantage.

Section 5.5.2:

Page 9425, line 9: Where are the listed four parameters coming from? Do the results critically depend on them? If yes, how to deal with this (e.g., where to get these parameters from)?

We conducted an idealised experiment to support our analytic results. We selected a published plume model that has been used by other teams of scientists. With real observations to evaluate an emission source strength, we plan to use a more realistic model that includes real time meteorology to track the plume, such as the Weather Research & Forecasting model (http://www.wrf-model.org), perhaps coupled with a sub-grid-scale model like CalPuff.

Page 9425, line 15: It is a bit strange that the water cloud is considered to be part of the aerosol.

Both clouds and aerosols affect the light propagation in similar manner through scattering and absorption. In this context they are the same. Furthermore, most of the literature for OCO uses "aerosol" in a generic sense, referring to any scattering material in the atmosphere, and therefore comprising both clouds and the smoggy stuff.

Page 9425, line 17-18: The assumption of a scene independent (i.e., constant) albedo appears quite unrealistic.

This idealised model was selected to demonstrate the effect of wind and aerosol/cloud contamination on the emission source estimation. Thus the effect of albedo variability was excluded. For real data, methods that admit albedo variations will be developed.

Page 9426, line 3: Where is the value of 0.15 coming from?

> Dirty power plans can emit aerosol an addition to the CO2. That is why we consider situations when the plume contains not only CO2 but a soot aerosol component.

Page 9426, line 27: “the the”.

> It will be fixed.

Section 5.6:

Page 9428, line 11 following: Vertical profiles: It seems that the trace gas profiles in the emission plume are not enhanced in the boundary layer as it should be as the source is at the surface and the observations are made near the source. Please clarify.

> The emission plume was enhanced at the boundary layer.

Page 9428, line 22: “AOD exceeds the threshold of 0.1 set in the PPF”: Is this the AOD threshold? Larger values are mentioned earlier in the paper!? Using AOD for PPF requires that the retrieved AOD is sufficiently accurate. Is this the case? Please clarify.

> There are many applications for the XCO2 data. In applications where the bias in XCO2 is critical, then the retrieved AOD generally must be included in the PPF. However, if the overall bias may be neglected, as seems to be the case when estimating power plant emissions, then the AOD requirement can be loosened or even omitted.

Page 9429, line 13 following: How large would be the emission errors for the mentioned XCO2 biases of 1.1 and 1.3 ppm? Is this really within the “acceptable range” as stated in line 16.

> Our experiments show that the overall bias results in small errors in emission source estimation.

Section 6:

Page 9430, line 18: The estimated emission accuracy of 3% (which I consider very optimistic) is only valid for the assumptions made, e.g., no modelling related errors, perfect knowledge of the meteorology, averaging of individual snap-shots possible (i.e., very stable conditions), etc. This needs to be clearly mentioned here. Same for “The power plant results are robust” (line 27).

> We understand that the performance estimated for geoCARB based on our simulations will be optimistic,because multiple factors have been neglected, such as modeling related errors, inaccurate spectroscopy and errors in the instrument characterizations. We specifically make clear that only under special observation conditions will such accuracy be achieved.

Page 9431, lines 7-8: “mission requirements”: No clear what is meant here: XCO2 error or CO2 emission error requirement. If the latter, what is the emission error requirement?

We meant the emission rate error requirement.

Table 6: Replace (2x) hPa by ppm for XCO2.

Thanks, it will be fixed.

Figure 7: Add unit for x axis.

Thanks, it will be fixed.

Figures 11 and 12: Please use the same x axis for all panels where possible

Thanks, it will be fixed.